PROCEEDINGS

OF THE

NATIONAL ACADEMY OF SCIENCES

OF THE UNITED STATES OF AMERICA

VOLUME 3, 1917

Publication Office: Williams & Wilkins Company, Baltimore
Editorial Office: Massachusetts Institute of Technology, Cambridge

Home Office of the Academy: Washington, D. C.

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VOLUME 3 JANUARY, 1917 NUMBER 1

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Publication Office: Williams & Wilkins Company, Baltimore
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PROCEEDINGS
OF THE
NATIONAL ACADEMY OF SCIENCES

Volume 3 JANUARY 15, 1917 Number 1

INFERENCES CONCERNING AURORAS

By Elihu Thomson

GENERAL ELECTRIC COMPANY, WEST LYNN, MASSACHUSETTS

Read before the Academy November 14, 1916

It seems doubtful if any extensive auroral display has occurred without coincident existence of exceptional areas of disturbance on the sun. We may assume that at such times great jets or streams of electrified matter (electrons perhaps) akin to cathode rays in a vacuum, are projected with high velocities outward, and that occasionally some of these jets cross the earth's orbit or pass near to it, being when leaving the sun in a general radial direction, bent backwards for obvious reasons.

That electrified matter in a vacuum does move in jets or streams for indefinite distances is a fundamental fact. Moving charges of the same name in paths, straight or curved, act like parallel currents and attract one another, the more as their velocity is greater, until the static repulsion of such like charges sets a limit to further approach. It is possible that the space around the sun may be crossed by many such jets or streams of electrified matter moving at very high velocities into the vacuous space. The coronal streamers may be the visible composite effects of the projected jets. Such electrified jets may act inductively by proximity to the earth or directly by conduction of electricity to the earth's outer atmosphere.

In the present paper, however, it is hoped to prepare the way for further study by pointing out certain physical facts regarding the relations of auroral phenomena to the earth and its atmosphere: to locate and give direction to the streamers seen in auroras; and to explain the nature of the so called auroral arch, the zenith crown, and other characteristics. It is believed that the following propositions may be shown to be true.

1. Streamers seen in auroras, singly or in composite masses, are in reality vertical, or approximately so, to the earth's surface, nearly parallel when adjacent, and only slightly divergent even when miles apart; the divergence being due to curvature of the earth's surface.

2. In any aurora, the streamers appear to be located in bands or zones more or less wide in latitude extending generally in east and west direction, or forming belts or zones between parallels of latitude in which the streamers extend vertically upward like trees in a forest.

3. In some rare auroras the vertical streamers are closely limited to a narrow belt of latitude; sometimes only 2 or 3° or even less, in width north and south, while the east and west extent of the narrow belt may be very great.

4. In wide spread auroras the belt of vertical streamers may cover great extents of latitude and extend east and west unlimited distances. This appears to have been the case in the recent great aurora of August 26, 1916.

5. The curvature of the so called auroral arch is an optical effect of perspective, slightly added to by the curvature of the earth. The appearance of folded curtains of streamers is a similar effect of superposition and perspective when the active band or zone covers many degrees in latitudes. It is probable that the lower ends of auroral streamers have about the same height in the earth's atmosphere; a layer from which they stream upward to heights which vary in different displays or even in the same display. This layer probably exists at a height of about fifty miles and conducts laterally or horizontally, thus distributing the electricity discharged from it into the streamers.

6. The convergence of long streamers toward the zenith seen in the greater auroras, is purely an optical effect of perspective, the streamers being vertical.

7. The so called zenith crown is in reality due to bundles of streamers, nearly vertical but seen on end. They appear as patches of shifting light in or near the zenith, sometimes surrounded by apparently converging streamers from the north, east, and west, and even from the south; converging in appearance only.

8. The convergence of streamers is of the same nature as the convergence of straight parallel railway tracks in the distance, or better, the apparent convergence toward the sun, of the sunbeams seen in dust-laden air, when the sun itself is obscured by a small irregular cloud, or is back of a broken mass of clouds.

In April, 1883, at near the middle of sunspot period and coincidently with the occurrence of an enormous sunspot, there was perhaps the

greatest auroral display seen in our latitudes for more than half a century. Telegraph lines from east to west could not be operated owing to arcing at the keys. The display as witnessed by me, was characterized by colored streamers passing upward from all around towards the zenith from north, east, west and south. The coloring gave me the first clue to the true relations existing. In the north the sharply defined streamers beginning their appearance low in altitude with the usual greenish auroral light, shot, or spread upward with changes of tint, finishing at their upper ends in a deep crimson. The light patches of the zenith crown went through the *same succession* of coloring constantly, each time ending in deep crimson. This fact suggested in the strongest way that in the crown one was observing bundles of streamers on end, but of the same character as all the other streamers in the field of view. Great masses or broad bands to the east and west though steady were colored likewise. These dense masses in the east and west were unquestionably due to the composite effect of great numbers of streamers superposed in the line of sight through great distances to east and west; the colors less pure blurred by distance and failure of exact superposition. Other observers at places far to east and west of my position might observe zenith crowns but composed of other streamers seen on end. A necessary consequence of the display presenting the same appearance to observers in many places far apart, up to hundreds of miles east and west, and through a zone north and south, is that the streamers everywhere were in reality vertical, or approximately so, to the earth's surface. In Chicago, a distance west of about eight hundred miles, this aurora was described in the same way, the appearances coinciding with my observations. The effects being so similar in places far apart, the only inference possible is the one stated, namely, nearly vertical streamers wherever seen.

In this view observations upon subsequent auroras could be made more intelligently than before. But auroral displays of great magnitude are rare in our latitudes, especially such as are so far south as to cause a belt of streamers directly over the place of observation giving the zenith crown. While several highly instructive displays occurred meanwhile, one on September 29, 1908, gave streamers converging from all directions joining the crown in the zenith which had the usual characteristics. My note of it says "The rapid procession of streamers and the waving curtains make this display quite exceptional; though far inferior to that of April, 1883." On August 26, of this year (1916) there occurred a display second only in my experience to that of April, 1883, but without the remarkable coloring of the display of that year.

It was observed by me in the Adirondacks, and served to confirm the inferences drawn from the earlier displays.

But on August 28 there occurred a simple type of aurora which I had only seen on two former occasions. It may be said to contain a single element only. It consisted of a single band a belt of light, estimated at from $1\frac{1}{2}°$ to $2°$ wide extending across the sky due east and west, from horizon to horizon and passing through the zenith. It lasted for an hour or a little more. At the zenith and for 2 or 3° east and west thereof, the band was broken into patches which shifted, faded and were replaced by others of different shapes and positions. A friend observed the same appearance in Maine hundreds of miles to the east on the same night.

The explanation of this simple type of aurora is not difficult when made in accordance with the ideas here presented. No streamers were seen as such. At the zenith they were directly over the observer, and though present were seen only on end as small patches of light constantly changing: while in the extensions or narrow bands to east and west from the zenith the luminous streak was a composite of super-posed streamers in the same latitude as the observer, which streamers overlapped each other all the way down to the apparent horizon along the overhead arch east and west. They were like the palings of an extended fence located far above the observer. He would see the separate palings, directly above him but only on end. Otherwise they overlap and obscure each other or one another. To appreciate fully the reality, the observer is better placed if the auroral arch when, as in this case, very narrow is not directly overhead. The usual auroral arch is the location of the lower ends of the vertical streamers extending upward from it. This location would appear from the work of Störmer and others to be in a layer of atmosphere about fifty miles high. This layer on which the feet of the streamers may be said to rest is probably a conducting layer like the partial vacuum in a Geissler tube.

The so called auroral arch appears as an arch or curve merely because of perspective vision, just as a searchlight beam or a long straight cloud appears bent when crossing the sky, or as a cloud layer, known to be horizontal appears above us as a great inverted bowl.

The streamers in the aurora of August 26, were, as in the aurora of 1883, directed towards the zenith crown from all sides. The appearance of the whole aurora when at its height, and looking upwards towards the zenith was as if one were looking into a truncated hollow cone from below its base, with the slant sides composed of ribs of light; these ribs as well as the blunt apex, constantly changing, shifting, fading, and

returning and traversed by waves of light of varying intensity. These appearances were not, of course, peculiar to the particular post of observation but were observed from points far east and west without substantial differences. A vivid description by Dr. C. C. Nutting is found in *Science* for October 6, 1916, pp. 496 and 497. See also *Science* of November 10 to November 17 and December 8, 1916, for other letters concerning this great aurora.

The belt covered by this display was evidently of very great extent east and west and spread far to the north in latitude. One is compelled to recognize that observers far apart seeing the same appearances are looking up between nearly parallel and vertical streamers, seeing those on end as a zenith crown when directly above them; while laterally they are superposed by being back of one another at varying distances. The same auroral appearances are possible to be seen alike at different places simultaneously, *only* when a system of *vertical streamers exists.*

Let us for illustration assume an extended horizontal flat surface and that there be erected above it a set of vertical and very long rods in a vertical plane extending east and west, the rods being spaced apart like the paling in a long straight picket fence. An observer on the plane faces north looking towards the rods or paling the lower ends of which are high above his position, appearing say at an altitude of 60° in the north direction. The lower ends will now appear to lie east and west in an arch of curve convex upward owing to diminished angle of vision with distance and the rods or palings will appear to converge upward if long enough almost to the observer's zenith, much foreshortened; the whole effect being that of perspective. Removing the point of observation further to the south, the middle point of the arch drops more and more, the arch becomes flatter, and the vertical rods appears less foreshortened and longer, while still converging towards the observer's zenith. These varied appearances, modified to a minor degree by the earth's curvature, are just what are seen in auroras. If the vertical rods are spaced apart irregularly, increased in number and spread into a band so that they do not lie exactly in a vertical plane east and west, but in an arrangement like a long strip of forest extending east and west and of considerable width north and south (an arrangement corresponding to a belt or zone of streamers instead of a single line), the analogy to the auroral arrangement at any instant is much closer.

It can readily be seen that the recognition of the vertical relation of streamers to the earth's surface and the nearly constant level of their lower ends simplifies to a great extent the study of auroras, particularly

the determination of the total height reached by them, curvature of the earth being allowed for. The effect of this curvature will be less the higher in altitude the auroral arch, or the nearer it is to being overhead. The streamers are often observed to rise from the arch first as short streamers, gradually developing and extending upward until their upper ends are a few degrees from the zenith. When they originate in an arch which is of low altitude and extend nearly to the zenith, as they appear to do in the greater displays, their length must be hundreds of miles, possibly in some instances reaching one thousand or more. It would appear that no limit can be set for their possible height. In most auroras, however, the visible extent of the streamers is more limited. A low altitude auroral arch implies great distance north from the observer and for a given actual streamer length a less apparent height or length. Paulsen's class of auroras without streamers may mean either that the streamers are too short and too many to be noted separately, or that the electrification is too feeble for their development, the observed luminous glow being due to flow of current in the conducting layer itself, an arch forming horizontally, but without outward projection.

If our assumptions are approximately correct the arch of an aurora, if located farther north than about 600 miles from an observer will be below his horizon but the streamers extending upward from it if long enough may be seen. Auroras far north of this will probably be invisible or be seen merely as a luminous glow well down on the northern horizon. When the breadth in latitude of the auroral zone is great and the display is seen from the south, the streamers may overlap or be arranged in apparent curtains or folds, the lower ends of the streamers being in that case at varying apparent altitudes above the northern horizon even when in the same general line of view. In such case they may be superposed in the line of sight and therefore be composite, or increased in apparent length owing to imperfect superposition in their lengths with respect to those back or front of them. It is believed that these and like considerations will suffice for the explanation of observed appearances of auroras in spite of their great variety.

The direction of streamers, as indicated, being vertical to the earth's surface, is suggestive of electrical discharges, ions or electrons projected outward into space from the conducting layer of our atmosphere; discharge into space in which the mean free path is unlimited. These discharges might have their origin in a charged conducting layer in which the potential suddenly rose to a critical value, or might be brought about inductively by the presence in the space around the earth of

opposite charges possibly arriving in great jets from the sun. Further the charges might be communicated to our air from such jets followed by discharge into space beyond.

A sudden uplift of a highly charged thin air layer itself in waves or ridges might easily disturb the normal potential distribution and precipitate discharges from the crests. Atmospheric currents or displacements may cause such uplifts as when a colder stream strikes down under a warmer charged layer.

A set of vertical streamers would deflect a compass needle on the earth's surface one way or the other depending on whether such streamers exist to the north or to the south of the position of the compass. Observations seem to confirm this but more work is needed. The direction of the compass deflection would determine the direction of the virtual streamer currents to which the deflection was due. Aside from these and other considerations, the effort has, however, been to present in this communication a rational theory which will at least enable a proper conception of the actual space relations of the visible portions of an aurora in relation to the earth's surface to be obtained; and to place on record ideas which through many years of consideration by the author have seemingly received at each appearance of an aurora with streamers, repeated confirmation.

While I have in former papers, as in "Thoughts on Osmical Electricity," an address before the Franklin Institute of Pennsylvania (December 19, 1893) and notably in an address on "Atmospheric Electricity" delivered at Princeton University October 21, 1909, and published in *Science*, December 17, 1909, pp. 857 to 869, given a brief sketch of some of the views presented, particularly the outward direction of the streamers, later observations have served to provide cumulative evidence and extend their application.

APPLICATION OF THE LAWS OF ACTION, REACTION AND INTERACTION IN LIFE EVOLUTION

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Read before the Academy, November 13, 1916

Since 1893 I have been working upon the interrelations of the various so-called factors of evolution and have published a series of studies of this subject. In 1908 I presented before the American Society of Naturalists an exposition of a law which I termed "the law of the four inseparable factors of evolution," these factors being regarded as the

sum of the forces inherent in environment, in individual development or ontogeny, in race development or heredity, and in natural selection. This conception was worked out more fully in 1912 and published in the form of a preliminary statement, as "Tetraplasy, the Law of the Four Inseparable Factors of Evolution." During the past two years I have been engaged in working out the aspects of this law from the standpoint of physics[1] and chemistry, that is, interchange of energy, in preparation for the Hale Lectures before the National Academy of Sciences on "The Origin and Evolution of Life upon the Earth." I perceive that it was an error to regard Selection as one of the four inseparable factors because it is not a form of energy. Consequently the law should be restated in the following terms:

In each organism the phenomena of life represent the action, reaction, and interaction of four complexes of physico-chemical energy, namely, those of (1) the inorganic environment, (2) the developing individual (cytoplasm and somatic chromatin), (3) the germinal or heredity chromatin, (4) the organic environment. Upon the resultant actions, reactions, and interactions of each organism Selection is constantly operating whenever there is competition with corresponding actions, reactions, and interactions in other organisms.

I believe this to be the most fundamental biologic law which can be expressed from our existing knowledge. It is in part an application to life phenomena, first, of Newton's third law of motion,[2] in the light of which physicists have given the full dynamical meaning to the modern laws of thermodynamics, second, of the laws of thermodynamics, and, third, of Darwin's law of Selection as developed by Weismann, Roux, Osborn, and others in modern biology. The reign of the laws of motion, including the motion of electricity, and of thermodynamics in the life processes follows as a necessary consequence of our modern physio-chemical interpretation of many of the phenomena which were formerly regarded as vitalistic.

This law as operating between two or more organisms may be clearly expressed in the following scheme.

Organism A		*Organisms B—X*
Physico-chemical Actions, Reactions, and Interactions of the	Natural Selection	Physico-Chemical Actions, Reactions and Interactions of the
Lifeless Environment	(Competition, Survival, Elimination,	Lifeless Environment
Developing Organism	Cessation of Selection,	Developing Organism
Cytoplasm and Chromatin	Reversed Selection,	Cytoplasm and Chromatin
Heredity Chromatin	Organic Selection, etc.)	Heredity Chromatin
Life Environment		Life Environment

Physico-chemical actions and reactions, which, so far as known, follow the laws of conservation of energy, are the chief phenomena observed in modern physiology, as set forth in such works as Loeb's "Dynamics of Living Matter." Through catalysis many of the actions and reactions are known to send off chemical messengers which are among the chief means of interaction in different parts of the organism. There are in animals other means of interaction, such as the enzymes, the secretions of the duct glands, the internal secretions of the ductless glands, and the nervous system. These interactions do not follow the laws of conservation of energy; they work at a distance, and the effects do not balance the causes. Certain phenomena of interaction, in which chemical messengers of various kinds coördinate the actions and reactions of the organism, are now understood both in physiology and pathology as coördinating, correlating, accelerating (hormones), and retarding (chalones), as well as balancing growth and development.

The nature of the actions, reactions, and interactions between the physical environment, the development organism, and the life environment are relatively well understood. It remains to be determined what relations these actions, reactions, and interactions respectively have to the physico-chemical processes in the somatic and in the germinal chromatin. There are some grounds for the hypothesis of Cunningham that some of these chemical messengers affect in a similar manner the germinal and bodily chromatin, but this subject is very obscure at present.

[1] I am indebted to my colleague M. I. Pupin for valuable suggestions in formulating the physical aspect of these principles. He regards Newton's third law as the foundation not only of modern dynamics in the Newtonian sense but in the most general sense, including biological phenomena. The second law of thermodynamics started from a new principle, that of Carnot, which apparently had no direct connection with Newton's third law of motion. This second law, however, in its most general form cannot be fully interpreted except by statistical dynamics, which is a modern offshoot of Newtonian dynamics. With regard to the first law of thermodynamics, it is a particular form of the principle of conservation of energy as applied to heat energy. Helmholtz, who first stated the principle of conservation of energy, derived it from Newtonian dynamics.

[2] "I. Every body perseveres in its state of rest, or of uniform motion in a right line, unless it is compelled to change that state by forces impressed thereon."

"II. The alteration of motion is ever proportional to the motive force impressed; and is made in the direction of the right line in which that force is impressed."

"III. To every action there is always opposed an equal reaction: or the mutual actions of two bodies upon each other are always equal, and directed to contrary parts."

"Newton's Principia; the Mathematical Principles of Natural Philosophy, by Sir Isaac Newton," translated into English by Andrew Motte, publ. Daniel Abbe, New York, 1848, pp. 83–84.

THE RESISTANCE OF METALS UNDER PRESSURE

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Communicated by E. H. Hall. November 27, 1916

In this note are summarized many of the most important results of an extended series of measurements on the resistance of metals under hydrostatic pressure. A more detailed account of the experiments has been offered for publication in the *Proceedings of the American Academy of Arts and Sciences*. The pressure range of this work is from 0 to 12,000 kg., and the temperature range from 0° to 100°C. The most extensive previous measurements have been made by Lisell and Beckman,[1] who made all their measurements at 0° over a pressure range of 3,000 kg.

This investigation includes 22 metals, embracing nearly all the common metals obtainable in the form of wire. Special precautions were taken in most cases to insure the greatest possible purity; the temperature coefficient of resistance at atmospheric pressure affords an indication of the probable purity. Of the metals above, Sn, Cd, and Zn were Kahlbaum's 'K' grade, Tl and Bi were prepared by electrolysis and were of high purity, Pb, Ag, Au, Cu, Fe, and Pt were of exceptional purity, and the others are probably not better than of high commercial purity except Al, which was much better than ordinary.

The results are summarized in the table. Two kinds of pressure coefficient are listed: 'instantaneous coefficient' and 'average coefficient'. The 'instantaneous coefficient' is the value of the derivative $\frac{1}{w}\left(\frac{\partial w}{\partial p}\right)_t$, where w is the observed resistance at the pressure and temperature in question. The 'average coefficient' between 0 and 12,000 kg. is the total change of resistance between 0 and 12,000 kg. divided by 12,000 and by the resistance at atmospheric pressure at the temperature in question.

The effect of pressure is to decrease the resistance of all metals except Bi and Sb, the resistance of which suffers a comparatively large increase. The anomalous behavior of Bi was known before, but that of Sb is new. The Sb was used in the form of extruded wires. The magnitude of the effect for normal metals varies from 12% to 0.8% for 10,000 kg., but Te forms a notable exception.

It is apparent from the table that the relative change of pressure coefficient with temperature is much less than the relative change of

resistance itself; the direction of change may be either an increase or a decrease. Another statement of this fact is that the temperature coefficient changes very little with pressure; this is shown in the second and the third columns of the table. This is perhaps surprising when it is remembered that the pressures used here are in many cases great enough to compress the metal to considerably less than its volume at 0°Abs. at atmospheric pressure. The instantaneous coefficient de-

TABLE

METAL	AVERAGE TEMPERATURE COEFFICIENT 0 TO 100°		PRESSURE COEFFICIENTS					
			Instantaneous coefficient at 0°		Instantaneous coefficient at 100°		Average coefficient 0 to 12,000 kg.	
	At 0 kg.	At 12,000 kg.	At 0 kg.	At 12,000 kg.	At 0 kg.	At 12,000 kg.	At 0°	At 100°
In	+.00406	+.00383	$-.0_41226$	$-.0_41016$	$-.0_41510$[3]	$-.0_41072$[3]	$-.0_41021$	$-0.{}_41131$[3]
Sn	447	441	$_41044$	$_5936$	$_41062$	$_5973$	$_5920$	$_5951$
Tl	517	499	$_41319$	$_41180$	$_41456$	$_41200$	$_41151$	$_41226$
Cd	424	418	$_41063$	$_5837$	$_41106$	$_5887$	$_5894$	$_5927$
Pb	421	412	$_41442$	$_41220$	$_41483$	$_41237$	$_41212$	$_41253$
Zn	416	420	$_5540$	$_5425$	$_5524$	$_5407$	$_54700$	$_54544$
Al	434	435	$_5416$	$_5365$	$_5397$	$_5373$	$_53815$	$_53766$
Ag	4074	4069	$_5358$	$_5321$	$_5355$	$_5331$	$_53332$	$_53362$
Au	3968	3964	$_5312$	$_5286$	$_5304$	$_5292$	$_52872$	$_52918$
Cu	4293	4303	$_5201$	$_5179$	$_5184$	$_5175$	$_51832$	$_51770$
Ni	4873	4855	$_5158$	$_5142$	$_5163$	$_5156$	$_51473$	$_51575$
Co	3657	3676	$_6941$	$_6814$	$_6755$	$_6704$	$_6873$	$_6726$
Fe	6206	6184	$_5241$	$_5218$	$_5247$	$_5230$	$_52282$	$_52353$
Pd	3178	3185	$_5198$	$_5190$	$_5189$	$_5187$	$_51895$	$_51863$
Pt	3868	3873	$_51975$	$_5181$	$_5190$	$_5182$	$_51870$	$_51838$
Mo	4336	4340	$_5133$	$_5126$	$_5130$	$_5125$	$_51286$	$_51265$
Ta	2973	2967	$_5149$	$_5139$	$_5153$	$_5147$	$_51430$	1486
W	3219	3216	$_5128$	$_5121$	$_5130$	$_5123$	$_51234$	$_51258$
Mg	390[1]		0_555				0_555	
Sb	473	403	$+ 0_41220$	$+ {}_41064$	$+ {}_5768$	$+ {}_5723$	$+ {}_41220$	$+ {}_5768$
Bi	438	395	$+ 0_4154$	$+ {}_4213$	$+ {}_4152$[4]	$+ {}_41895$[4]	$+ {}_42228$	$+ {}_41980$[4]
Te	$-.0063$[2]		$-.0_5129$					

[1] 0° to 20°, [2] 0° to 24°, [3] extrapolated from 50°, [4] extrapolated from 75°.

creases with rising pressure, as is natural, but it is at first sight strange that the percentage decrease is in nearly all cases less at the higher temperatures.

The numerical results of the above table are not in particularly good agreement with the previous results of Lisell and Beckman.[1] Suggestions as to the reasons for the discrepancies may be found in the detailed paper. Beckman has made extended application of his results to a theory of the pressure effect recently put forward by Grüneisen,[2] and

has come to the conclusion that Grüneisen's theory can not be more than a first approximation. This conclusion will not be altered by using the values of the table above instead of those of Beckman.

The theory of Grüneisen is incomplete in the sense that it gives the pressure coefficient of resistance in terms of the temperature coefficient as well as several thermodynamic constants. I hope to show at some length elsewhere that both the temperature and the pressure coefficient of resistance may be calculated with better agreement than by Grüneisen's formula by putting the proportional change of resistance in any direction equal to twice the proportional change of average amplitude of atomic vibration. This is capable of theoretical explanation on the ground that when the atoms are at rest the electrons pass freely from atom to atom, but when the separation of the atoms by haphazard heat agitation becomes too great, the electrons encounter difficulty in jumping from atom to atom.

The expenses of this investigation were in large part met by generous appropriations from the Bache Fund of the National Academy of Sciences, and from the Rumford Fund of the American Academy of Arts and Sciences.

[1] Lisell, E., Inaug. Dis., Upsala, 1902; and Beckman, B., Inaug. Dis., Upsala, 1911; *Ark. Mat. Astr. Fys.*, 7, 1912, No. 42 (1-18); *Ann. Physik., Leipzig*, 46, 1915, (481-502) and (931-941); *Physik. Zs., Leipzig*, 16, 1915, (59-63).

[2] Grüneisen, E., *Berlin, Verh. D. physik. Ges.*, 15, 1913, (186-200).

THE RATE OF DISCHARGE OF CENTRAL NEURONES

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Communicated by W. B. Cannon, November 27, 1916

The frequency of nerve impulses discharged from the central nervous system in voluntary and reflex contraction of the skeletal muscles presents a problem concerning which great difference of opinion is found among investigators. Piper, studying the electrical disturbance with a string galvanometer, has shown that in human muscles a fairly regular series of action currents with a rhythm of about 50 per second accompanies voluntary contraction. He inferred from this that the central nervous system sends to the muscle 50 impulses per second.

Buchanan, chiefly on the basis of experiments on frogs, reached the conclusion that the observed rhythm is not that of the motor nerve impulses but is dependent on the condition of the muscle itself.

By applying the principle of Buchanan's experiments to human muscles we have obtained results which confirm her main conclusion. Electrodes connected with a string galvanometer were so applied to the skin as to lead off the action currents of the first dorsal interosseous muscle lying close under the skin between the thumb and forefinger. Records were made in this way with the muscle in voluntary contraction at normal, subnormal and supernormal temperatures. Lowering of temperature was produced by immersing the hand and most of the forearm in water as cold as could readily be borne (6° to 7°C)., for about fifteen minutes; heating was produced by immersion in water at 45°C. for a somewhat shorter time. By immersion to a point above the elbow the forearm flexor muscles were treated and investigated in the same way. An unmistakable and fairly marked decrease in the frequency of muscular action currents results from this process of chilling; and an increase, which, as is to be expected, is less marked, results from heating. As nearly as can be judged from the somewhat irregular oscillations, the average action current frequencies at the three temperatures studied were in the case of the interosseous muscle as follows: cold 39 per second, normal 56 per second, hot 63 per second, in the case of the forearm flexors the corresponding estimates are, cold 36, normal 48, hot 50. We claim no strict quantitative accuracy for these figures, but the uniformity of the estimates justifies the conclusion that there is a real change of rhythm correlated with change of temperature. The temperature of the body as a whole was shown to undergo no significant change during the experiments.

The result apparently leads to the conclusion that the observed electrical muscle rhythm is not that of central innervation. It is difficult to conceive how a change in temperature in the muscle could alter the frequency of discharge of impulses from the ganglion cells whose temperature remains constant. But each propagated disturbance in a muscle fiber must depend on stimulation by a nerve impulse. Nerve impulses must therefore be so distributed in time as to render possible various frequencies of response such as those observed, depending on the temperature of the muscle. This demands a higher frequency of nerve impulses than that observed in the muscle rhythm. A careful study of the most perfectly rhythmical oscillations in our records leads us to estimate that there must be at least 300 nerve impulses per second to account for the muscle rhythms shown to be possible at various temperatures.

Piper's strongest argument against Buchanan's view of muscle rhythm which we here substantiate, is that the frequency of stimuli applied

to a motor nerve can be raised as high as 300 per second and a corresponding action current rhythm will still appear in the innervated muscle. If the muscle can respond separately to as many as 300 nerve impulses per second, he argues that it will not respond as slowly as fifty times per second unless it is stimulated only fifty times per second.

An explanation of the paradox is found in an analysis of the series of events involved in the excitation of a muscle through its nerve, in the light of certain functional properties shown by Lucas and Adrian to be essentially common to the two tissues. In either tissue a response is followed by a brief 'refractory period,' at first absolute when the tissue cannot be excited, then relative when the tissue can only be excited by a stronger stimulus than usual, and when the response is of subnormal magnitude. During the relative refractory period the excitability and the magnitude of response both return gradually to normal. At all times, including the refractory period, the magnitude of response in nerve is independent of the strength of stimulus and dependent only on the state of the tissue. The refractory period in nerve, though similar to that in muscle is much shorter.

If we assume that the stimulating effect of the nerve impulse on muscle is correlated with its magnitude as shown by other criteria, then we find no conflict between the ability of muscle to respond separately to 300 impulses per second and its response at a slower frequency to nerve impulses of a higher frequency. For as each nerve impulse is made to come earlier in the nerve's refractory period which was due to the preceding impulse, its magnitude is reduced till, coming at so early a stage in the refractory period of the muscle, it fails to excite it. One or more nerve impulses will be ineffective following each that is effective. The higher the nerve frequency the weaker will be each impulse and the more must the muscle recover to be again excited. Thus after the critical frequency is reached, the higher the nerve frequency the slower the muscle rhythm. In this respect we depart from Buchanan's view to the extent of supposing an indirect dependence of muscle rhythm upon nerve rhythm.

In view of this analysis, since the nerve rhythm evidently does not correspond with the muscle rhythm, it must have a higher frequency than the critical value (300 or more per second) below which the muscle can follow the nerve rhythm; otherwise the muscle rhythm would correspond with it.

Inferences based on these considerations together with observations of Lucas, Adrian and Garten, lead us to suppose that the normal fre-

quency of nerve impulses discharged from the ganglion cells in voluntary contraction must lie between 300 and 5000 per second.

The complete paper will appear in the *American Journal of Physiology*, January, 1917.

A PHYSIOLOGICAL STUDY OF NOCTILUCA, WITH SPECIAL REFERENCE TO LIGHT PRODUCTION, ANAESTHESIA AND SPECIFIC GRAVITY

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Communicated by A. G. Mayer, December 5, 1916

The specific gravity of *Noctiluca* is less than that of sea water, so that normally the animals float at the surface. They contain no air bubbles or large oil drops, and their lower specific gravity must therefore be due to a lower salt content than the sea water. In more concentrated sea water, the animals shrink and in more dilute sea water they swell; the plasma membrane therefore has the usual semipermeability toward the balanced salts of sea water, i.e., permeability to water and impermeability to salts. When placed in a mixture of 4 sea water; 6 fresh water, the animals sink, their salt content being now greater than that of the surrounding medium, but later they rise to the surface, a process independent of the movement of the tentacle. They must therefore have absorbed water not only until their salt content is the same as that of the surrounding medium (when they should still sink), but enough water to make their salt content again less than the 4 s.w.: 6 f.w. mixture, thus re-establishing their normal relation to the surrounding medium. This water must be absorbed against the osmotic pressure of the salts of sea water, a process contrary to physical laws. The animals can not only lessen their specific gravity, but they can also increase it, as is shown by the fact that on windy days they sink far beneath the surface of the sea. Anesthetics, acids and alkalies, KCN, and the pure solutions of the salts of sea water do not interfere with this regulatory mechanism except when they cause irreversible changes and death of the cells; dead *Noctiluca* always sinks to the bottom.

Light production in *Noctiluca* normally occurs only on stimulation of any kind, and is a momentary bright flash; dying animals produce a bright steady glow. The luminescence is traceable to points of light coming from granules in the protoplasm. No substances were found which would cause a rhythmic flashing, comparable to the rhythmic

twitching of a muscle in pure NaCl; the only responses given were the momentary bright flashing and the steady glow. The latter response was called forth by anesthetics, cold (5°–0°C.), heat (43°–49°C.), acids and alkalies, fresh water, and a constant galvanic or interrupted induced electrical current. Cells injured by puncturing with a needle or by passage of an induced current respond to mechanical or electrical stimulation by a flash just as normal cells do. If, however, the cells are completely broken to fragments by pressing them through cheesecloth, they do not flash on stimulation although they do give a steady glow. Light production does not occur in absence of oxygen. The supposition that the cells are ordinarily impermeable to oxygen but become permeable on stimulation and on death, is not true, however, for the cells deprived of oxygen immediately give light when oxygen is readmitted. without being stimulated mechanically or in any other way.

Noctilucas may be anesthetized by certain concentrations of ether, chloroform, thymol, chloretone, ethyl and butyl alcohol so that they fail to give a flash on stimulation, but they always give a very faint glow; this disappears and the normal response returns on removing the anesthetic. *Noctilucas* differ in this respect from luminous bacteria whose light giving power can be completely anesthetized. (E. N. Harvey, *Biol. Bull.*, **29**, 1915, p. 308). Although the anesthesia of many processes, e.g., the contraction of heart muscle, cell division and growth has been shown to be independent of the consumption of oxygen, we should expect the anaesthesia of light production to be dependent on the consumption of oxygen since this process is a luminescent oxidation which will take place in solutions in a test tube free from cells. That this oxidation is different from that in most cells is shown by the fact that KCN has no effect on the light production in *Noctiluca* in relatively high concentrations, whereas it quickly paralyses the oxidations of most cells. The question arises whether the anesthesia of luminous cells is due to the fact that oxygen cannot pass through the membrane, or to the fact that it cannot be used. The latter alternative is suggested by the experiment of removing oxygen from the cells and readmitting it, thus showing that oxygen can pass the membrane at any time. The question is answered by an experiment in which the cell substance of anesthetized cells was permitted to come in contact with dissolved oxygen. Narcotized cells were broken up by shaking with sand, and it was found that they produced only a faint light whereas normal cells so treated became very brilliant. The anesthetic must therefore attack the mechanism of the utilization of oxygen in the cell, and not the permeability of the cell membrane for oxygen.

PHYSIOGRAPHIC SUBDIVISION OF THE UNITED STATES

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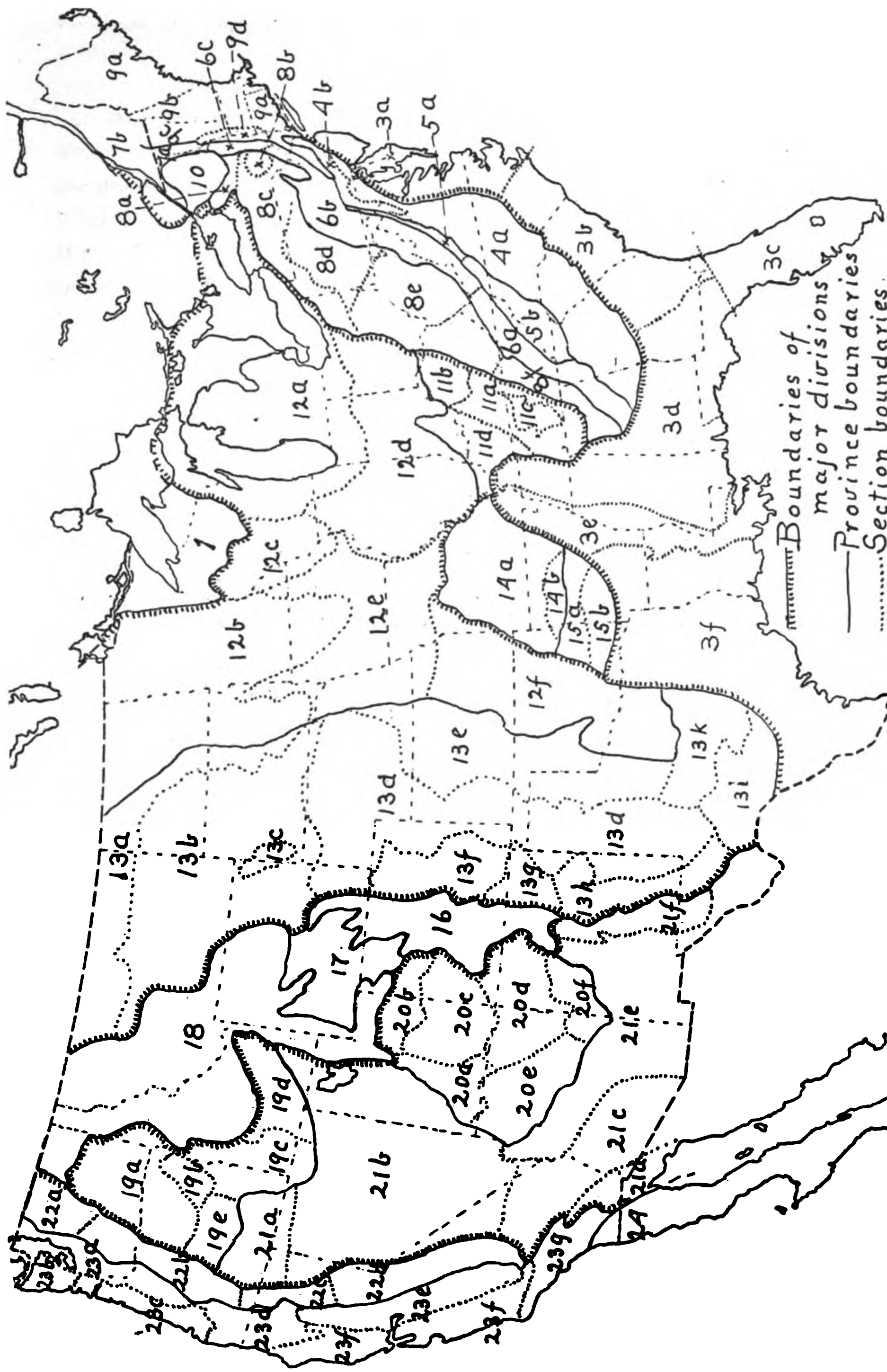
Communicated by W. M. Davis, November 24, 1916

Various attempts at subdivision of the United States into physiographic provinces have been made, beginning with that of Powell.[1] The Association of American Geographers, recognizing the fundamental importance of this problem, appointed a committee in 1915 to prepare a suitable map of physiographic divisions. The committee consists of Messrs. M. R. Campbell and F. E. Matthes of the U. S. Geological Survey and Professors Eliot Blackwelder, D. W. Johnson, and Nevin M. Fenneman (chairman). The map herewith presented and the accompanying table of divisions constitute the report of that committee. The same map on a larger scale (120 miles to the inch) will be found in Volume VI of the *Annals of the Association of American Geographers*, accompanying a paper by the writer on the Physiographic Divisions of the United States. In that paper are given the nature of the boundary lines and those characteristics of the several units which are believed to justify their recognition as such. Though the above-named committee is not directly responsible for the statements there made, many of them represent the results of the committee's conferences. The paper as a whole is believed to represent fairly well the views of the committee, though in form the greater part of it is a revision of a former publication.[2]

The basis of division shown on this map, here reproduced, is physiographic or, as might be said in Europe, *morphologic*. The divisions are based on land forms, not on climate or vegetation. If subdivision were carried far enough on the same principle each unit of the lowest order would comprise but one physiographic type. In most cases this has not been done. Even the units of the lowest order generally embrace several types closely associated in their development.

The genetic classification of land forms is now generally familiar to geographers, even to those who do not use it. In this system physiographic forms are classified according to their histories. Forms which result from similar histories are characterized by certain similar features, and differences in history result in corresponding differences of form. Generally the distinctive features which are important in a genetic classification are also obvious to the casual observer, but this is not universal. Thus a maturely dissected plateau may grade without a break from rugged mountains on the one hand to mildly rolling farm



PHYSIOGRAPHIC DIVISIONS OF THE UNITED STATES

lands on the other. So also, forms which are not classified together may be superficially similar; for example, a young coastal plain and a peneplain. Hence this map, which distinguishes physiographic types based on a genetic classification, does not in all cases make the distinctions which are most obvious to the casual observer. On the whole however, this discrepancy is not great. A very large proportion of all the boundaries shown on this map are familiar features. To have based the divisions purely on superficial features in proportion to their magnitude, would not have resulted in the making of units suited to scientific treatment.

Important physiographic differences between adjacent areas are, in a large proportion of cases, due to differences in the nature or structure of the underlying rocks. Where this is the case the two areas are distinguished on the geologic as well as the physiographic map. Distinctions based on geologic age also correspond to physiographic distinctions where the forms are so recent as to be in their first erosion cycle, as is generally the case with sheets of glacial drift. When these facts are remembered, it is not surprising that a large proportion of the boundary lines shown on the accompanying map are also geologic lines.

The segments here presented are of three orders, called respectively major divisions, provinces and sections. The basis of distinction among coördinate units is very much the same in all the orders. On the whole it may be said that contrasts in structure are stronger and more general between adjacent major divisions than between adjacent divisions of lower orders. Naturally also, the degree of topographic homogeneity is greatest in the units of the lowest order, but the reasons for calling one area a major division and another a province or a section are not clearly defined.

The degree of homogeneity in the several divisions of the same order is not in all cases the same. Homogeneity is strong in the Dissected Till Plains (12-e) which are practically everywhere submaturely dissected plains of moderate relief; also in the Snake River Plain (19–d) which is everywhere a young lava plain. On the other hand, the East Gulf Coastal Plain (3–d) is a heterogeneous area, for it grades from a young marine plain with undeveloped drainage near the coast to a maturely dissected, belted coastal plain farther inland; this case illustrates the inclusion of several types in one section by gradation, where no good dividing line is known, and where practical convenience requires that the types be considered in their mutual relations. Again, the Nevada Basin (21–b) comprises isolated mountain ranges (probably dissected block mountains) separated by aggraded desert plains; here

is an intimate intermixture of several types which are, however, so related genetically that both are accounted for by the same history. Indeed, while this work has no direct reference to teaching, there is something almost final about the requirement that a province or section should be a suitable unit for scientific treatment. This is quite as necessary from the standpoint of government surveys as from that of the university.

The committee distinctly disclaims finality for this work. With further investigation and more exact mapping some of the boundary lines here given may be shifted. The lines on this map were located by aid of the largest scale topographic and geologic maps available. Parts of the country are, however, imperfectly mapped, hence, with respect to exact plotting, the values of the several lines are unequal. All are necessarily generalized. As the result of future studies it may well be that the rank assigned to some of the units will be changed. Units of still lower orders will of course be made. Above all, the presentation of this map is not intended to preclude the use of other kinds of physical divisions like those of Supan, De Martonne, Herbertson, or Dryer. It is believed, however, that for a map of physiographic divisions, the main features of the one here presented will not be greatly changed.

The uses of such a map are of two general classes, (1) for scientific (explanatory) description, and (2) for comparative studies with other geographic elements. In the former, physical features are looked upon as the product of geologic processes, in the latter they constitute factors or conditions of life and human activity. In the former aspect they are an effect; in the latter a cause. The potency of such causes can only be known when statistics of population, agriculture and industry and even politics are graphically shown with due respect to natural units. It is plain that if matters statistical are to be represented on a map of natural divisions, and things human are to be discussed in terms of their physical setting, the value of the relations discovered will depend largely on the character of the natural divisions and their proper delimitation.

In the following table the names of major divisions are printed in italics; the provinces are numbered, and the sections lettered. The province number and the section letter correspond to those on the map.

Table of physiographic divisions of the United States

Laurentian Upland.—

1. Superior Upland

Atlantic Plain.—

2. Continental Shelf
3. Coastal Plain: (a) Embayed section; (b) Sea Island section; (c) Floridian section; (d) East Gulf Coastal Plain; (e) Mississippi Alluvial Plain; (f) West Gulf Coastal Plain.

Appalachian Highlands.—

4. Piedmont province; (a) Piedmont Upland; (b) Triassic Lowland.
5. Blue Ridge province: (a) Northern section; (b) Southern section.
6. Appalachian Valley province: (a) Tennessee section; (b) Middle section; (c) Hudson section.
7. St. Lawrence Valley: (a) Champlain Valley; (b) Northern section.
8. Appalachian Plateaus: (a) Mohawk section; (b) Catskill section; (c) Allegheny Plateau (glaciated); (d) Allegheny Plateau (Conemaugh section); (e) Kanawha section;* (f) Cumberland section.
9. New England province: (a) New England Upland; (b) White Mountain section; (c) Green Mountain section; (d) Taconic section.
10. Adirondack province.

Interior Plains.—

11. Interior Low Plateau:† (a) Highland Rim Plateau; (b) Lexington Plain; (c) Nashville Basin; (d) not named.
12. Central Lowland: (a) Eastern Lake section; (b) Western Lake section; (c) Wisconsin Driftless section; (d) Till Plains; (e) Dissected Till Plains; (f) Osage Plains.
13. Great Plains: (a) Missouri Plateau (glaciated); (b) Missouri Plateau (unglaciated); (c) Black Hills; (d) High Plains; (e) Plains Border; (f) Colorado Piedmont; (g) Raton section; (h) Pecos Valley; (i) Edwards Plateau; (k) Texas Hill section.

Interior Highlands.—‡

14. Ozark province: (a) Salem-Springfield plateaus; (b) Boston Mountains (plateau);
15. Ouachita province; (a) Arkansas Valley section; (b) Ouachita Mountains.

Rocky Mountain System.—

16. Southern Rocky Mountains (to be divided into sections).
17. Wyoming Basin.
18. Northern Rocky Mountains (to be divided into sections).

Intermontane Plateaus.—

19. Columbia Plateau: (a) Walla Walla Plateau; (b) Blue Mountains; (c) Payette section; (d) Snake River Plain; (e) Harney section.

* Likewise part of the Allegheny Plateau.

† In the report of the committee this is called the Highland Rim Province. Messrs. Campbell and Matthes do not concur in the exclusion of this entire province from the Appalachian Highlands. They would divide it, and assign the eastern half to the Appalachians.

‡ In the report of the committee this is called the Ozarkian Highlands.

20. Colorado Plateaus: (a) High Plateaus of Utah; (b) Uinta Basin; (c) Canyon Lands; (d) Navajo section; (e) Grand Canyon section; (f) Datil section.
21. Basin-and-range province: (a) Oregon lake section; (b) Nevada Basin; (c) Sonoran Desert; (d) Salton Trough; (e) Mexican Highland; (f) Sacramento section.

Pacific Mountain System.—

22. Sierra-Cascade Mountains: (a) Northern Cascade Mountains; (b) Middle Cascade Mountains; (c) Southern Cascade Mountains; (d) Sierra Nevada.
23. Pacific Border province: (a) Puget Trough; (b) Olympic Mountains; (c) Oregon Coast Range; (d) Klamath Mountains; (e) California Trough; (f) California Coast Ranges; (g) Los Angeles Ranges.
24. Lower Californian province.

[1] An excellent account of these attempts has been given by Joerg, W. L. G., *Assoc. Amer. Geogr., Annals*, 4, 1914 (55–84), 22 maps.

[2] Fenneman, N. M., *Assoc. Amer. Geogr., Annals*, 4, 1914 (84–134), 3 maps.

ON THE COMPOSITION OF THE MEDUSA, CASSIOPEA XAMACHANA AND THE CHANGES IN IT AFTER STARVATION

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Communicated by A. G. Mayer, December 13, 1916

Cassiopea may be divided into three distinct parts; mouth-organs, umbrella and velar margin. Since these three parts differ not only morphologically, but also in their absolute weights, as well as in the relative amount of cellular and non-cellular constituents, it was thought desirable to study the normal growth of these parts in order to determine whether the starving Cassiopea loses weight uniformally or whether the loss is dissimilar in the three parts concerned. A large number of observations were also made on the undivided Cassiopeas.

The observations made on the normal Cassiopea may be summarized as follows: (1) Relative weights of mouth-organs, umbrella and velar margin differ somewhat according to the size of the entire body. (2) The water content of the entire body, as well as of different parts, is practically identical throughout the animal's life cycle, so far as followed. (3) The percentage of nitrogen in the solids is highest in the smallest medusa, and the values decrease progressively with increasing body weight. The percentage of nitrogen is highest in the velar margin and decreases in the mouth-organs and umbrella in the order named.

All these observations are interpreted as indicating that in the structures in which the cellular elements are abundant, the nitrogen content tends to be high. (4) The ash content, like the water content, is probably the same not only in the Cassiopea as a whole, but also in each of the three parts throughout the life cycle. The next step was to determine the changes in Cassiopea as the result of starvation.

Mayer ('14) reported (*Carnegie Inst. Publ.*, No. 183, pp. 55–84) that in Cassiopea the percentage of nitrogen to the total solids remains constant during the entire period of starvation. Mayer infers from this that "no appreciable chemical change occurs in the composition of its body, and that there is no appreciable selective use of different substances at different times during the progress of starvation." This is remarkable since the starving mammalian body reveals a totally different relation owing to the rapid disappearance of reserve substances such as carbohydrates and fats during the earlier period of starvation, followed by a slow consumption of protein substances later. Thus the starving mammalian body gives different percentage values for the nitrogen at different periods of starvation, especially in the earlier stages.

At present my observations are limited to the composition of the Cassiopeas at the end of the starvation experiment, while I reserve the question of progressive changes during the stages of inanition for the future.

For this purpose eight freshly caught normal Cassiopeas, having different body weights, were subjected to starvation. The starvation was accomplished by placing the animal in filtered sea water. The filtration was made with all the precautions adopted by Mayer ('14) and the water (4500 cc.) in the vessel was changed once every day. Briefly summarized the results of the observations made on the starved Cassiopea are as follows:

1. In general the smaller Cassiopea loses relatively more in weight than does the larger Cassiopea.

2. The percentage of water found in the entire body, as well as in the three different parts is nearly the same in all sizes of Cassiopea. However the values of water content in the starved, appears to be slightly higher than that found in the normal Cassiopea.

3. The nitrogen content of the entire body is higher in the smaller than in the larger Cassiopea, as in the case of the normal animals.

4. However the absolute amount of nitrogen found in the starved Cassiopea is considerably higher than in the normal having the same body weight. It was noted also that although high when compared with the normal, equal in weight to the starved animal, it is very low for the initial body weight of

the starved animal. This shows that the nitrogen also has been consumed during the period of starvation.

5. The nitrogen contents for the different parts of the body are similar in their relations to those found in the normal Cassiopea.

6. The loss in weight of the different parts is of such a character that their proportions in the starved remain similar to those in the normal Cassiopea.

From his data Mayer concluded that the percentage of nitrogen in the solids is independent of the period of starvation, and is practically identical with that obtained from the non-starved Cassiopea. I have however found, as stated above, that starvation tends to increase not only the percentage of nitrogen in the solids, but also that the absolute amount of nitrogen shows an increase when the starved Cassiopeas were compared with the normals having the same body weight.

The discrepancy between the conclusions drawn by Mayer and by myself is I believe due to the fact that Mayer's observations were limited to the larger Cassiopeas (body weights over 100 grams) in which the percentage of nitrogen in the solids shows little variation following the large variations of the body weight, while the variations in the nitrogen are quite noticeable in the Cassiopeas of smaller size. I may add here that the data given by Mayer in his table 2, show also a slight indication of a difference in the nitrogen content between the normal and starved Cassiopeas.

I have applied Mayer's law for the loss of weight in starving Cassiopeas to my own data, and found a satisfactory agreement between the observed and calculated values by the formula

$$Y = 83.58\ (1-0.05)^{x-1}$$

where Y represents body weight and X the number of days of starvation. My formula differs from that of Mayer in that I give the exponent as $(x-1)$ while Mayer gives it simply as x. He did not however consider the loss during the first day as being due to starvation, for during this time considerable quantities of undigested food and slime are discharged and the loss is thus excessive and irregular. Hence our formulas are in essential accord one with the other, both applying *after* the medusa has discharged its undigested food and its gastric cavity is empty. The lower body weight obtained, as compared with that calculated at an earlier period of starvation, was probably due to the frequent handling of the animal in order to determine the body weight daily.

A more detailed paper will appear in a volume of *Researches* from the Department of Marine Biology of the Carnegie Institution now in preparation.

STUDIES OF THE MAGNITUDES IN STAR CLUSTERS, IV. ON THE COLOR OF STARS IN THE GALACTIC CLOUDS SURROUNDING MESSIER 11

By Harlow Shapley

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Communicated by G. E. Hale, December 18, 1916

The investigation of phenomena relating to brightness and color in the clusters that are located in rich regions of the sky necessitates a knowledge of certain properties of the neighboring stars. A catalog of the members of any cluster must inevitably include some stars extraneous to the group, and to a greater or less extent the colors and magnitudes of such objects will influence the interpretation of the results. For the globular clusters, particularly in high galactic latitude, the preponderance of cluster stars over those that chance to be included with them is such that no serious effect is to be feared. For the open clusters, however, and especially for those in the Milky Way, the number of background or foreground stars may easily exceed the number of physical members of the cluster, even at the center of the group. In these cases it becomes necessary to make a direct and special study of the extra-cluster stars in fields far enough removed from the cluster to be practically free of its outlying members, but not so distant that the field stars can not be considered representative of those that in projection appear commingled with the cluster.

A study of the colors in the open galactic system Messier 11 (N. G. C. 6705) has been completed with the compilation of a catalog of the magnitudes and colors of nearly five hundred stars. The cluster is situated in the constellation Scutum Sobieski, in one of the densest of the great star clouds of the Milky Way. Unlike the stars in Messier 67, in which all the fainter objects are yellow and red (and similar in most respects to the neighboring non-cluster stars), those in Messier 11 fall into all color classes, but with a distinct preference for the bluer types. The group is but four degrees from the mid-line of the Galaxy; Messier 67 is in galactic latitude $+34°$. To find whether the presence of blue faint stars in Messier 11 is a peculiarity of the cluster or is only a result of its low galactic latitude and its association with the star clouds, a special study in several neighboring fields was undertaken. This investigation bears directly on the relation of the cluster to the surrounding stellar masses, and also yields information regarding the nature of the galactic clouds themselves and the spectra and intrinsic luminosities of the numberless stars that compose them.

Nearly forty plates, made with the 60-inch reflector, have been used in the present work. In addition to the above-mentioned catalog of stars in and near the cluster, photographic and photovisual magnitudes of more than 300 stars in four other neighboring fields have been determined by comparisons with the cluster and checked by direct comparisons with the Polar Standards. Special care was taken to avoid

PERCENTAGE FREQUENCY OF COLORS IN GALACTIC CLOUD FIELDS

	COLOR CLASS											
	*b*0	*b*5	*a*0	*a*5	*f*0	*f*5	*g*0	*g*5	*k*0	*k*5	*m*0	*m*5
Field I	0	2	6	12	18	12	12	16	12	4	2	4
Field II	1	3	7	17	13	8	8	8	12	12	5	6*
Field III	0	2	17	29	10	10	10	8	4	8	4	0
Field IV	0	0	2	27	24	22	10	0	8	4	4	0
All fields	0.3	2	8	20	15	11	9	8	10	8	4	4†

* 2.5% have color indices greater than +2.00.
† 1.3% have color indices greater than +2.00.

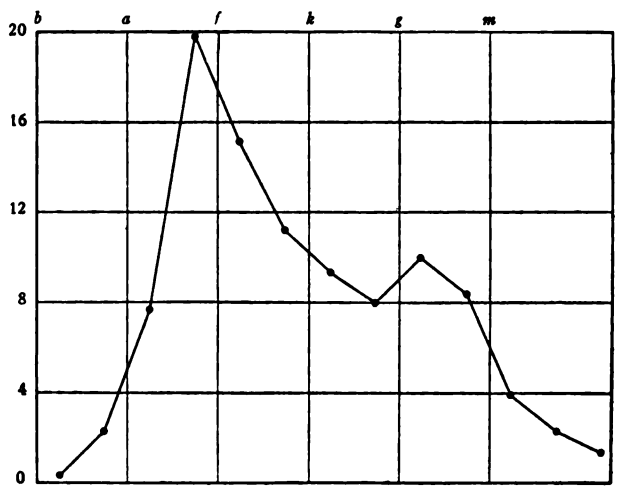

FIG. 1. FREQUENCY OF COLORS IN GALACTIC CLOUDS. ABSCISSAE ARE COLOR CLASSES; ORDINATES ARE PERCENTAGES.

the use of questionable photographs or of doubtful star images, and systematic errors of any importance are probably completely absent from the results. The average probable error of a color index varies in the different fields from a tenth to a twentieth of a magnitude.

The interval of photovisual magnitude investigated is from 11.0 to 15.5. The relative frequency of color class is given in the accompanying table for each field separately and for the four together. Figure 1

illustrates the combined results. The most important features are the great diversity of color index and the general resemblance of this frequency curve to that for the brighter stars in the immediate neighborhood of the sun.[1]

The full presentation of the material will appear in two forthcoming *Contributions from the Mount Wilson Solar Observatory.* Some of the conclusions to be drawn from the results may be summarized as follows:

1. Messier 11, in common with many other open clusters, does not differ greatly in the brightness and color of its stars from the non-condensed fields which surround it. Though its members without doubt form a distinct physical system, it is probably at approximately the same distance from the sun as the stars of corresponding color and apparent magnitude in the galactic cloud. In fact, the study of colors and magnitudes tends to confirm Barnard's inference, based on general appearances, that the cluster may be nothing more than a nucleus of the extensive surrounding stellar masses.[2] In this similarity to its environment, therefore, Messier 11 is vastly different from a globular cluster, such as Messier 13, which is apparently much more distant than the non-cluster stars in the same part of the sky.[3]

2. The distance of all the stars of Messier 11 is sensibly the same; hence apparent magnitudes represent relative absolute luminosities. Plotting the average color index against magnitude, as in figure 2, a striking progression of color with decreasing brightness is revealed. The neighboring stars, though showing much the same frequency of color class, are probably more scattered in space, and each interval of magnitude includes stars varying greatly in distance and of most diverse spectral types. The indefinite relation between magnitude and color index here observed is, therefore, not unexpected.

3. Accepting the present results as dependable and taking color class and spectral type as closely analogous, the presence of the small and negative color indices in the galactic clouds indicates either that the stars are at a great distance or that they are not similar in luminosity to the brighter stars near the sun. In the light of recent researches on the dispersion of absolute magnitudes of B and A-type stars, the first alternative is decidedly preferable. The parallax of a typical B-type star of apparent magnitude 13.5 would be about 0".0002, a value that may be taken as a first estimate of the distance of Messier 11. The *b*-class stars in the surrounding clouds are between magnitudes 13 and 15 and some are possibly fainter—suggesting still greater distances for parts of the stellar background.

4. Just as a wide range of color class and the presence of negative

color indices in the globular cluster Messier 13 indicated the absence of scattering of light in space,[4] the similar properties of the faint stars in Messier 11 and the galactic clouds show that in this direction also light is not appreciably diminished by scattering.

5. The present investigation contributes to the problem of the color of the faint stars, and consequently, if space absorption is accepted as ineffective, to the question of the extent and character of the galactic system. The increase of the minimum color index with decreasing brightness has been observed by Seares[5] for the north polar stars, galactic latitude +28°, by Hertzsprung[6] and Seares[7] in the open cluster

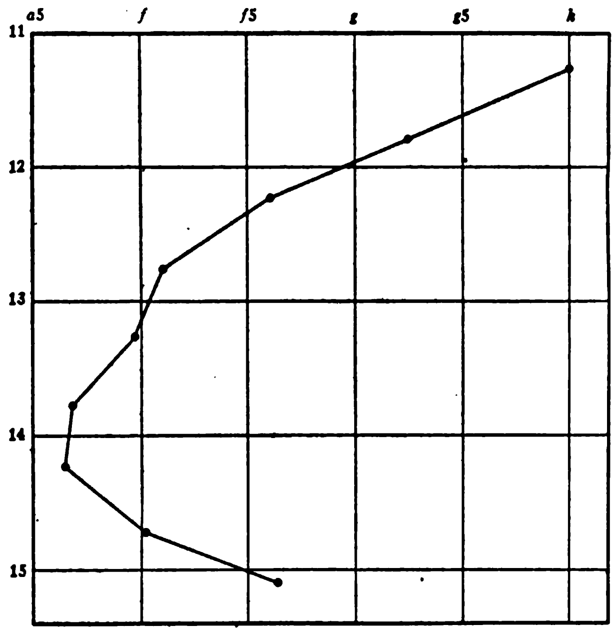

FIG. 2. RELATION BETWEEN COLOR AND LUMINOSITY IN MESSIER 11. ABSCISSAE ARE COLOR CLASSES; ORDINATES ARE APPARENT MAGNITUDES.

N. G. C. 1647, galactic latitude −15°, by the present writer in and around Messier 67,[8] galactic latitude +34°, and in the neighborhood of Messier 13, galactic latitude +40°. In none of these fields have very small or negative color indices appeared among the fainter stars;[9] and a definite, accessible limit to the galactic system was thus suggested, at least for the higher galactic latitudes. The first divergence from this tendency was found in the field of the variable star XX Cygni,[10] galactic latitude +13°, where 13th magnitude stars of color *b* were found, as well as a number of faint *a*'s. Similarly, faint blue stars have been recently observed by the writer in the region of the Perseus cluster,

galactic latitude $-3°$. With the addition of the data for Messier 11 and for the four neighboring fields, it appears that the redness of the faint stars will be found to depend, as might be expected, upon galactic latitude, and in the mid-galactic regions will vary with the density of the star clouds.

6. The presence of negative color indices for faint stars in the three widely separated galactic regions mentioned above shows that, if these stars are typical in absolute brightness, the dimensions of the galactic system in the plane of the Milky Way are many times greater than has been inferred from studies of variables and investigations of the motions and magnitudes of the brighter stars.

[1] For instance, see Parkhurst, Yerkes Actinometry, *Astroph. J., Chicago, Ill.*, **36**, 1912, (218, 225) and figure 5 on p. 56 of *Mt. Wilson Contrib.*, No. 116.

[2] *Berkeley, Univ. Cal. Pub., Lick Obs.* **11**, 1913, Plate 62.

[3] *Mt. Wilson Contrib.*, No. 116, 1915, (81 ff.).

[4] These PROCEEDINGS, **2**, 1916, (12–15).

[5] *Astroph. J., Chicago, Ill.*, **39**, 1914, (361–369); [*Mt. Wilson Contrib.*, No. 81].

[6] *Ibid.*, **42**, 1915, (92–119); [*Mt. Wilson Contrib.*, No. 100].

[7] *Ibid.*, **42**, 1915, (120–132); [*Mt. Wilson Contrib.*, No. 102].

[8] *Mt. Wilson Contrib.*, No. 117, 1916. The unpublished results for the fields near Messier 13 are provisional.

[9] In N. G. C. 1647 Hertzsprung (*loc. cit.*) finds one star of photographic magnitude 12.40 that is apparently of spectral type B.

[10] *Astroph. J., Chicago, Ill.*, **42**, 1915 (148–162); [*Mt. Wilson Contrib.*, No. 104].

THE COLOR OF THE STANDARD POLAR STARS DETERMINED BY THE METHOD OF EXPOSURE-RATIOS

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Communicated by G. E. Hale, December 18, 1916

An earlier note in these PROCEEDINGS describes a method of measuring the color of a star which depends upon the ratio of the exposure times necessary for its blue and yellow light to produce images of the same size.[1] A comparison of the observed exposure-ratio with a curve derived by combining similar ratios for stars of known color affords a means of expressing the results in terms of color-index or color-class.

The method is expeditious, and, under favorable conditions, precise; it is entirely independent of stellar magnitudes, and hence avoids the systematic errors which so easily enter as a result of uncertainties in the magnitude scales or in their zero points. Moreover, the method is direct, in the sense that color is measured and not inferred from observations of spectral type. The results thus include that part of the color

which is a function of the star's intrinsic luminosity and also the influence of a possible scattering of light in its passage through space.

The following paragraphs indicate briefly the results found with the 60-inch reflector for about 80 of the North Polar Standards between magnitudes 2.5 and 16.3 whose colors had already been derived by a comparison of their photographic and photovisual magnitudes. The brighter stars, owing to their distribution, had to be observed separately. To avoid photographic difficulties, the plates for these were all of the same emulsion. Further, all the brighter objects were reduced to an equivalent of the 11th magnitude, approximately, by means of screens and diaphragms and then given the following series of exposures on a Cramer Inst. Isochromatic plate:

a. Yellow images (through yellow filter), 16^s, 32^s.
b. Blue images (without filter), 2^s, 4^s, 8^s, 16^s, 32^s.
c. Yellow images (through yellow filter), 16^s, 32^s.

In a few cases the exposures for the yellow images were 32^s and 64^s.

The data on each plate were reduced graphically and gave two values for the exposure-ratio corresponding to equal blue and yellow images. In general, three plates, exposed upon different nights, were used for each star. To derive a reduction curve connecting color-index with exposure-ratio, the mean ratios were collected in groups according to color-index. Since each group contains stars of widely differing magnitudes, the resulting curve is probably free from the influence of any systematic error in the color-indices depending on brightness, and should give a reliable value for the color of any star whose exposure-ratio has been determined under conditions similar to those underlying the derivation of the curve itself. Moreover, the question of systematic errors in the original color-indices, which were taken from the Mount Wilson investigation of the photographic and photovisual magnitudes of the polar stars, can be put to a direct test.

For this purpose the color-index of each star was determined from its exposure-ratio by means of the reduction curve and compared with the original value based on magnitudes. The differences, arranged according to the brightness of the stars, should reveal any systematic error in the original system of color values, and thus test the accuracy, relative to each other, of the photographic and photovisual scales of the North Polar Standards. The means of these differences for groups of stars of a limited range of brightness are given in the second column of the table. Obviously there are no systematic differences of any importance which depend upon magnitude.

In order that the investigation might be extended to the fainter stars,

five additional photographs were made with single yellow exposures of 128^s and 10^m, respectively, and corresponding blue exposures of 8^s, 16^s, 32^s, 64^s, and 1^m, 2^m, 4^m. For these the full aperture of 60 inches was used, and the measurable images of all the stars shown were included in the discussion. On the plates of the first series described above, the images for objects of all degrees of brightness were approximately of the same size; but here there was no attempt at equalization.

The question as to the effect of size of image upon the exposure-ratio had accordingly to be examined. Inasmuch as the gradation of a photographic plate is to some extent a function of the wave-length of the light producing the image, the necessity for some correction of the exposure-ratios derived from these plates of longer exposure was anticipated. Special photographs of bright stars taken with a wide range in aperture, but with the same series of exposure times, gave data for a provisional determination of the corrections. These seem to be moderate, unless the images used for finding the exposure-ratio are very large. The corrected mean differences are in the third and fourth columns of the table. The two plates of 10^m exposure were of another emulsion, requiring a different reduction curve, and were accordingly reduced to the standard curve which applies to the remainder of the data.

Finally, an additional group of four plates of the original emulsion was exposed in November, four months after the others had been taken. The results for these are in the fifth column of the table. As the small correction depending on the size of the image is not yet well determined, it has been omitted in the case of this last group of plates. It will be noted that the results are substantially as before.

Difference Between Color-Index from Magnitudes and that from Exposure-Ratio
(Unit = 0.01 mag.; number of values in parentheses)

Photographic Magnitudes	Duration Yellow Exposures				Weighted Mean Difference	Number of Stars in Group
	$16, 32^s$	128^s	10^m	16^s to 8.5^m		
3.5	0(12)				0	2
5.7	+3(16)				+3	3
6.5	−2(20)				−2	4
7.5	+2(20)				+2	3
8.6	−3(24)				−3	3
9.4	−7(18)				−7	3
10.6	−4(48)				−4	6
11.4	+5(54)	+5(24)	+4(14)	−5(23)	+3	8
12.3	+2(4)	+3(15)		−5(15)	−1	7
13.3		+6(25)	+1(17)	+4(25)	+4	12
14.1		0(5)	+3(26)	−5(7)	+1	13
14.8			0(26)		0	13
15.5			−6(21)	+7(6)	−3	11

The last two columns of the table give the weighted mean differences between the Mount Wilson color-indices and the color-values derived from the exposure-ratios, and the number of stars included in each group.

The close agreement in the two series of results shows clearly enough the usefulness of the exposure-ratio method, and indicates that the photographic and photovisual magnitude scales for the North Polar Standards are substantially in the proper relation to each other. The agreement affords no test of the presence of errors affecting the two scales equally; but there can be no important divergence of either scale relative to the other, for the differences in the color-values derived by the two methods would then show a progressive change with magnitude.

In general, the precision of the method of exposure-ratios is excellent. For example, the probable error of a color-index derived from a single exposure-ratio is about 0.07 magnitudes. This value is based upon the results for the 36 stars for which there are five or more separate determinations, and speaks well for the uniformity of different plates of the same emulsion. Puzzling abnormalities have occasionally occurred, but the cases thus far met with are perhaps to be attributed to causes external to the plates themselves. Each emulsion will undoubtedly require a special investigation for the determination of factors which will reduce it to the standard curve connecting exposure-ratio and color-index, but this should not be a matter of any great difficulty. Nevertheless, the limitations of the method can not be fixed until a number of questions of this sort have been examined.

In the meantime, however, it may be noted that the colors of the Polar Standards, brighter than the 13th magnitude, have been determined to about the same precision as was reached in the investigation of the magnitude scales, with an expenditure of time and labor which was perhaps a tenth of that required for the earlier investigation. All of the observational data for the present investigation, including about 80 photographs and nearly 400 separate determinations of color, were obtained during three nights with a total of less than 22 hours observing time.

One of the most interesting consequences of this recent determination of the colors of stars near the Pole is to be found in the confirmation it affords of a result previously announced,[2] namely, that there are no faint stars in this region with negative or small positive color-indices. The lower limit of the color-index gradually increases as the fainter stars are approached, and at the 16th photographic magnitude its value is of the order of +0.5 magnitude. Although this state of affairs was

indicated with some certainty by the comparison of photographic and photovisual magnitudes, it is of interest to find it appearing as the result of an entirely different method of investigation.

The absence of faint white stars is known to be a characteristic of other regions as well,[3] but it must not be inferred that such objects are not to be found anywhere in the sky. In Selected Area No. 88, for example, in one of the outlying clouds of the Milky Way, photographs by the exposure-ratio method indicate that the stars of the 14th or 15th magnitude are nearly normal in color and thus include a considerable number of objects that are white. Mr. Shapley has also accumulated evidence of this sort in connection with his study of clusters.

[1] These PROCEEDINGS 2, 1916, (521–525).
[2] *Astroph. J., Chicago, Ill.*, 39, 1914, (361–369); [*Mt. Wilson Contrib.*, No. 81].
[3] *Ibid.*, 40, 1914, (187–204), 42, 1915, (92–119), (120–132); [*Mt. Wilson Contrib.*, Nos. 83, 100, 102].

TERRACING OF BAJADA BELTS

By Charles Keyes

Communicated by W. B. Clark, December 16, 1916

For the local stream-trenching and the resultant terracing of the higher zones of those long uniform slopes which so often spread out from the foot of desert mountain ranges there is an explanation very much simpler than any of the numerous ones yet offered. It has the advantage of being in strict accord with the regular and ordinary phases of erosional action which recent critical observation shows to be now at work as vigorously and as effectively as they have been in any past period. It is, in effect, nothing more than a reiteration, in a somewhat new form to be sure, of the old law of parsimony which forbids the unnecessary multiplication of explanatory elements and agencies.

In all late physiographical writings in which the term bajada is used it is unfortunately misconstrued. Spanish-speaking Americans do not seem ever to have given the title so broad a meaning as that sometimes attached to it. If the name is to remain a useful geographical term of description it should be allowed to retain something of its original significance, and should be restricted in its application to the steeper slopes of the desert piedmonts. Without exception bolsons appear to present four distinct physiographic areas, or belts, three of which are plains. There is the central, more or less level tract, sometimes covered for a period of a few days or weeks of each year by a

thin sheet of gathered storm-waters—the *playa*. There is next the long smooth slope of low inclination, having about a 2% gradient. This is followed by a short, steeper slope, with a 4% grade, to which the name *bajada* is properly given. Fourth is the mountainous periphery.

In the literature on the arid regions the bajada-belt is usually treated as one of the most conspicuous and important drainage features, as formed by prodigious outwash from the peripheral highlands of basins of centripetal drainage, and as consisting of a series of great coalescing delta-fans. To this interpretation several strong objections arise. Discordant facts greatly outweigh the supporting evidence. The piedmonts of western deserts most frequently described chance to be on the margins of the Great Basin where the lofty Wasatch range and the still loftier Sierra Nevada produce effects which are not at all typical of the true Basin-ranges. In other places the steeper parts of the intermont plains, or bajadas, often mark belts of resistant rocks and are almost devoid of soil. In still other cases the extension of the even-sloping bajadas up into the valleys of mountain-arroyos is manifestly the direct result of rapid and tremendous drifting of soils from the lowland plains, rather than of the gravitational flow of detrital materials from the adjacent highlands. The volume of finer rock-waste brought down from a desert-range by the infrequent storm-waters is not by any means what might be expected; it is, in reality, phenomenally small. As more fully stated elsewhere the yearly amount washed down by the rains may be swept away by the winds in a single day.

In spite of the fact, then, that the *bajada* is often a belt of thick, adobe soils, of drifting sands, or of sporadic outwash from the nearby mountains, it is also still oftener true that it is an area of the most indurated rock, so free from soil that, as W. J. McGee describes, "the horses' shoes beat on the planed granite, and schist and other hard rocks in traversing the plain 3 or 5 miles from the mountains." Then, again, there are typical bajadas widely separated from desert ranges by deep longitudinal valleys which hug the mountain bases—conditions under which there is no possible chance for the outwash from the highland to reach the plain. The Sandia, Manzano and Caballos sierras, in central New Mexico, are a few of the many notable examples.

The terracing of arroya-courses in the piedmont belt appears usually to be merely the outcome of a vigorous contest which is constantly waged between the local, temporary aggrading of wind-driven soils or sands on the one hand, and on the other hand by the weak degrading action of the infrequently running mountain torrent. The phenomena of bajada-terracing is not, as urged by some physiographers, a necessary

consequence of the general lowering of the highlands by stream-action, while the intermont lowlands are being filled up; because some of the best examples of terracing border broad plains having rock-floors. For the same reason it does not appear possible that there ever occurs during so-called topographic maturity an adjustment by water action between one bolson and an adjacent lower one which results in the terracing of the higher. There is little or no actual evidence to show that bajadas were all formed during periods of glaciation; since some of the most typical expressions of these sloping plains are found surrounding low knolls near sea-level, and far below all possible altitudes of glacial action in the region. Neither does it appear likely that bajadas were built up during interglacial epochs of materials which accumulated in the mountains when the latter were covered with ice; for this does not explain the many cases in which rock-floors are present. Nor is it any better to postulate a recent increase of temperature and a different distribution and amount of rain-fall abetted by the advancement of the area in the geographic cycle; for terracing is now going on before our very eyes at an astonishingly rapid rate, and as quickly is it also completely obliterated. In many localities over-grazing is manifestly a potent and direct cause of the tremendous recent trenching and corrading by sporadic storm-waters of the soft temporary soil accumulations in the desert.

In recently setting forth reasons for believing that the gradational effects displayed by the intermont plains of arid regions are mainly accomplished by means of the winds, I have attempted to point out the fact that the action takes place chiefly uphill instead of gravitationally down-stream as in the case of running waters. I have also endeavored to emphasize the point that the relatively steep slope of the bajada-belt represents the highest possible wind-gradients, just as the river-bed in a peneplain approaches the lowest possible water-gradient. This statement appears to be amply supported by the results arising from the artificial diversion of arroyo-courses over the smooth bajadas.

Were the leveling tendencies of the winds wholly absent from the desert regions it is quite possible that the corrasive effects of what desert waters there are would be much the same as they are in humid lands, differing only in degree. This is well shown in the cases of wing-dams that have been constructed to protect lines of railway from the disasters of the flood-sheet and the latter has come before the earth-works have had time to be leveled by the winds. In one instance in particular the culvert and track were washed out in less than an hour's time, and a canyon, 75 feet deep, 50 feet wide and several miles long,

was excavated in the smooth surface of the sloping plain. By the time a permanent bridge was built to span the deep trench the winds had filled the entire excavation, so that where a yawning chasm had been was again as smooth as the rest of the plain, and the wing-dams also had melted down into the general evenness of the desert's surface. For several years, until it was finally replaced by an earthen grade, travelers were wont to express great wonderment at the possible utility of so fine a steel bridge resting on the smooth sands of the desert plain.

The Socorro arroyo, in central New Mexico, presents another pertinent case. There is water running in the shallow wash once or twice a year, the supply coming off the lofty Magdalena peak 20 miles away. For many years this arroyo, which divides the town of the same name, has given the residents an infinite amount of trouble. That its 2% grade really produces torrential conditions when the waters do run is indicated by the fact that the arroyo-bed is composed largely of pebbles and boulders, many of the latter attaining a size of 2 feet. In order to obviate the yearly inconveniences of flooding it was determined, a few years ago, to divert the channel 4 miles above the town. This change of course was accomplished by cutting a narrow trench from the bed of the water-way through its bank to a point some 50 yards to one side, where the general plain was slightly lower than the bed of the wash at the head of the ditch. A low dam was thrown up obliquely across the arroyo by piling up boulders from the bed. The theory was that the first water coming down the wash would flow out the ditch, or spill-way, and there would soon cut a deep channel; and that eventually this would carry away all of the future flood-waters. Results more than fulfilled expectations. The first time the dry creek became a brook there was trenched in a single night a chasm 50 feet deep for a distance of more than a mile down the slope of the plain. The materials from the great artificial canyon spread out over the railroad tracks 3 miles away to a depth of 7 feet and to a width of half a mile, necessitating the rebuilding and raising of the grade for a distance of several miles. These two illustrations might be infinitely repeated.

Observations such as these demonstrate beyond a shadow of doubt that whenever around the desert ranges there are alluvial fans, or accumulations of unindurated deposits, it is possible at any time for profound and rapid dissection to take place through means of the copious but infrequent storm-waters. The recent notes of C. L. Baker, A. C. Trowbridge and others on the eastern slopes of the Sierra Nevada, amply confirm these conclusions. Since, however, the restricted areas in which the latter investigations were undertaken lie at the foot of

lofty ranges and border a region of moister climate the action of running waters is more nearly normal than in the Great Basin proper. Yet, in the interior of the latter the same phenomena are also well displayed. Around the southern rim of one remarkable desiccated Las Vegas, in southern Nevada, in the low Spring, Newberry and Eldorado ranges, the bajada is frequently dissected and terraced in a singular manner. So extensive is it that it may be clearly distinguished even at a distance of 10 or a dozen miles. Dissection and terracing are also everywhere apparent around the region of the excessively dry basins of Death valley, the Armagosa plains and the Mojave desert.

In this connection there is one circumstance which not only A. C. Trowbridge and C. L. Baker appear to have overlooked, but likewise F. L. Hess, J. E. Spurr, G. K. Gilbert, H. W. Fairbanks and other earlier writers. In the descriptions of the terrestrial deposits the origin of the latter is ascribed entirely to water-action. No account seemingly is taken of possible assistance of wind-action in piling up locally these masses of debris. Photographs of the region, which the authors named reproduce, display unmistakable signs of plenty of wind-work. On this point direct personal observation is even more conclusive. In the building up of the so-called alluvial fans and of the bajada when composed of fine materials the winds appear to be the controlling power. Arroyo-waters seem mainly to be merely modifying agents, supplying some coarse rock-waste from the mountains, but largely locally turning back the materials brought in up-grade from the lowland plains. The effects simulate the alluvial fans of humid regions; but they are not by any means their exact counterpart.

After the lower reaches of the canyons, immediately before they debouch upon the plains, become over-filled in the course of a few months or a few years, with the wind-driven sands and dusts and are eolicly aggraded they are readily dissected and even terraced by the first appearance of heavy storm-waters on the mountains. For accomplishing these results the time-element is certainly not so interminably long as has been commonly supposed. It is not necessary to stretch it back to the Glacial period, and far beyond. It is not to be gauged by tens of millenia. Its span is to be measured not even by years, but by months or weeks. It is known to have been limited by a single rising and setting of the sun.

The local dissection and terracing of the so-called alluvial fans in arid regions may have a significance still broader than that commonly ascribed to it. In the explanation of these phenomena we may have the key to the specific method by which general leveling and lowering of

desert lands are accomplished. Neither feature is confined to areas which are situated at the mouths of canyons. Both are displayed in bajada belts where rock-floors are present and where the once even surfaces are worn out on the beveled edges of tilted strata. The Calico range, in the Mojave desert, north of Daggett, California, is a notable but not an isolated example. No eminence of the Great Basin region appears, at first glance, to be more certainly a 'lost mountain,' a lofty range buried up to its shoulders in its own debris. The bajadas on either side of the ridge all but meet over its summit. So low and rounded is the crest that manifestly there is no opportunity for extensive outwash around the borders. There is, in this instance, not only remarkable dissection of the bajada belt taking place at the present time but a widening of the apparent lines of drainage into wide flat-bottomed esplanades with deep reentrants. Elsewhere there is the anomaly of a long sinuous terrace several hundreds of feet in height separating the higher general plains-surface from the lower local plains-level. In this we get a glimpse of the formation of those heretofore inexplicable but characteristic desert features known as plateau-plains. In their last stage the isolated Tówa-yal-lané, Acoma and Chupadera mesas, of New Mexico, are conspicuous illustrations. To this phase of the problem attention is later turned.

The feature of desert bajada-terracing, when explained upon a strictly aqueous basis, cannot but lead to complete misinterpretation. The phenomenon has no necessary connection with former and greater stream-activity. It is one of the wide-spread characteristics of desert lands. It is far more largely the result of wind-action than of water-action. Its marvellous aspect is the great rapidity with which it takes form. It is, in reality, one of the subordinate expressions of regional eolation.

RELATION OF THE APEX OF SOLAR MOTION TO PROPER MOTION AND ON THE CAUSE OF THE DIFFERENCES OF ITS POSITION FROM RADIAL VELOCITIES AND PROPER MOTIONS

By C. D. Perrine

OBSERVATORIO NACIONAL ARGENTINO, CÓRDOBA

Communicated by E. B. Frost, November 27, 1916

Continuing the investigation of the apparent dependence of the position of the solar apex upon proper motion as derived from radial velocities,[1] apices have now been derived from the proper motions

of the same stars which confirm the conclusions from the radial velocities. Solutions were made also from Adams' Mount Wilson list of 500 radial velocities[2] which show the same dependence. This list, however, contains no stars south of $-26°$.

The results from both sets of data are given in table I.

TABLE I

CLASS μ_α	RADIAL VELOCITIES			PROPER MOTION		P.M.—R.V.
	A	D	No. stars	A	D	Δ D
	degrees	*degrees*		*degrees*	*degrees*	*degrees*
2.9 and brighter. All spectral classes	258.0	+41.5	110	258.8	+24.6	−16.9
B	276.0	+29.6	193	272.8	+32.4	+ 2.8
A, F, G						
Large	268.1	+ 8.6	141	261.3	+22.1	+13.5
Medium	269.0	+ 7.0	81	238.5	+50.8	+43.8
Small	254.7	+35.0	277	263.5	+47.4	+12.4
K						
Large	288.7	+18.8	85	255.9	+13.0	− 5.8
Medium	250.4	+15.1	85	258.5	+42.9	+27.8
Small	260.4	+36.8	220	276.4	+69.2	+32.4
Mean	265.7	+24.0		260.7	+37.8	+13.8
Adams' 500 R. V.						
Large	276.9	+ 3.1	47	279.2	+38.4	+35.3
Medium	244.2	+41.6	32	249.6	+50.3	+ 8.7
Small	272.0	+31.6	349	266.4	+67.7	+36.1

These results also show systematic differences between the positions of the apex of the kind noted by Campbell[3] between his apex from 1193 radial velocities and that of Lewis Boss from 4686 proper motions. These discordances are chiefly in declination and are given below according to spectral class in the sense P.M.—R.V.

SPECTRAL CLASS	Δ D	SPECTRAL CLASS	Δ D
	degrees		*degrees*
B	+ 5.3	G	+22.1
A	+13.0	K	+14.7
F	+17.6	M	+ 7.1

There is some indication that the discordance is greater for the later spectral types, at least among the stars with small proper motion in right ascension, and possibly also some relation to magnitude and size of proper motion.

Asymmetry in the proper motions of the B stars and other peculiari-

ties prompted the making of separate solutions from northern and southern stars, the results of which are given in Table II.

TABLE II

Proper Motions

Spectral class μ_α	North					South					Δ D
	A	D	X	Y	Z	A	D	X	Y	Z	N−S
		(all +)		(all +)	(all −)		(all +)		(all +)	(all −)	
2.9 and brighter											
All classes.....	260°0	26°0	+27.3	155.3	77.1	255°4	21°6	+ 26.6	101.8	41.6	+ 4°4
B.............	281.5	46.4	− 3.3	16.3	17.4	269.4	26.2	+ 0.4	34.5	17.0	+20.2
A, F. G											
Large.......	258.0	35.6	+48.0	228.0	167.0	263.7	11.2	+ 39.0	356.0	71.0	+24.4
Medium......	256.3	36.8	+ 6.3	25.8	19.9	227.9	(56.6)	+ 29.8	32.9	67.3	(−19.8)
Small........	262.2	59.4	+ 2.4	17.2	29.4	264.5	40.1	+ 3.2	33.6	28.5	+19.3
K											
Large.......	255.4	17.9	+46.0	178.0	59.0	256.9	3.9	+ 31.0	131.0	9.0	+14.0
Medium.....	269.8	46.4	+ 0.2	58.9	61.8	245.5	35.9	+ 14.6	31.9	25.4	+10.5
Small........	267.6	72.4	+ 0.5	12.2	38.8	282.2	66.2	− 3.2	14.7	34.1	+ 6.2
Mean.........		42.6					32.7				+ 9.6
Omitting ( )...							29.3				+13.8
Adams' 500 R.V.											
Large.......	279.8	33.6	−56.0	323.0	218.0	275.8	35.9	− 63.0	626.0	455.0	− 2.3
Medium......	281.9	35.4	− 8.1	38.2	27.7	202.8	18.7	+115.2	48.5	42.2	+16.7
Small........	279.4	70.7	− 1.2	7.2	20.9	230.9	50.9	+ 6.0	7.4	11.7	+19.8
π Obs. 0".06 and over.....	253.7	47.9	+97.0	332.0	383.0	258.0	24.9	+186.0	871.0	414.0	+23.0

These results disclose, if we omit one abnormal southern D, quite consistent differences similar to those between the results from radial velocity and proper motion for all portions of the sky. From these 1194 stars of the spectral classes B, A, F, G and K, the average of the apices from the proper motions of the southern stars agrees closely with the apices from the radial velocities of the same stars, but there is a difference of over 20° for the northern stars, as follows:

	D	
	Northern	Southern
	degrees	degrees
Proper motions..	+43	+29
Radial velocities..	+21	+26
P.M. − R.V..	+22	+ 3

An examination of the rectangular coördinates resulting from the solutions (Table II) shows that nine out of the entire twelve values of Y and all of the values from the small μ_α, are smaller for the northern than for the southern stars, indicating smaller average parallactic displacement for the northern than for the southern stars.

Taking the stars of type B as the best marked example, if we use the value of Y derived from the *southern* stars to determine D for the *northern* stars, we obtain essentially the same D for both northern and southern stars, a value which agrees well with that obtained from the radial velocities of the same stars and with that from the radial velocities of all of the spectral classes. This will be true in principle also for the other cases, but appears to be complicated in the later types by other factors.

Summary.—1. The position of the apex of solar motion depends upon the proper motions in right ascension of the stars used. The differences appear to be greatest in declination, the stars of large proper motion yielding apices south of those from stars with small proper motion. This effect is shown in the results both from proper motions and radial velocities.

2. The differences in D of the apices of solar motion as derived by other investigators from radial velocities and from proper motions are consistent and they appear in general to be greater for the stars of late than for those of early type. This discordance appears to arise chiefly from the proper motions of the northern stars and to be satisfactorily explained by the assumption that the parallactic displacement of the stars is systematically less in the northern sky.

The details of this investigation have been sent to the *Astrophysical Journal.*

[1] These PROCEEDINGS, 2, 1916, (376–378); *Astroph. J., Chicago, Ill.*, 44, 1916, (103–116).

[2] *Contrib. Mount Wilson Solar Obs.*, No. 105.

[3] *Berkeley, Lick Obs., Univ. Cal., Bull.*, No. 196, p. 128.

HYDROLOGY OF THE ISTHMUS OF PANAMA

By Brig. Gen. Henry L. Abbot

UNITED STATES ARMY, RETIRED

Read before the Academy, November 13, 1916

The Panama Canal being now opened to traffic, there remains for study only one important hydraulic problem—the sufficiency of the available water supply to meet the needs for lockage, for mechanical

power to operate the canal and railroad, and for the electric lighting of the Canal Zone. It should not be forgotten, however, that if a larger volume of water be desirable for these and other uses, the plan proposed by the New French Company to supplement the volume of its smaller projected lake is still available; namely, the construction of a masonry dam near Alhajuela, where a good site exists for creating an upper lake to hold back the surplus water which during the rainy months now runs to waste through the spillway.

The Canal Zone lies between the ninth and tenth degrees of north latitude in a region of exceptional rainfall; where the sun, closely followed by rain clouds raised from the oceans in his annual journey between the tropics, exerts a controlling influence upon the volume of local rainfall. When near his southern limit, in January, February, March and April, precipitation upon the Isthmus is at its minimum; December and May are usually intermediate in volume; in the remaining six months, when near his northern limit, heavy downpours are the rule. Furthermore, the local annual rainfall is not uniform across the Isthmus. As one passes from the Atlantic to the Pacific coast, the volume falls off gradually from about 130 inches at Colon to about 70 inches at Panama. It will be noticed that even the latter is more than double the usual downfall in the United States, a fortunate circumstance for our great artery of commerce. Another local advantage is the fact that the atmosphere of the Isthmus is nearly saturated with aqueous vapor, which largely reduces the losses by lake evaporation. Our hydraulic problem seems therefore to be specially concerned with Isthmian rainfall and outflow, and the relation between them.

To determine accurately the average annual rainfall at any locality, the records should cover at least half a century. Although the Isthmus has been known to civilization for more than four hundred years, the first annual rainfall records date from 1863, when they were begun by the agents of the Panama Railroad Company. As to the outflow from the watershed, the records are even less complete. The volume received from the clouds, after reduction by evaporation, by plant growth, and by possible infiltration, represents the available flow at the dam site. To determine to what extent existing data throw light upon this quantity, has seemed to me to be of primary interest.

The earlier records are given in a paper published in the *Monthly Weather Review* in May, 1899; they include those of the Railroad company, and those at that date collected by the two French Companies and the Liquidation. A gap of a few years in the former is supplied by a paper by Mr. A. P. Davis, published in the Twenty-second Annual

Report of the United States Geological Survey for 1900–1901. These data are supplemented and discussed by the writer in papers published in the *Monthly Weather Review* of February, 1904, and of June, 1908, together with some later data collected by the American engineers. In March, 1913, Mr. Caleb M. Saville contributed a valuable paper upon the Hydrology of the Canal to the Transactions of the American Society of Civil Engineers, bringing the records up to 1910. The Annual Reports of the Isthmian Canal Commission, of course, cover the American operations. The first point for consideration is, how these data can best be grouped for study?

The narrow limits to which the earlier observations were restricted suggest that the basin of the Chagres River above Bohio should be adopted until Gatun Lake began to fill early in 1910. Its area has been accurately determined by recent surveys to be 779 square miles. The early rain records were mostly restricted to Colon and Gamboa. Fortunately, Colon is situated near the Atlantic Coast line where the rainfall is largest, and Gamboa well represents the Pacific limit of the watershed. A careful analysis of the more ample records of recent years (1898–1907) has shown that the average rainfall in the basin above Bohio is about 89% of that at Colon, 124% of that at Gamboa, and 52% of the aggregate of the two stations. To avoid a change in the standard, these ratios have been used throughout the following table to estimate the annual rainfall in the basin; and where a few dry months are missing in the early railroad records the vacancies have been supplied by the mean values of the missing months.

The Chagres River is a torrential stream, and the first French Company early established a fluviograph at Gamboa to register continuously the heights of the water. This record has been carefully kept since 1883; until the rise of the lake in 1910 began to affect the local water level. At the times of freshets the oscillations are so sudden that their number and duration are readily noted, and they furnish valuable checks upon the discharge estimates. For this study they are assumed to begin and end at a stage 10 feet above low water, which is rarely exceeded.

Accurate measurements of discharge were inaugurated by the Liquidation and the New French Company, the continuous record dating from 1890. The annual outflow is given in the tables under the forms of cubic feet per second and depth in inches upon the watershed; the latter to permit a direct comparison with the rainfall. Years of great floods are also indicated, with the same object in view.

The first table is intended to present, for the basin above Bohio, all

available hydraulic data prior to the date when Gatun Lake began to fill, with some later figures added for comparison with those of the second table, which includes the entire watershed above Lake Gatun during the filling of the lake. In future studies this latter basin will doubtless be adopted, as the net outflow is easily determined and rainfall measurements are now made at many stations. For the second table I first[1] adopted the mean of the rainfall records at nine stations—Colon, Gatun, Trinidad, Camacho, Empire, Culebra, Gamboa, Alhajuela and El Vigia—as well representing the average volume falling upon the lake watershed near the Canal Zone. Seven new stations then unknown to me had been added, and the figures now given are taken from the noteworthy paper by Mr. Willson in the *Transactions of the International Engineering Congress at San Francisco.*

HYDROLOGY OF THE WATERSHED ABOVE BOHIO; 779 SQUARE MILES

YEAR	PRECIPITATION		IN THE BASIN ABOVE BOHIO			GAMBOA FRESHETS		AUTHORITY
	Colon Inches	Gamboa Inches	Rainfall Inches	Outflow		Annual No.	Duration Hours	
				Ft.-sec.	Inches			
1863	134.3		120					Panama Railroad Records
1864	123.4		110					
1865	107.4		96					
1866	129.7		115					
1867	120.8		108					
1868	120.0		107					
1869	114.8		102					
1870	149.6		133					
1871	99.6		81					
1872	168.5		150					
1873	87.1		78					
1874	137.7		123					
1875	94.7		84					
1876	124.5		111					
1877	115.5		103					
1878	86.7		77					
*1879	164.2		130					
1880	136.4		121					
1881	102.3	89.5	100					de Lesseps French Cie
1882	124.1	79.6	106					
1883	115.3	76.6	100			21	201	
1884	86.5	95.9	95			25	334	
*1885	146.3	97.5	127			37	437	
1886	137.2	102.9	125			43	437	
1887	154.9	136.2	151			46	709	
*1888		102.6	127			29	596	

* Great flood of the Chagres River.

HYDROLOGY OF THE WATERSHED ABOVE BOHIO; 779 SQUARE MILES—*continued*

YEAR	PRECIPITATION		IN THE BASIN ABOVE BOHIO			GAMBOA FRESHETS		AUTHORITY
	Colon Inches	Gamboa Inches	Rainfall Inches	Outflow		Annual No.	Duration Hours	
				Ft.-sec.	Inches			
1889		75.7						Collapse. Liquidation.
*1890	154.3	105.0	135	6,304	110.0	34	308	
1891	124.7	77.7	105	4,476	78.1	10	115	
1892	145.3	104.4	130	6,513	113.6	24	269	
*1893	131.9	117.8	130	7,081	123.3	12	220	
1894	153.7	90.6	127	6,098	106.4	25	263	
1895	151.5		135	4,482	78.2	7	54	The New French Cie
1896	131.5		117	4,216	73.5			
1897	138.0	107.8	128	4,830	84.2	16	157	
1898	115.5	82.6	103	3,944	68.8	8	51	
1899	133.0	80.0	111	3,384	59.0	8	57	
1900	116.1	78.7	101	3,509	61.2	12	84	
1901	107.7	91.6	104	3,855	67.2	13	107	
1902	112.6	97.7	109	4,179	72.9	5	37	
1903	126.3	99.3	117	3,958	69.0	12	94	
1904	126.9	84.5	110	4,110	71.7	13	94	U. S. in charge.
1905	115.4	82.2	103	2,800	48.8	6	48	
*1906	138.1	97.8	123	4,169	72.7	12	178	
1907	125.6	78.1	106	3,597	62.7	11	69	
1908	137.7	77.5	112	3,732	65.1	14	131	
*1909	183.4	122.1	159	7,335	128.0	27	466	
1910	150.0	116.0	138	6,340	110.6	15	77	Lake began to fill
1911	112.8	70.6	95					
1912	117.6	89.1	107					
1913	131.2	86.3	113					
1914	132.7	77.4	109					

* Great flood of the Chagres River.

HYDROLOGY OF THE WATERSHED ABOVE LAKE GUTUN; 1,320 SQUARE MILES

YEAR	LAKE ABOVE MEAN SEA LEVEL FEET	IN THE LAKE WATERSHED			LAKE EVAPORATION. FOOT-SECONDS
		Rainfall inches	Net outflow		
			Foot-seconds	Inches	
*1909	3.63	162.42	10,704	110.1	
1910	13.07	149.66	11,938	122.9	
1911	15.15	98.41	5,710	58.8	
1912	31.24	102.83	4,985	51.3	
1913	57.87	102.40	5,272	54.3	484
1914	85.26	100.54	5,118	52.7	704
1915	86.17	118.17	7,106	80.2	733

* Great flood of the Chagres River.

An inspection of these tables shows a satisfactory correspondence between the observed annual rainfall and the outflow and river oscillations as indicated by the number and duration of the freshets. There is, furthermore, a suggestion of a tendency to a progressive annual change between years of maxima and minima in these quantities, which is worthy of notice. If future records confirm this suggestion, it will be needful to study the reason for it, as is now the case with solar spots, aurora borealis, and other natural phenomena. As to the second table, it must not be forgotten that it covers a period when the area of the lake was gradually increasing, causing a variable loss due to evaporation. The values given for it for the last three years are taken from the official reports, and are based probably on direct measurements.

The tropical conditions of the Canal Zone are so different from those in the Continental United States that it is interesting to compare them. This may be done from Rafter's tables, in his valuable paper published as the Water-Supply and Irrigation Paper, No. 80, of the Geological Survey. It is done in the following table.

Average Rainfall, Run-off, and Difference

Watershed	Area Square Miles	Period Years	Rain-fall Inches	Losses			
				By Outflow		By Evap. Etc.	
				Inches	Per cent	Inches	Per cent
Muskingum River	5,828	8	39.7	13.1	33	26.6	67
Genessee River	1,070	9	40.3	14.2	35	26.1	65
Croton River	339	23	49.4	22.8	46	26.6	54
Lake Cochituate	19	38	47.1	20.3	43	26.8	57
Sudbury River	78	26	46.1	22.6	49	23.5	51
Mystic Lake	27	18	44.1	20.0	45	24.1	55
Neshaminy Creek	139	16	47.6	23.1	49	24.5	51
Perkiomen Creek	152	16	48.0	23.6	50	24.4	50
Tohickon Creek	102	15	50.1	28.4	57	21.7	43
Hudson River	4,500	14	44.2	23.3	53	20.9	47
Pequannock River	64	9	46.8	26.8	57	20.0	43
Connecticut River	10,234	8	43.0	22.0	51	21.0	49
Chagres River	779	21	118.1	82.1	69	36.0	31
Gatun Lake Basin	1,320	6	119.4	75.0	63	28.3	24

An inspection of the last four columns will show how much more favorable to the operation of a canal are the hydraulic conditions prevailing upon the Isthmus than those existing near the Atlantic Coast of the United States. Considerably more than double the volume of rainfall is available, and the losses from evaporation, plant growth,

and percolation are represented by only about 30% as against 50% of that volume. As stated above, it remains to be determined by the observations of many future years whether there is a tendency to a normal variation in annual rainfall upon the Isthmus, as seems to be suggested by the figures already of record.

[1] In a paper which appeared in *Professional Memoirs, Corps of Engineers, U.S.A.*, for November-December, 1915.

THE METEOR SYSTEM OF PONS-WINNECKE'S COMET

By Charles P. Olivier

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Communicated by E. W. Morley and read before the Academy, November 14, 1916

The National Academy of Sciences, by two grants from the J. Lawrence Smith Fund, having been the means of greatly extending the membership and work of the American Meteor Society, it seems well to report to the Academy the most interesting single result of the work so far obtained.

Late in May and early in June of this year two of our observers, Mr. John Koep and Mr. Philip Trudelle, both of Chippewa Falls, Wis., sent in a series of observations which showed that meteors were at least three times as numerous as is usual for the time of the year mentioned. This attracted immediate attention and the observations were worked up without delay. The orbits, which corresponded to the positions found for the radiants, were then computed. As several of the orbits turned out to have the same elements, a search was next made to see if any of the known comets had a similar orbit.

At once it was seen that Pons-Winnecke's Comet fulfilled the conditions excellently and there was no doubt that the meteors and the comet had a common origin, and that another case of the connection between a meteor stream and a comet had been found.

The first orbits for the meteors had been computed on the usual assumption that they moved with a parabolic velocity, but there were obvious reasons to show that this was only a first approximation. The orbits were then all computed as elliptical, assuming their major axes equal to that of the comet. This set made a far better agreement with the elements of the comet's orbit and put the connection beyond question.

Before speaking further about the meteors themselves, it will be of interest to review briefly the history of this rather remarkable comet. Discovered by Pons in 1819, no further observations were obtained until

Elliptical Elements							Parabolic Elements									
No.	Log a	Log e	Log q	i	P	N	No.	Date		R A	Decl.	i	Log q	P	N	Obs.
1	0.514	9.857	9.962	16.7	280.1	60.7	1	May	21.6	224.5	25.2	20.2	9.954	279.7	60.7	J. K.
2	0.514	9.865	9.948	18.6	287.6	65.5	2		26.68	230.3	27.4	22.4	9.958	283.4	65.5	P. T.
3	0.514	9.854	9.970	17.6	282.6	66.1	3		27.25	231.0	27.5	22.5	9.958	284.2	66.1	J. K.
4	0.514	9.856	9.964	19.0	286.2	66.5	4		27.67	232.1	26.8	22.8	9.954	285.4	66.5	P T.
5	0.514	9.855	9.996	19.1	285.6	68.0	5		29.2	232.7	28.0	22.9	9.960	284.9	68.0	J. K.
6	0.514	9.852	9.974	17.6	287.6	73.2	6	June	3.7	234.4	27.5	22.2	9.965	288.4	73.2	J. K
7	0.514	9.855	9.967	18.2	291.6	74.2	7		4.68	235.8	25.6	22.0	9.961	291.0	74.2	P. T.
8	0.514	9.839	0.005	17.1	268.3	96.9	8		28.5	203.0	53.0	20.9	0.005	268.3	96.0	B
Comet	0.514	9.846	9.988	18.3	271.6	99.3										

1858 when it was rediscovered and named after Winnecke. Since then it has been seen in 1869, 1875, 1886, 1892, 1898, 1909, and 1915. While it has never been a bright comet, at its last return—a rather unfavorable one it is true—it never surpassed the twelfth magnitude in brightness and was more than a unit's distance from the earth. The comet belongs to Jupiter's family and at present has a major axis of about 6.52 astronomical units and a period of 5.89 years.

Since at certain epochs it has passed very near Jupiter, the most massive of the planets, the elements of its orbit have been changed in a most marked manner due to perturbations caused by that body. Briefly from 1858 to 1909 its longitude of perihelion has changed from 276° to 272°, its ascending node from 114° to 99°, the eccentricity of its orbit from 0.76 to 0.70, and finally its perihelion distance from 0.70 to 0.97 astronomical unit. This last change is the one of greatest importance for the present discussion, because it made possible the intersection of the earth's orbit with those of the meteors connected with the comet.

It will be seen that this element was slowly increasing during this interval of 51 years, and hence, if the progression continued in the same direction, in the seven years since 1909 it should have reached the value unity almost exactly, which means that the nodal point was indeed very near to the earth when the latter passed by it, late in June. While the inclination of the comet's orbit has increased from 11° in 1858 to 18° in 1909, still the extreme distances between the earth and the comet's 1909 orbit was only about 14,000,000 miles on May 25, and this decreased to 2,000,000 on June 28. Even this first figure is not excessive, because we already have positive proof that members of the Perseid and Aquarid streams have been observed at about that distance from the orbit of the parent comet.

The longitudes of the nodes differ considerably. This is due to the fact that this element of a meteor's orbit depends merely upon the position of the earth in its orbit, and when the agreement of the other elements is satisfactory, considerable differences in the node mean little. It will be seen that the other elements agree very well indeed, remembering the limit of accuracy possible in work on meteors, which is never comparable in this regard to results obtained with the telescope.

The data on which final conclusions were based are rather extensive. For America we had about 1100 observations reported by five of our members during the interval these meteors were seen, and three English observers also published results about the time of maximum, which are available. Eight orbits were calculated from this material, a number quite sufficient to illustrate the general agreement and order of accuracy of the work. Several others have since been computed. Of course in the final discussion all the data will be included and published in full detail.

While the working up of the American observations and the conclusions drawn from them are of course my own, yet I desire to make full acknowledgment to Messrs. Koep and Trudelle, through whose enthusiastic and excellent observing my part was made possible. Their work is of a high order, and it may be said that in 1916, from January to September, Mr. Koep sent in about 1800 observations and Mr. Trudelle about 1330.

It is only just to say that Mr. W. F. Denning of Bristol, England, observed these meteors and later published his observations, saying that the meteors moved in orbits somewhat similar to that of Pons-Winnecke's comet and they might possibly be connected. He gave no elements nor did he state on what sort of investigations his inference was based.

However, he published this before I sent my results to Harvard College Observatory, for publication in one of its *Bulletins*, but the journal containing Mr. Denning's results had not then arrived from England and I was ignorant of his conclusions.

Finally the members of the National Academy of Sciences will be interested to learn that since the first grant from the J. Lawrence Smith Fund became available last year no less than 5000 observations of meteors made by members of the American Meteor Society were sent in during 1915, and 7500 more were received up to October 1 of this year.

IMPROVEMENTS IN CALORIMETRIC COMBUSTION, AND THE HEAT OF COMBUSTION OF TOLUENE

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Read before the Academy, November 14, 1916

The object of this investigation, which is part of a much larger program, was to secure further more precise knowledge of the heat of combustion of typical compounds of carbon, and further development of the methods of determination.[1] The work herein described followed directly after that detailed in the recent communication published with Dr. Frederick Barry; and the methods and apparatus resembled in most respects those already explained. Having profited by the experience of the earlier work, we were able to improve upon some of its details. Especial emphasis will be laid upon the improvements.

The method consisted in the successive combustion of toluene and a standard substance in the Berthelot bomb as modified by Atwater and Benedict, in oxygen under about 22 atmospheres pressure. The rise of temperature of the colorimeter containing the bomb was paralleled in the environment, so that no correction for cooling was needed. That this adiabatic method is capable of giving excellent relative results is shown by the series of investigation conducted at Harvard University; its absolute accuracy is proved by the recent careful work of the Bureau of Standards by H. C. Dickinson and his assistants.[2] The general assemblage of apparatus is adequately described and depicted in the most recent of the preceding papers, and the reader is referred to these as regards minutiae.

The details in which improvements were instituted were as follows: the mode of sealing the bomb, the mode of providing for the well regulated and complete combustion of the volatile substance; the mode of ignition; the automatic control of the temperature of the environment by a special 'synthermal regulator;' and the analysis of the residual gases for traces of unburned carbon monoxide. These several topics are discussed in order below, and finally the results for naphthalene and toluene are given. Other substances also were burned, but the details concerning these will be reserved for another communication.

The closing of the bomb.—The bomb was sealed by a washer of lead, sunk in a suitable circular slot and covered by a continuous round plate of gold foil (0.4 or 0.5 mm. thick) which protected the whole inside of

the cover of the bomb. The foil was soft enough to conform perfectly to the double rim of the lower part of the bomb, without breaking; the lead was soft enough to secure complete closure, and the gold wholly protected the lead. If the gold foil is pressed closely against the steel top of the bomb, there is little danger of its melting and if an accident happens, the foil is readily replaced. Accordingly, this method was uniformly used in the present research.

The detrimental effect of friction of the steel screw-cap against the movable steel cover was greatly diminished by the use of a thin, smooth, flat ring of phosphor-bronze between these two parts. Thus rupture of the platinum lining was avoided, and the closure greatly facilitated.

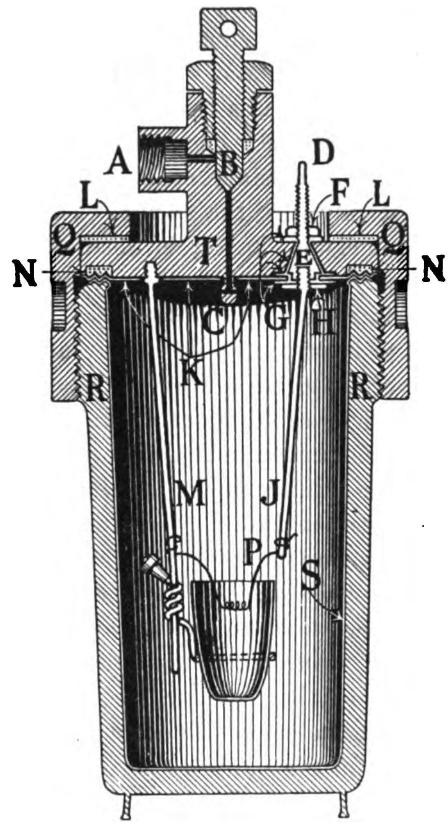

FIG. 1. NEW FEATURES IN CALORIMETRIC BOMB

DE, steel conical plug; *F*, steel nut holding plug in place; *G*, mica insulation; *H*, platinum nut protecting mica and clamping *J*. *J*, platinum rod for filing connection; *H*, gold lining to cover protecting lead washer *N*; *L*, phosphor-bronze ring to prevent friction of screw-cap *Q* on steel lid *T*.

The insulation of the wire for conveying the current necessary for the ignition was improved by making the conical piece (passing through the cover) of steel, not platinum, and insulating this from the cover by a thin layer of mica. A stout thread and nut, likewise insulated, on the outside, permits forcing the strong steel cone so firmly into place that the arrangement is wholly gas-tight. Into the base of this cone a thread is tapped for the stout platinum wire used to conduct the current, and the iron is protected by a wide platinum nut, which is insulated from the cover by mica. Thus the platinum wire is very firmly held in place and yet may be easily unscrewed, and the insulation of the steel cone remains undisturbed indefinitely. Figure 1 records all these improvements.

Other precautions concerning the makeup and treatment of the bomb were similar to those already described in previous investigations.

Mode of enclosing and igniting volatile liquids in thin-walled glass bulbs.—For enclosing the liquids to be burned, very thin bulbs, holding about 1 cc. and weighing about 0.4 g. were blown from a piece of ordinary soft glass essentially in the way illustrated in the preceding communication.[3] These must be much flattened on both sides to give flexibility (necessary on account of the pressure in the bomb), tested by pressing with the fingers. The volume of each bulb is easily found by immersion, and it is filled essentially in the way previously described. The bulb should not only yield under the pressure, but should be thin enough to burst easily. Otherwise the violent explosion might lead to incomplete combustion, and to injury to the apparatus. Typical very thin bulbs filled with benzene or toluene were found to burst with slight explosion at a temperature of about 50° in a water bath—a circumstance which may explain the occasional projection out of the crucible and incomplete combustion of some of the sugar at first used to ignite the liquid. Accordingly, instead of sugar about 0.1 g. of paraffin was used for this purpose, melting it upon the inside of the mouth of the weighed crucible by a hot glass rod outside of the crucible. A weighed fiber (about 0.5 mgm.) of cotton wool was attached to the paraffin, while heated for a moment locally by means of the rod applied outside of the crucible. A minute coil of very fine platinum wire enveloping the projecting cotton served to ignite it and the paraffin. The heat from the burning ring of paraffin ruptured the bulb through the expansion of the liquid, and the vapor of the liquid rose into a ring of flame at the mouth of the crucible and was completely burned. This method is so easy to adjust and is so convenient that it is a distinct improvement over the method formerly used. The result was of course corrected for the accurately known heat of combustion of the paraffin and shred of cotton wool.

Method of ignition.—Iron wire, heated by an electric current to start the combustion, has several disadvantages. The amount of iron burned and the stage of its oxidation are uncertain. Moreover, drops of fused oxide are sometimes driven against the platinum, and frequently destroy the crucible or the lining of the bomb. Very fine platinum wire, used by Zubow and Roth, is decidedly better. In detail the method used by us was as follows: 7–10 cm. of very fine platinum wire were wound in a spiral about 1 mm. in diameter, and attached at the ends of two supports directly above the crucible. As already stated, within the spiral was placed a weighed shred of filter paper or cotton wool,

dipping into the substance to be ignited first, whether this was sugar, napthalene or paraffin. A current of approximately 0.8 ampere ignited the cotton wool in about one-fifth of a second. The variations in the amount of heat obtained from the current in this time are wholly negligible in results like these, where one substance is measured by comparison with another treated in the same way.

The automatic control of the temperature of the environment.—In a recent communication from this laboratory an automatic device, or 'synthermal regulator,' for causing the environment around the calorimeter to match the temperature of the calorimeter itself, is described.[4] The present investigation was well in progress before this synthermal regulator was perfected, and in the meantime we had evolved a quite different much simpler device which served the present purpose sufficiently well, although not so generally useful as the other.

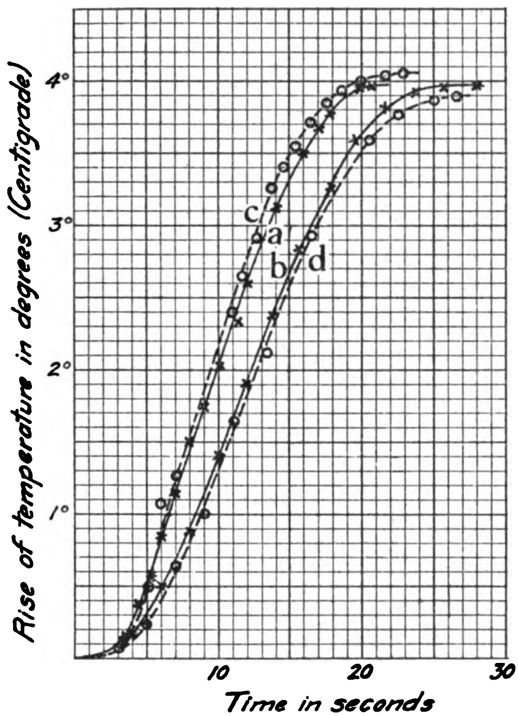

FIG. 2

Continuous lines *A* and *B* represent temperatures in calorimeter as indicated by thermometer in combustions of two different speeds.

Broken lines *C* and *D* indicate rise of temperature in environment as produced by apparatus to be described and illustrated in figure 3.

The general impression seems to exist that combustion within the calorimetric bomb is explosive and instantaneous.[5] According to our experience, this is by no means the case, especially with solid substances. We find that the rate of combustion is very variable, depending partly upon the nature of the substance, partly upon the state of aggregation, and partly upon the oxygen pressure. Thus benzoic acid or napthalene in powder, or a volatile liquid cause the temperature of the calorimetric system to rise very quickly when they are burned; but if the solids are compressed into hard tablets, the temperature rise is slow and steady and may require over three minutes for completion. The dif-

ference can only be accounted for by differences in the time actually required for the burning of the substances in the bomb. This conclusion is entirely in accord with that of Benedict and Fletcher,[6] based upon the measurement of the pressures developed during the combustion of solid substances.

The immediate practical problem was to match this rise in the calorimeter by an equal rise in the environment. Since the rise in temperature of the calorimetric environment, caused by adding sulphuric acid to the alkaline outside bath, is directly proportional to the amount of acid added, a device was needed for delivering very little acid at first, then rapidly increasing the amount, then for some time adding acid at a nearly constant rate, and finally diminishing the stream until it is reduced to nothing, in order to follow the carefully ascertained course of the rise of temperature of the calorimeter. These differences in rate of flow can easily be attained by constructing an apparatus for delivering acid automatically under different pressures. To attain the first part of the curve the level of the acid must run uphill; to parallel the straight line during the rapid rise, the liquid must be delivered from a larger reservoir of nearly constant level; to match the last part of the curve, it must rapidly sink in a fine delivery tube. The necessary dimensions of the several tubes were found by trial, and an apparatus like that shown in figure 3 was found to serve the purpose excellently. The tube to the right hand had to be long and spiral because its inside diameter could not be greater than 4 mm., on account of the small capillary constant of concentrated sulphuric acid.

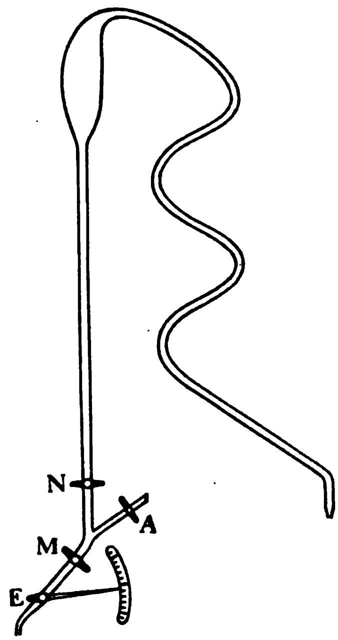

FIG. 3

A, stopcock for admitting sulphuric acid to apparatus in process of filling, *N* being open; *M*, stopcock for starting and stopping flow of acid into tank; *E*, regulating stopcock for determining rate of flow without altering general character of curve.

For each substance with each method one or two preliminary trials determined the general inclination of the curve. Previous experiments with alterations in the stopcock showed the inclination corresponding to each position on its scale. Therefore, for any particular substance, when all was ready for the combustion, the stopcock *E* had simply to

be set at the appropriate point, and at the moment of ignition M and N fully opened. The acid was then delivered automatically at a rate suitable for the case in hand.

Incomplete combustion.—In one of the earlier Harvard investigations the gases remaining in the bomb after the combustion of sugar were repeatedly analyzed for carbon monoxide or other volatile carbon compound, and none was found.[7] Nevertheless, with volatile liquids the danger is much greater, as is indicated by the careful work of Auwers, Roth and Eisenlohr.[8] The final test is the actual analysis of the resulting gases for combustible carbon compounds, especially carbon monoxide. This we have now carried out after many combustions, and can definitely report that with volatile liquids the combustion is often somewhat incomplete.

The arrangement of the apparatus was as follows: To the bomb, after each calorimetric combustion, was connected a train of tubes, as follows: 2 U-tubes filled with concentrated potassium hydroxide solution and glass pearls; a Liebig absorption tube containing saturated barium hydroxide solution; a drying tube filled with phosphorus pentoxide; a tube of copper oxide heated to a dull red heat by electricity; a tube of phosphorus pentoxide, and finally either a Liebig's potash bulb or a spiral filled with a definite quantity of tenth normal barium hydroxide, which was afterwards titrated.

In very many cases considerable quantities of carbon monoxide were thus found, often corresponding to errors of several tenths of a per cent in combustions carried out by the older methods, which seemed otherwise complete. This discovery entirely explains the discrepancy in previous results of many experimenters for benzene. Fortunately, it is only with volatile liquids of this sort that any important incompleteness of combustion was found.

On the other hand, the combustion was found to be fairly complete when volatile liquids were burned by the finally approved method of enclosing the liquids in very thin bulbs ignited by the ring of paraffin.

The gases from a combustion of benzene were also tested for oxidizable carbon compound by bubbling through a weak solution of alkaline potassium permanganate, and for ozone by allowing them to inpinge on starch-iodide paper. Both of these tests gave negative results.

Source and preparation of materials.—Napthalene was obtained from the Bureau of Standards; it had evidently been prepared by them with very great care, gave every evidence of having a high degree of purity, and serves admirably for standardizing calorimetric determinations. We found it much superior to sugar.

Toluene was prepared from commercial pure toluene by shaking with successive portions of sulphuric acid until the acid remains colorless. Further agitation with mercury until it had ceased to pollute a clean mercury surface, washing with water, and many times repeated distillation followed. The fraction which came over in the immediate neighborhood of 110.31° under normal pressure was retained as pure.[9] Several samples from different sources were employed.

There follows the record of the last three (and most successful) determinations of the heat of combustion of napthalene, chosen as a standard substance, and of three determinations of the heat of combustion of toluene, conducted in the thin sealed bulb with a rim of paraffin around the top of the containing crucible, as already described. Since they were thus all made over about the same range of temperature any uncertainty in the calibration of the thermometer largely disappears from the result—nevertheless this was carefully calibrated. These determinations serve to indicate the consistency attainable by use of the precautions just described.

Napthalene

NO. OF EXP.	WEIGHT NAPTHALENE	WEIGHT OF COTTON FOR IGNITION	SUM OF ALL CORRECTIONS	t_1°	t_2°	CORR. RISE OF TEMPERATURE
			degrees	*degrees*	*degrees*	*degrees*
5	1.1537	0.008	0.0220	16.008	20.202	3.616
6	1.1002	0.0095	0.0260	16.036	20.043	3.618
7	1.1023	0.008	0.0222	16.046	20.059	3.615
Average						3.616

Toluene.

NO. OF EXP.	WEIGHT TOLUENE	IGNITION MATERIAL	CORRECTION	CO FOUND	t_1°	t_2°	CORR. RISE OF TEMPERATURE FOR 1 GRAM.
			degrees		*degrees*	*degrees*	*degrees*
6	0.9154	Paraffin 0.0982 Cotton 0.0036	0.4256	None	15.961	19.885	3.822
7	0.9986	Paraffin 0.0953 Cotton 0.003	0.4143	2.0 cc.	15.553	19.782	3.820
8	1.0566	Paraffin 0.0747 Cotton 0.0079	0.3348	1.7 cc.	15.987	20.357	3.822
Average							3.821

Thus 1.0000 g. of napthalene in burning was capable of raising our calorimetric system by 3.616° and 1.0000 g. of toluene 3.821°, (or 1.0567 times as much as the same weight of napthalene) both substances having been weighed in air. According to the careful absolute determination of the Bureau of Standards 1 g. of naphtalene thus weighed evolves 9622 calories (20°) on burning. Hence 1 g. of toluene must evolve 10,168 calories, the toluene being weighed in air. Corrected to vacuum and to 18° this would become 10,158, a value only slightly less than that (10,166) found by Richards and Barry. A part at least of the difference may be due to the different standard of reference used in this case (napthalene instead of sugar). Evidently, the combustions in the earlier Harvard research must have been essentially complete, for such difference as exists is in the direction opposite from that which would have been caused by incompleteness of combustion.

Both results are much higher than that found by Roth and Auwers,[10] who apparently did not test the residual gases for carbon monoxide after combustion, and found the gram of toluene to give only 1.0529 times as much heat of combustion as a gram of napthalene, instead of 1.0567. It is not impossible that either all our toluene contained a slight aliphatic impurity[11] (a rather unlikely contingency because we obtained samples from various sources) or theirs contained traces of some other contamination,—since it is so very difficult to purify completely a compound of carbon, even by countless distillations, from substances with nearly the same boiling point.

Many other determinations of a variety of organic substances have been made here with the help of these improved methods and the matter will be pursued further, with especial reference to purity of materials. The results will be communicated in the near future in a publication of the Carnegie Institution of Washington, to which we are much indebted for generous assistance in this work.

Summary.—In this paper there are described improvements in various details of the procedure of calorimetric combustion, to wit: means of effectively closing the bomb with less risk of injury to the platinum lining and cover; means of burning volatile liquids without loss; a method of automatically controlling the temperature of the environment about the calorimeter so as to make the calorimetric operation more convenient and more truly adiabatic; and means of evaluating the incompleteness of combustion, if any volatile carbon compounds should remain unburned. New determinations of the heat of combustion of toluene are recorded, giving the value 10,155 calories (18°) per gram (weighed in vacuo). This is but a preliminary publication.

[1] For references, see Richards and Barry, *J. Amer. Chem. Soc., Easton, Pa.*, **37**, 1915, (993–1020).

[2] *Washington, D. C., Bull. Bur. Stand.*, 11, 1914, (243).

[3] Richards and Barry, Loc. cit.

[4] Richards and Osgood, *J. Amer. Chem. Soc., Easton, Pa.*, **37**, 1915, (1718–1720).

[5] Stohmann, *J. prak. Chem., Leipzig*, 39, 1889, (514).

[6] Benedict and Fletcher, *J. Amer. Chem. Soc.*, **29**, 1907, (739–757).

[7] Richards, Frevert, and Henderson, *Boston, Mass., Proc. Amer. Acad., Arts. Sci.*, **42**, 1907, (584).

[8] Auwers, Roth, and Eisenlohr, *Liebig's Ann. Chem., Leipzig*, **385**, 1911, (102–116).

[9] Richards and Barry, *J. Amer. Chem. Soc., Easton, Pa.*, 36, 1915, (997).

[10] Roth and Auwers, *Liebig's Ann. Chem., Leipzig*, **407**, 1914, (154, 158).

[11] For example, 2–4 Dimethyl hexane, boiling point 110°, L. Clarke, *J. Amer. Chem. Soc., Easton, Pa.*, 30, 1908, (1148). Octanes have higher specific heats of combustion than toluene. (Richards and Jesse, *Ibid.*, 32, 1910, (292).

THE MASS OF THE ELECTRIC CARRIER IN COPPER, SILVER AND ALUMINIUM

By Richard C. Tolman and T. Dale Stewart

Communicated by R. A. Millikan, November 28, 1916

In a previous article [*Physic. Rev.*, **8**, 97 (1916), these PROCEEDINGS 2, 189 (1916)] we have described some experiments in which a coil of copper wire was rotated about its axis at a high speed and then suddenly brought to rest, the ends of the coil being connected with a sensitive ballistic galvanometer which permitted a measurement of the pulse of current which was produced at the instant of stopping by the tendency of the electrons to continue in motion.

We have continued these experiments making use of three new windings of copper wire, and using two different windings each of silver and aluminium wire. These further experiments were made, not only because it seems desirable to subject so new a phenomenon to a more rigid test, but because it is also desirable to see if the mass of the carrier of electricity is the same in all different metals and how much it differs, if at all, from the mass of the electron in free space.

We now have a record of the results of 624 individual runs made on a number of different coils, using three kinds of wire, two different sizes, and two different kinds of insulating binder to hold the coils in place. The runs were made with various total resistances in the circuit, with various lengths of wire, and at various velocities, rotating sometimes in one direction and sometimes in the other. Not only was the pulse of electricity every time in the direction which would be predicted on the basis of a mobile *negative* electron as the carrier of electricity,

but the experiments have led to concordant results for the mass of this carrier. In free space the mass of the electron may be taken as 1/1845 times that of the hydrogen atom, while we have found for the carrier in copper 1/1660, in aluminium 1/1590, and in silver 1/1540. We hope to construct a new apparatus which will increase the accuracy of measurement enough so that we can make certain whether the mass of the carrier is really larger in metals than in free space.

A more complete account of these experiments has been accepted by the *Physical Review* for publication.

THE SILVER VOLTAMETER AS AN INTERNATIONAL STANDARD FOR THE MEASUREMENT OF ELECTRIC CURRENT

By E. B. Rosa and G. W. Vinal

U. S. BUREAU OF STANDARDS, WASHINGTON, D. C.

Read before the Academy, November 14, 1916

The International Electrical Conference which met in London in 1908 adopted the ampere as the second fundamental electrical unit, the ohm being the first, and defined the international ampere in terms of the electrolytic deposit of silver in the silver voltameter. At the time of this conference it was the opinion of the delegates from this country that the volt should have been chosen in place of the ampere, because the standard cell was more reproducible than the silver voltameter and was the means then as now actually employed (in conjunction with the ohm) for measuring the ampere by the drop in potential method. The decision of the conference was, however, accepted as final, and researches were undertaken in several different countries, and particularly in this country, with the aim of making the voltameter worthy to bear the responsibility imposed upon it by the London Conference. The purpose of this paper is to give briefly the most important results that have been obtained and to show the remarkable agreement of the measurements recently made in the national laboratories of several different countries of the electromotive force of a Weston normal cell, in terms of the international volt as officially defined. This agreement is due to the fact that great advances have been made in our knowledge of the silver voltameter in recent years; and although no adequate specifications have been formally adopted, the methods followed by recent investigators have agreed in essential particulars, although differing in details.

No concrete standard for the ampere, corresponding to the column

of mercury for the ohm or the standard cell for the volt, is possible. The value of an electric current may, however, be conveniently expressed for the purposes of precise measurement in two ways. Either the silver voltameter may be used to calibrate an ampere balance as was done by the British Board of Trade, or the voltage of the Weston normal cell may be determined when a measured current passes through a known resistance. The latter method was adopted by the London Conference, but at that time information was lacking as to the proper value to assign to this cell, which was adopted by the conference in place of the Clark cell.

In 1910 a committee consisting of representatives of the National Laboratories of England, France, Germany, and the United States met in Washington and made careful voltameter measurements to determine the voltage of the Weston normal cell, in the manner agreed upon at London in 1908. The Committee found the value to be very nearly 1.0183 volts at 20°C. This value, recommended by the Committee as the international value of the Weston normal cell, has become the basis of measurement in general use for voltage and (with a resistance) for current measurements also. Although the cell has thus become the practical standard, the voltameter has not lost its importance, for it is the ultimate standard which is to be depended upon in the future to determine whether the standard cells preserved by the various national laboratories are maintaining an unchanging voltage.

The researches on the voltameter which have been made since the international technical committee adjourned have been in part for the purpose of finding out with greater precision (to the sixth significant figure) how close the value for the cell as computed from the voltameter results and the international ohm is to the value 1.0183 volts adopted by the Committee. Although the difference is inappreciable for most purposes, it is desirable that it be determined as accurately as possible. It is also very necessary to have adequate specifications for the voltameter that shall enable us to obtain results in the future comparable with those obtained now. In this work the Bureau of Standards has been able to take an active part. Many sources of error in the voltameter have been discovered and means for their elimination provided, and the voltameter may now be considered capable of being used to check the voltage of the Weston normal cell.

During the course of the work differences of opinion have arisen among some of the investigators in this and other countries, but by co-operative experiments and correspondence practically all of these differences

have been settled so that at present, we believe no important questions are still outstanding. A few of the most important developments during the recent voltameter work may be mentioned.

It was well known before the Bureau of Standards began its work that the deposits in the Rayleigh or filter-paper form of voltameter exceeded those in the porous cup voltameters which were used in series with them. No satisfactory explanation of this difference had been given, and its existence had indeed been questioned. The most recent investigation prior to the London Conference appeared to show that under certain conditions the two forms of voltameter agreed. It was discovered at the Bureau of Standards that the difference could be doubled if the amount of filter paper was doubled and that when the filter paper was wrapped around the porous cup the results were the same as if the porous cup were not present. In short, it was definitely proved that the filter paper itself was a source of serious error in the voltameter. A chemical study showed that the cellulose fibers of the filter paper are slightly oxidized in the air and the minute amounts of oxycellulose are soluble in water. These give rise to strong reducing agents in silver nitrate solutions and colloidal silver is formed. The colloidal particles carry electrical charges much smaller proportionally to their mass than do the univalent silver ions and therefore give rise to deposits in excess of the true deposit according to Faraday's law. They also alter the form and appearance of the deposit.

Methods have been developed at the Bureau for the preparation and testing of silver nitrate of the exceptional purity which is necessary for voltameter work, because very minute impurities produce exaggerated effects in the voltameter deposits. All forms of voltameter (except the filter paper form and forms using linen, cotton, silk, etc., as a septum) are in substantial agreement when used with solutions of the highest purity and with proper precautions against anode slime. The temperature coefficient with pure solutions was shown to be zero as it ought to be. The precaution taken by some observers to soak the silver deposits over night in distilled water to remove the last traces of electrolyte was shown to be harmful because it was discovered that silver in contact with platinum is appreciably soluble in distilled water. This was shown by repeated tests. The silver and the platinum differ slightly in potential so that a current passes from the silver to the platinum through the water. Although this current is very small, it was demonstrated by a sensitive galvanometer and the loss of silver from the deposit was measured.

Another discovery of importance is the alloying effect of the silver and platinum when determining the inclusions of foreign matter in the deposits by the method of heating. After the slight alloying has taken place, the removal of the silver leaves a very thin layer of platinum black on the inside surface of the cup. Unless this is entirely removed its adsorptive properties make the weight of the cup uncertain and its catalytic properties cause the deposition of hydrogen ions when another silver deposit is made so that the amount of silver is not a true measure of the electricity according to Faraday's law.

Various observers in different countries by avoiding these sources of error have obtained very concordant results as the following table shows:

TABLE 1

VALUES FOR THE WESTON NORMAL CELL AT 20°C. COMPUTED FROM SILVER VOLTAMETER MEASUREMENTS

COUNTRY*	FORM OF VOLTAMETER†	VALUE FOR CELL	Δ
			parts in 100 000
United States	Smith	1.01827_4	-0_2
United States	Richards	1.01826_7	-0_9
Japan	Smith	1.01826_9	-0_7
Russia‡	Smith	1.01829_5	$+1_9$
Holland	Smith	1.01826	-1_6
Germany	Kohlrausch	1.01829_0	$+1_4$
Mean		1.01827_6	$\pm 1_1$

* England and France have not published any measurements of this kind since the time of the International Committee. The Observers are: for Japan, Obata; for the U. S., Rosa, Vinal, and McDaniel; for Russia, Mlle. Ferringer; for Holland, Haga and Boerema; for Germany, von Steinwehr.

† The Smith form and the Kohlrausch form are similar in principle. They have no separation between anode and cathode except a glass trap to catch the anode slime, but the construction of this trap is very different in the two forms. The Richards form makes use of a porous cup septum.

‡ This is the mean of four sets of measurements on a particular cell at different periods during several years.

The average deviation from the mean result is only 0.001%. The experimental error involves the errors of both the cell and the standard resistance as well as the numerous sources of error in the voltameter itself. These include the errors of weighing the bowls both before and after the deposit, the errors of timing the deposits, the errors due to fluctuation in the current, and the errors in washing and drying the deposits. In view of these it is remarkable how small the deviations are for the individual countries. These results are the work of investigators

of five nationalities using three different types of instruments, and yet they agree to about 1 part in 100,000.

The national laboratories were in correspondence on the matter of specifications when the war in Europe began. Since then the Bureau of Standards has prepared and published its specifications for the voltameter and it will recommend these for adoption when it is again possible to do so after the restoration of peace.

The specifications which the Bureau of Standards has formulated have been made as broad as is consistent with work of the highest accuracy. They do not specify any particular form of voltameter, but rather the conditions which must be fulfilled by the voltameter. The concentration of the solution is given rather wide limits because it has been found that identical results may be obtained with pure solutions over a considerable range.

An important question as to the purity of the silver obtained has only recently been settled. The impurities are very small, as indeed previous observers, including Van Dijk in Holland, Jaeger and von Steinwehr in Germany, and Boltzmann in Austria, have previously shown. The Bureau of Standards has found the average amount of foreign matter in its standard deposits to be about 0.004%. It is so uniform in the different deposits that the results given by the voltameter as a measuring standard are sufficiently accurate without the necessity of determining the foreign matter in each individual deposit. Without the greatest precautions the error introduced in attempting to determine the inclusions will be greater than the total amount of the inclusions.

In some of the experiments at the Bureau of Standards our absolute current balance has been used in connection with the silver voltameter. This has permitted the determination of the absolute electrochemical equivalent of silver. It was found to be 1.11804 mg. per coulomb, but when corrected for the inclusion of foreign material this becomes 1.11800 mg. This figure is exactly the value assigned to this constant, although at the time it was adopted by the London Conference, eight years ago, the best information available indicated a higher value by about 30 parts in 100,000. In Holland, Prof. Haga, has obtained 1.11802 using a tangent galvanometer. We believe that the closest figure that can be assigned at the present time to this constant is the round number 1.11800 mg. per absolute coulomb, and is this precisely the value fixed by international agreement in 'international coulombs.'

Since one absolute coulomb of electricity deposits 0.00111800 gram of silver, the number required to deposit a gram-equivalent of silver,

107.88 grams, is 96,494 coulombs, which is the value of the Faraday. For general use the value 96,500 coulombs is a convenient round number which is within the experimental error.

Although some phenomena of the voltameter are not yet perfectly understood, the recent researches have shown how to use it as a reliable current standard and as a means of checking the constancy of the value of the Weston normal cell. The experiments made at the Bureau of Standards during the past eight years have been fully described in a series of papers which have appeared in the *Bulletin* of the Bureau.

VOLUME 3 FEBRUARY, 1917 NUMBER 2

PROCEEDINGS

OF THE

NATIONAL ACADEMY OF SCIENCES

OF THE

UNITED STATES OF AMERICA

Publication Office: Williams & Wilkins Company, Baltimore
Editorial Office: Massachusetts Institute of Technology, Cambridge
Home Office of the Academy: Washington, D. C.

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PROCEEDINGS

OF THE

NATIONAL ACADEMY OF SCIENCES

Volume 3 FEBRUARY 15, 1917 Number 2

ATLANTIS AND THE PERMANENCY OF THE NORTH ATLANTIC OCEAN BOTTOM

By Charles Schuchert

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Communicated, January 8, 1917

In 1912 Prof. Pierre Termier, the director of the Geological Survey of France, delivered before the Oceanographic Institute of Paris a very interesting and stimulating lecture on the probable existence of Plato's Atlantis. This lecture is now published in English in the *Annual Report of the Smithsonian Institution* for 1915 (1916), pages 219–234. In his lecture the speaker drew a conclusion that needs to be examined, as it is of considerable importance in paleogeography whether one is in harmony with it or not. Termier thinks it "A fair conclusion . . . . that the entire region north of the Azores and perhaps the very region of the Azores, of which they may be only the visible ruins, was very recently submerged." This means that the area believed to have been submerged is at least equal to 40,000 square miles, and may be even far more than 200,000 square miles; it is said to have sunk quickly about 10,000 feet beneath the surface of the sea.

What are the facts that lead Termier to this very important conclusion? He relates them as follows:

Some cataclysms certainly have occurred, and they date only as from yesterday. I ask all those who are concerned with the problem of Atlantis to listen attentively and to impress on their mind this brief history; there is none more significant: In the summer of 1898 a ship was employed in the laying of the submarine telegraphic cable which binds Brest to Cape Cod. The cable had been broken, and they were trying to fish it up again by means of grappling irons. It was in north latitude 47° 0′ and longitude 29° 41′ west from Paris, at a point about 500 miles north of the Azores. The mean depth was pretty nearly 1700 fathoms, or 3100 meters. The relaying of the

cable presented great difficulties, and for several days it was necessary to drag the grappling irons over the bottom. This was established: The bottom of the sea in those parts presents the characteristics of a mountainous country, with high summits, steep slopes, and deep valleys. The summits are rocky, and there are oozes only in the hollows of the valleys. The grappling iron, in following this much-disturbed surface, was constantly being caught in the rocks by hard points and sharp edges; it came up almost always broken or twisted, and the broken pieces recovered bore large coarse striæ and traces of violent and rapid wear. On several returns, they found between the teeth of the grappling iron little mineral splinters, having the appearance of recently broken chips. All these fragments belonged to the same class of rocks. The unanimous opinion of the engineers who were present at the dredging was that the chips in question had been detached from a bare rock, an actual outcropping, sharp-edged and angular. The region whence the chips came was furthermore precisely that where the soundings had revealed the highest submarine summits and the almost complete absence of oozes. The fragments, thus torn from the rocky outcrops of the bottom of the Atlantic, are of a vitreous lava, having the chemical composition of the basalts and called *tachylyte* by the petrographers. We are preserving some of these precious fragments at the Musée de 'lÉcole des Mines at Paris.

The matter was described in 1899 to the Académie des Sciences. Few geologists then comprehended its very great import. Such a lava, entirely vitreous, comparable to certain basaltic stones of the volcanoes on the Hawaiian Islands, could solidify into this condition only under atmospheric pressure. Under several atmospheres, and more especially under 3000 meters of water, it might have crystallized. It would appear to us as formed of confused crystals, instead of being composed solely of colloidal matter. The most recent studies on this subject leave no doubt, and I will content myself with recalling the observation of M. Lacroix on the lavas of Mount Pelée of Martinique: Vitreous, when they congealed in the open air, these lavas became filled with crystals as soon as they were cooled under a cover, even not very thick, of previously solidified rocks. The surface which today constitutes the bottom of the Atlantic, 900 kilometers (562.5 miles) north of the Azores, was therefore covered with lava flows while it was still emerged. Consequently, it has been buried, descending 3000 meters; and since the surface of the rocks has there preserved its distorted aspect, its rugged roughnesses, the sharp edges of the very recent lava flows, it must be that the caving in followed very close upon the emission of the lavas, and that this collapse was sudden. Otherwise atmospheric erosion and marine abrasion would have leveled the inequalities and planed down the entire surface. Let us continue our reasoning. We are here on the line which joins Iceland to the Azores, in the midst of the Atlantic volcanic zone, in the midst of the zone of mobility, of instability, and present volcanism. It would seem to be a fair conclusion,

then, that the entire region north of the Azores and perhaps the very region of the Azores, of which they may be only the visible ruins, was very recently submerged, probably during the epoch which the geologists call the present because it is so recent, and which for us, the living beings of today, is the same as yesterday.

Now let us see what are the geologic conditions of the Azores. Gagel[1] tells us that this group of nine islands rises out of the Atlantic from depths of 10,945 feet to elevations of 3250 feet, and in one case even to 8040 feet above the surface of the sea. There are here no old sedimentary or old eruptive formations and the islands appear to be of very recent volcanic origin. Among the volcanic materials have been found only inclusions of fossiliferous middle Miocene limestone. He concludes that they are volcanic islands of Tertiary age that are made up in the main of trachytic and basaltic lavas, that these have probably built themselves up to elevations of from 16,250 to over 21,125 feet, and that some of these volcanoes have been active during the past four centuries.

If the region of the Azores and that to the north of them for many hundreds of miles had been parts of a great continent now sunk deep into the Atlantic, there should be some evidence of this sinking shown in a well marked elevated sea terrace all along the Atlantic, for it is postulated that Atlantis sank when humanity had attained a high state of civilization; in fact the time when the warriors, according to Plato, came from Atlantis cannot be more ancient than Egyptian history. In other words, Plato's Atlantis must have disappeared not more than 8000 to 10,000 years ago, for the priests of Egypt told "of a singularly powerful army, an army which came from the Atlantic and which had the effrontery to invade Europe and Asia . . . . Later, with great earthquakes and inundations, in a single day and one fatal night, all who had been warriors against you [Athens] were swallowed up. The island of Atlantis disappeared beneath the sea."

The area of land supposed by Termier to have sunk is not less than 40,000 square miles, and if we accept his greater supposition that it included also the region to the north, the mass would be not less than 700 miles long by 300 miles wide, or 210,000 square miles. Now let us see how much water the sinking of such masses is equal to, with a view of learning how much the eustatic strand-line of all oceans would be lowered. Murray estimates the superficial area of the oceans as about 139,000,000 square miles, and the mean depth as 13,000 feet. Therefore to sink a mass so small as 40,000 square miles to a depth of 10,000 feet would only lower the general strand-line a little more than

3 feet. If, however, the greater estimate of 210,000 square miles be taken, then the oceanic level would be reduced about 15 feet and this should show in a well marked terrace all along the Atlantic shores. However, it is not only in the Azores that Termier seeks for Plato's lost land, but in the Canary and Cape Verde Islands as well. In other words, he believes that a continent greater than anything assumed for the Azores has very recently foundered, and therefore we should all the more easily observe an elevated strand-line along the Atlantic shores of North America. It is true that there are at least three Pleistocene elevated terraces recorded in Maryland, the highest and oldest one at 220 feet, known as the Talbot terrace, the middle Wicomico one at 100 feet, and the lowest and youngest at 40 feet, the Sunderland terrace. None of these, however, can have any connection with the foundering of Atlantis, as they are far older in age than Plato's account. On the other hand, these and the other Pleistocene terraces are due not only to isostatic and orogenic factors, but also to the climatic factor, as explained by Barrell.[2] From this we see that if a continent situated in the Atlantic foundered into the depths of this ocean, it must have done so in far more ancient times than those of civilized man. Furthermore, the geology of the Azores shows that these islands are not parts of a foundered continent, but that they are volcanic islands that have arisen above the Atlantic bottom during the latter part of Cenozoic time. On the other hand, we learn from Gagel that five of the islands of the Cape Verde group and three of the Canaries have rocks that are unmistakably like those common to the continents. Taking into consideration also the living plants and animals of these islands, many of which are of European-Mediterranean affinities of late Tertiary time, we see that the evidence appears to indicate clearly that the Cape Verde and Canary Islands are fragments of a greater Africa. It is therefore not to the north of the Pillars of Hercules that we should look for Atlantis, but to the southwest of the rock of Gibraltar.

To follow out another line of evidence, the writer understands that petrographers know little from actual observations as to the behavior of flowing lavas under the sea, and whether the cooling phenomena and the formation of vitreous lavas would be the same there as beneath the atmosphere. At least three of them, however, are of the opinion that tachylyte would form equally as readily beneath the sea as on land. In answer to a request for data that might bear on this problem, Doctor A. L. Day, director of the Geophysical Laboratory of the Carnegie Institution of Washington, directed my attention to a recently published paper by Perret.[3] Last year the latter studied the

flow of lava descending from the volcano of Stromboli, in Sicily, and entering the sea, and in 1914 a similar occurrence at Sakurashima, Japan. At Stromboli a surface flow was 20 meters wide, and the hot lava "entered the water at an average rate of about 3 cm. per minute." A little distance beyond the actual contact with the water and in "a perfectly calm sea there is rarely a continuous and uniform evolution of vapor. At Sakurashima, on March 12, 1914, the lava, at one place, was entering a sea as smooth as glass, yet the evolution of steam was spasmodic and resulted in a series of puffs." In regard to the subsurface flow of lava, a condition of greater importance in the present discussion, Perret writes as follows:

At Sakurashima there was a submarine lava flow extending from beneath the eastern lava field for a distance of 2 kilometers along the sea bottom. The lava had a depth of some 75 meters, with 40 meters of water above it . . . . The only disturbance visible at the surface was a succession of convection currents in the water, without eruption of gas, and without raising the water temperature above 64° F. at the surface and 72° just over the lava.

He concludes "that a flowing lava may exist in contact with water without the disintegration of either, thanks to the formation of a protective sheath, and this fact helps us to understand the quiet growth of submarine volcanoes. In such cases the only surface commotion need be that due to true gas emission at the central vent. In point of fact, a sub-aqueous lava stream comports itself more decorously than a similar sub-aerial one." This is due to an important fact, namely that even if the protective cooled sheath is broken in places "and a little water enter and be vaporized in the act of sheathing the raw places . . . . that which is thus evolved is simply the vapor of water, and this, in the presence of water in mass, condenses to water again—there is nothing to reach the surface and cause ebullition."

Doctor Day in the letter to me mentioned above, dated November 27, 1916, comments on Termier's conclusion and the observations of Perret as follows:

I have just read Professor Termier's interesting speculation entitled 'Atlantis' and must confess that I find nothing in my experience with which to support his views. In the forthcoming number of the *American Journal of Science* you will find an article by Perret [the one reviewed above] who is one of the most accurate observers of volcano phenomena with whom I ever came in contact. In this article he records in unmistakable terms that there is no essential difference in the behavior of a sub-aqueous and a sub-aerial

flow other than that which may be exerted by the superimposed hydrostatic pressure. From such experience as we have gathered in this laboratory, hydrostatic pressure can have no other effect than to raise the melting temperature 10 or 20° per thousand atmospheres, that is, 1 or 2%, and this factor must therefore be accounted comparatively insignificant in determining solidification. It is conceivable that great hydrostatic pressure might have the effect of preventing the escape of the volatile ingredients contained in a sub-aqueous lava flow and so facilitate crystallization. It is our experience that a very small quantity of such ingredients has enormously greater influence in determining crystallization than a very large hydrostatic pressure alone. It might therefore follow that the pressure operating in this indirect manner might serve to keep volatile ingredients 'on the job,' so to speak, which would otherwise escape and so promote crystallization in a mixture which would otherwise tend to cool in vitreous form. Beyond this possibility I can conceive of no basis in present experience for the assertion which Termier has made. In general, silicate mixtures which crystallize with difficulty will form glass if cooled quickly whether under pressure or not—the pressure apparently being the least important factor in the situation. Similar mixtures which crystallize readily can with great difficulty be cooled quickly enough to prevent crystallization, and here again the factor of pressure is relatively insignificant.

You may recall a paper by Johnston two or three years ago in which he showed plainly and unmistakably that in general small changes of temperature or concentration would have greater effect in determining the resulting solid form than a thousand atmospheres of pressure. This conclusion is in a sense obvious, for if a thousand atmospheres will produce no more than 10 or 20° effect on the melting temperature, then obviously 10 or 20° temperature change in this temperature region will be its equivalent. In the same sense a 1 or 2% admixture of one of the volatile ingredients will produce several tens of degrees lowering of the melting point of the solution in this temperature region. These considerations are perfectly general and apply without reservation to the condition of things which Termier is discussing. I am therefore disinclined to give any weight to the evidence which he adduces in proof of the contention that vitreous basalts could not have formed at depth as well as anywhere.

This paper has also been read by Prof. L. V. Pirsson, and he makes the following comments in regard to the formation of tachylytes:

Whether a magma will solidify in a vitreous or a crystalline condition appears to be much more due to temperature than to pressure. The latter, in the quantities which we have to deal with in the superficial crust of the earth, seems relatively negligible compared with very moderate changes of temperature. If the change of temperature of a basaltic magma on attaining a sub-aerial surface is sufficient to cause it to solidify as a glass, or tachylyte,

as we know it may, there seems no good reason why a basalt magma issuing into cold water on the sea-floor might not be similarly affected and have an upper glassy crust. Such a glassy skin on the lava would seem an even more natural result from the melt being plunged into cold water than if it cooled in the air, the pressure of the depth of water being a minor consideration compared with the sudden change of temperature. The experience of mankind from remote ages has taught that the quickest and most convenient way of suddenly cooling a heated material is to plunge it into cold water.

That basaltic glasses, or tachylytes, are not formed solely under atmospheric conditions is shown also by the fact that they have been found as the selvage edges of intrusive rocks, in dikes, and in intrusive sheets, in Finland, Sweden, Connecticut, and elsewhere. These glassy sulbands are now revealed to us only after prolonged erosion, and the geologic evidence would appear to indicate that they were probably formed under greater pressures than would be produced by the weight of the water of the ocean. It was the sudden chilling, produced by the contact with cold rocks, which forced the glass to form in spite of the pressure.

In the light of petrographic experience it does not seem that the generalization of Professor Termier is well founded. The fact of dredging glass splinters from oceanic depths in a volcanic region can hardly be held in itself as a proof of profound subsidence of such an area from sub-aerial conditions

The conclusions from these various studies are (1) that the Azores are volcanic islands and are not the remnants of a more or less large continental mass, for they are not composed of rocks seen on the continents; (2) that the tachylytes dredged up from the Atlantic to the north of the Azores were in all probability formed where they are now, at the bottom of the ocean; and (3) that there are no known geologic data that prove or even help to prove the existence of Plato's Atlantis in historic times.

The greater question, was Africa ever united to South America is being answered by biologists and geologists, 'yes' and 'no.' The writer, however, believes in this connection previous to the Tertiary and thinks that the down-breaking of western Gondwana began in the late Lower Cretaceous, with complete severance long before the close of Eocene time, for marine strata of this age are general along the western border of Africa. On the other hand, if this land bridge had continued unbroken into Tertiary time, even only as late as the later Eocene, then certainly the wonderful fossil mammalian faunas of Argentina should have revealed many and unmistakable African links. The African affinities in the ancient South American mammalian faunas are, however, so slight as to give but a very limited support to the theory that Gondwana was still in existence in early Tertiary time, and none at all to

the theory that the South Atlantic bridge was present even in the Miocene.

[1] Gagel, C., *Handbuch der Regionalen Geologie*, 7, Pt. 10, Heidelberg, 1910.
[2] Barrell, J., *Amer. J. Sci., New Haven, Conn.*, (Ser. 4), **40**, 1915, (1–22). Also see Wright, W. B., *The Quaternary Ice Age*, London, 1914, chaps. 16 and 18; and Goldthwait, J. W., *Amer. J. Sci., New Haven, Conn.*, (Ser. 4), 32, 1911, (291–317).
[3] Perret, F. A., *Amer. J. Sci., New Haven, Conn.*, (Ser. 4), **42**, 1916, (443–463).

THE RESPONSES OF HYDROIDS TO GRAVITY

By G. H. Parker

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Read before the Academy, November 14, 1916

Corymorpha palma is a solitary hydroid occurring on the mud-flats of False Bay and other like localities in the neighborhood of La Jolla, southern California. It has a length of body that may exceed even 6 or 7 cm. In its natural position under water its basal end is imbedded in the mud, above which its stem rises vertically, carrying at the opposite end the somewhat drooping head. When Corymorpha is removed from the mud and allowed to attach itself to some foreign base which can be conveniently turned in an aquarium, it assumes in a very short time a vertical attitude irrespective of the position of the base. This vertical attitude is acquired and maintained by the stem even after the head has been cut off and this part may therefore be said to exhibit negative geotropism. The stem is provided with a neuromuscular sheath and a core of vacuolated cells like those in the chorda of vertebrates. Torrey has raised the question of the relative importance of the neuromuscular sheath and of the core cells in bringing about the geotropic response and has advanced evidence in favor of the view that the core cells, acting as certain vegetable tissues often do, are the mechanism of this response. If, however, Corymorpha is placed in sea-water containing some chloretone, by which the neuromuscular activity is abolished but the core cells are left unchanged, no geotropic response can be obtained when the animal is moved out of the vertical. When the core cells are disorganized by twirling a needle in the axis of the stem, care being taken that the neuromuscular sheath is not injured, the stem will show a tardy but successful geotropism. It therefore seems probable that the geotropic response in Corymorpha, as in most other animals, is the result of the activity of the neuromuscular sheath and not of the core cells, though the latter very probably help to maintain the geotropic position by assuming a somewhat fixed

arrangement favorable to the particular curve or form of the stem imposed by the neuromuscular response. The work thus briefly reported upon was carried out last summer at the Scripps Institution for Biological Research, to the staff of which the writer is indebted for many courtesies.

[1] Contributions from the Zoological Laboratory of the Museum of Comparative Zoology at Harvard College, No. 287.

THE LIPS AND THE NASAL APERTURES IN THE GNATHOSTOME FISHES, AND THEIR HOMOLOGUES IN THE HIGHER VERTEBRATES

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Communicated by E. L. Mark, December 12, 1916

Two distinctly different types of upper and lower lips, and a third type of upper lip are found in the gnathostome fishes, and they may be called the primary, secondary and tertiary lips.

The primary lips lie immediately external to the dental arcades developed in relation to the palatoquadrates and mandibulae, and they must, because of this position, have primarily lain but slightly, if at all, anterior to the oral plate of embryos. The primary cavity of the mouth lies internal to these lips. The hypophysial invaginations probably lay external to them, as they actually do lie external to the upper and primary lip of Petromyzon, and to both the primary and secondary upper lips of Amia and Acipenser.

The secondary lips of either side lie external to the primary ones, and have been developed from what was primarily simply a fold of the external dermis that lay posterior to the angle of the gape of the primary lips. The pressure of the musculus adductor mandibulae, where it passed around the angle of the primary gape, caused this fold to bulge forward across that angle, and its anterior surface was then presented antero-mesially. The crest of this fold then formed a secondary angle of the gape, which lay antero-lateral to the primary angle, and short secondary lips ran forward from it, in either jaw, to join the primary lips. These primarily short secondary lips then gradually extended forward in either jaw and ultimately there reached the median line and coalesced with their fellows of the opposite side, a band of the external surface of the head, lying between these secondary lips and the primary ones, thus being enclosed in the cavity of the mouth as a secondary

addition to it. Successive stages in this process are actually found persisting in the adults of living Plagiostomi.

The fold of the secondary upper lip of either side, running forward, passes either (1) between the primary upper edge of the mouth and the oral one of the two nasal apertures of its side, (2) across that aperture, (3) or between it and the aboral aperture, this depending upon the position of the nasal apertures relative to the upper edge of the mouth, upon the height of the fold of the secondary upper lip, and upon the length of the secondary gape of the mouth.

In all of the Teleostomi that I have been able to examine, the fold of the secondary upper lip passes oral to both the nasal apertures, as it also does in all of the Plagiostomi that I have been able to examine in which there is no naso-buccal groove, excepting only Heterodontus. Where there is a naso-buccal groove, the fold either (1) abuts against that groove and there abruptly ends (Scyllium canicula), (2) forms the lateral edge of that groove (Raia clavata), (3) or, as shown in Müller and Henle's[1] figures of Ginglymostoma concolor and Stegostoma fasciatum, passes between the nasal apertures and is continued mesial to them as a slight fronto-nasal ridge. The naso-buccal groove is simply the oral edge of the oral nasal aperture combined with the nasal-flap furrow, and results from the obstruction, by that aperture and the nasal flap, of the normal progression of the fold of the secondary upper lip. In Heterodontus francisci, where this groove is not found, the fold of the secondary upper lip passes between the two nasal apertures and is continued mesial to them as a well developed fronto-nasal flap, or process, and the posterior nasal aperture, lying oral to the fold of this lip, is enclosed in the secondary cavity of the mouth; but it still lies external to the primary cavity, exactly as the corresponding aperture does in all of the other Plagiostomi examined. In the Holocephali the conditions are too complicated to be here explained, but they have been derived directly from those in those of the Plagiostomi in which there is a naso-buccal groove. In the Dipneusti the fold of the secondary upper lip passes either between the two apertures or oral to both of them, the conditions in the one specimen of these fishes that I have been able to examine (Ceratodus) not giving definite indications as to this.

The so-called fronto-nasal process of those Plagiostomi in which there is a naso-buccal groove and in which the oral edge of the nasal flap is extended orally so as to bound the upper edge of the mouth, as in Scyllium canicula, is not the homologue of the fronto-nasal process of Heterodontus, for the nasal flap arises wholly independently of the

fold of the secondary upper lip and is not even crossed by that fold. This edge of the nasal flap thus does not form part of the secondary upper lip of the fish.

Cartilages are usually found developed in relation to the secondary lips of the Plagiostomi and Holocephali, and bones, with or without teeth, in the Teleostomi, the bones in the upper lips of the latter fishes forming a premaxillo-maxillary dental arcade which lies external to, and concentric with, the primary palatoquadrate, or so-called vomero-palato-pterygoid arcade.

In the Teleostomi, Holocephali and Dipneusti a dermal fold may be found lying aboral to the secondary upper lip.

In the Teleostei and Holostei this fold lies along and encloses the ventral edge of the lachrymal bone alone, or the ventral edges of that bone and one or more of the suborbital bones, and some part of the dorsal edge of the maxillary bone passes upward internal to the fold. The fold may accordingly be called the supramaxillary fold, for although it is, in these fishes, definitely related to the lachrymal bone, it is not so related to that bone in the Holocephali and Dipneusti. The fold, in the Teleostei and Holostei, is not continued forward in relation to the antorbital and dermal ethmoid bones, those bones being developed in relation to the antorbital section of the buccalis latero-sensory canal, the fold apparently being restricted to the suborbital section of that canal. In the Anacanthini, because of the marked anterior extension of the lachrymal bone, the fold is carried forward between the nasal apertures and the upper edge of the mouth, and is there continuous, in the median line, with its fellow of the opposite side; this apparently being a specific character of these fishes.

In the Holocephali and Dipneusti the supramaxillary fold passes aboral to the nasal apertures, and in these fishes the buccalis latero-sensory line passes aboral instead of oral to those apertures, and neither lachrymal, antorbital nor dermal ethmoid bones are developed in relation to it. In Chimaera the fold encloses the outer ends of a series of ampullary tubules, the external openings of these tubules lying near the edge of the fold and representing the places of origin of the related ampullary organs (Allis,[2]); and the coalesced folds of opposite sides form a semicircular line which circumscribes the nasal apertures, the upper lips, and the related naso-labial folds. In Ceratodus this fold has this same relation to the nasal apertures and the secondary upper lips, but it has here become a tertiary upper lip, and that band of the external surface of the head that lies between it and the secondary upper lip has been added to the secondary cavity of the mouth. The two

nasal apertures of Ceratodus are accordingly enclosed in the buccal cavity so enlarged, but as I can not trace the secondary upper lip in my specimen of this fish I can not tell whether they both lie in the tertiary addition to that cavity or one of them in that part of the cavity and the other in the secondary addition to it. No tertiary lower lip is formed, the tertiary upper lip simply overlapping the secondary lower lip. The anterior dental plates of this fish lie immediately internal to a lip that would seem to be a primary one, but as I cannot trace the secondary lips I cannot definitely affirm this. In Chimaera the corresponding plates lie internal to the primary lips, and hence belong to the palatoquadrate arcade.

In the Amniota the definitive upper lip is a secondary one, and it passes between the two definitive nasal apertures. Maxillary and premaxillary bones are developed in relation to it, and it is certain, from conditions found in certain of the Sauropsida, that the internal nasal aperture lay primarily between this arcade and the vomero-palatoquadrate arcade. The palatine bone of certain of these latter vertebrates, however, develops laterally and anteriorly, either approaching, coming into contact with, or even fusing with the maxillary or premaxillary bones, and in Chelone a ventral process of the bone turns antero-mesially and fuses with a ventral plate of the vomer anterior to the definite posterior nasal aperture. The posterior nasal passage of this animal thus lies between dorsal and ventral plates of both the vomer and the palatine, and the definitive choana, which is a secondary one, lies posterior to the ventral plates, the primary choana lying, as it normally should, anterior to the dorsal plates and hence to the bodies of the palatine and vomer. An accentuation of these conditions would withdraw the palatine bone entirely from its own arcade and leave it definitely anterior to the choana, as it is in the human skull, but it there nevertheless still lies posterior to the primitive choana.

In embryos of all of the gnathostome vertebrates above considered, the primary lips are represented in the edges of the primary stomodaeum, and hence not only in the deeper portions of the so-called maxillary and mandibular processes but also in the tissues that lie between the anterior (symphysial) ends of those processes. In embryos of the Teleostomi and Plagiostomi, excepting Heterodontus and those other Plagiostomi in which similar conditions may exist, the secondary lips are represented in the superficial portions of these same processes, and the maxillary processes, like the secondary upper lips of the adult, in growing forward pass oral to the nasal apertures, and they alone, or they together with an intervening portion of the primary upper lip, form the

definitive upper lip of the fish. In embryos of the Amniota, and unquestionably also in embryos of Heterodontus and those other Plagiostomi in which similar conditions may exist, the fold of the secondary upper lip is represented in the maxillary and fronto-nasal processes, the fold of this lip having here been cut into two parts by its encounter with the nasal groove; and these two processes form the definitive upper lip. With the formation of these embryonic so-called processes the primary stomodaeum has been converted into a secondary one, and a portion of the external surface of the head is in process of being enclosed in the cavity of the mouth. In embryos of Ceratodus the supramaxillary fold is superadded to the maxillary process, and the stomodaeum becomes a tertiary one.

In embryos of the Amniota the maxillary and fronto-nasal processes, representing the two parts of the fold of the secondary upper lip, lie primarily oral, respectively, to the lateral and mesial nasal processes as those processes are defined by Peter,[8] the fold of the secondary upper lip thus passing across the oral edge of the nasal groove, as it does in the adult Chimaera, and not, as in the adult Heterodontus, in the line of the nasal processes and hence that of the future nasal bridge. The break between the two parts of the fold then forms the so-called naso-buccal groove, which is thus simply a partially closed portion of the future posterior nasal aperture and not a specially developed groove which cuts across the secondary lip and leads into the secondary cavity of the mouth. The two parts of the fold never, in the Amniota, completely fuse with each other above the nasal groove, being always separated from each other by an epithelial line or membrane which is later broken through, and when a nasal bridge has been formed by the fusion of the nasal processes, the fold traverses that bridge. This bridge is thus the strict homologue of the bridge in the Teleostomi; for it is highly improbable that any new material has been brought to it by a simple fold of the dermis, as it is also improbable that such a fold, simply because it passes across the bridge, can in any way change the morphological character of that bridge. That band of the external surface of the head that lies between the primary and secondary upper lips forms the primitive palate, and the nasal processes take part in its formation only as they are included, in part, in that surface. Where the nasal bridge lies wholly aboral to the secondary upper lip, as in the Teleostomi, it does not form a primitive palate and can in no way be compared with it.

Aboral to the maxillary process of embryos of the Mammalia there is another process, or more properly a fold, which diverges slightly from the maxillary process and extends as far forward as the lachrymal

groove. This fold is well shown in Keibel's[4] figures of embryos of the pig, and must represent the supramaxillary fold of the Teleostomi, the lachrymal groove representing a part of the supramaxillary furrow of those fishes. The supramaxillary fold is apparently not continued onward anterior to this point, as it is in Chimaera and Ceratodus, and the Schnauzenfalte of His's[5] descriptions of human embryos, notwithstanding that it strikingly resembles the median portion of the supramaxillary fold of Chimaera and Ceratodus, is probably not a part of that fold. The lips and nasal apertures of the Mammalia could, accordingly, not be derived from those in Ceratodus without marked reversions, but they could readily be derived from those in Amia or Polypterus by the simple shifting of the secondary upper lip from a position oral to the nasal apertures to one between those apertures.

In the Amphibia the formation of the nasal appertures, as described by authors, is markedly different from that above set forth, but this is certainly due simply to condensations and abbreviations of the normal developmental processes, for the posterior nasal apertures of the adults of these vertebrates lie, as they do in the Amniota, between the primary and secondary dental arcades, and the nasal apertures of either side are, in embryos of certain of these vertebrates, connected by an epithelial cord (Gymnophiona) or line (Urodela) derived from the external epidermis; this cord or line certainly indicating the line where nasal processes have fused with each other above the nasal groove to form a normal nasal bridge.

[1] Müller, J., und Henle, J., *Systematische beschreibung der Plagiostomen*, 1841, Berlin, xxii + 200 pp., 60 Taf.

[2] Allis, E. P., Jr., *Q. J. Microsc. Sci., London*, N. S. **45**, 1901, (87–236), pl. 10–12.

[3] Peter, K., *Handbuch vergl. exper. Entwickelungslehre d. Wirbeltiere* von O. Hertwig, Bd. 2, Teil 2, 1906, (1–82).

[4] Keibel, Fr., *Anat. Anz., Jena*, **8**, 1893, (473–487).

[5] His, W., *Arch. Anat. Physiol., Anat. Abth., Leipzig* Jahrg. 1892, (384–424).

NATURAL AND ISOGONAL FAMILIES OF CURVES ON A SURFACE

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Communicated by E. H. Moore, December 13, 1916

1. If F is a function of the coördinates of a point and ds is the element of arc length in any space, the curves along which $\int Fds$ *is a minimum* are said to form a *natural family of curves*. Such families include many interesting special cases. Thus if W is the negative potential function and h is a given constant of energy in a conservative

field of force, our natural family may be: (1) a system of *trajectories* arising from the principle of least action, where $F = \sqrt{W + h}$; (2) a system of *brachistochrones* or curves of quickest descent, where $F = \sqrt{(W + h)^{-1}}$; (3) a system of *general catenaries* or positions of equilibrium of homogeneous, flexible, inextensible strings, where $F = (W+h)$. Again, if F is the variable index of refraction in an isotropic medium, the *paths of light* in such a medium form a natural family. The *conformal representation of the geodesics* of any surface upon certain other surfaces is also a natural family. Natural families of curves have been geometrically characterized by Kasner for a plane and for space of three dimensions,[1] and by me for any surface and any curved space of n dimensions.[2]

If we have any set of ∞^1 curves on a surface, the system of ∞^1 curves which cut every curve of this set at a constant angle, α, form a system of isogonal trajectories of the original set; there are ∞^1 such systems for varying values of the parameter α, and these form the complete family of ∞^2 *isogonal trajectories*. Isogonal families of curves have been geometrically characterized by Kasner for the plane,[1] and by me[3] for any surface.

In this paper, §2 gives a very general geometric transformation by which a family of isogonals may be transformed into a natural family; §3 gives the analytic representation of the geometric transformation of §2, and exhibits the interchange of the two families through repeated application of this transformation; §4 gives the relations existing between the point functions which characterize dual (natural-isogonal) families.

2. If we take an isothermal system of curves as parameter curves on the surface, we can write the element of arc length in the form

$$ds^2 = \lambda\,(u, v)\,[du^2 + dv^2].$$

The variation problem

$$\int F\,(u,v)\,ds = minimum \tag{1}$$

then leads to a family of ∞^2 curves whose differential equation is given by[3,4]

$$v'' = [\,(\log F\sqrt{\lambda})_v - (\log F\sqrt{\lambda})_u\,v']\,[1 + v'^2] \qquad (type\ N) \tag{2}$$

and the problem of finding the isogonals of a simple system of curves

$$v' = \tan\omega\,(u,\ v) \tag{3}$$

leads to a family of ∞^2 curves whose differential equation is given by[3]

$$v'' = (\omega_u + \omega_v\,v')\,(1 + v'^2) \qquad (type\ I_\omega) \tag{4}$$

If we have any other simple system of curves

$$v' = \tan \alpha (u, v), \tag{5}$$

the differential equation of its isogonals is similarly given by

$$v'' = (\alpha_u + \alpha_v v') (1 + v'^2) \qquad (type\ I_\alpha) \tag{6}$$

For these families, ∞^1 curves pass through each point on the surface, one in each direction.

The geodesic curvature for any curve on the surface is

$$\frac{1}{\rho} = \frac{v'' - [(\log \sqrt{\lambda})_v - (\log \sqrt{\lambda})_u v'] [1 + v'^2]}{\sqrt{\lambda} (1 + v'^2)^{\frac{3}{2}}} \tag{7}$$

If we apply (7) to the unrelated I_ω and I_α curves, we have, for a curve in any direction v',

$$\left[\frac{1}{\rho}\right]_{I_\omega} = \frac{[\omega_u - (\log \sqrt{\lambda})_v] + [\omega_v + (\log \sqrt{\lambda})_u] v'}{\sqrt{\lambda} (1 + v'^2)}, \tag{8}$$

$$\left[\frac{1}{\rho}\right]_{I_\alpha} = \frac{[\alpha_u - (\log \sqrt{\lambda})_v] + [\alpha_v + (\log \sqrt{\lambda})_u] v'}{\sqrt{\lambda} (1 + v'^2)}, \tag{9}$$

and hence

$$\left[\frac{1}{\rho}\right]_{I_\omega} - \left[\frac{1}{\rho}\right]_{I_\alpha} = \frac{(\omega_u - \alpha_u) + (\omega_v - \alpha_v) v'}{\sqrt{\lambda} (+ v'^2)}. \tag{10}$$

On the other hand, if we apply (7) to the N curves, we have, for a curve in any direction v',

$$\left[\frac{1}{\rho}\right]_N = \frac{(\log F)_v - (\log F)_u v'}{\sqrt{\lambda} (1 + v'^2)} \tag{11}$$

and for a curve in a direction $\left(-\frac{1}{v'}\right)$, i.e., in a direction perpendicular to the direction v',

$$\left[\frac{1}{\rho}\right]_N = \frac{(\log F)_u + (\log F)_v v'}{\sqrt{\lambda} (1 + v'^2)}. \tag{12}$$

Comparing (12) and (10) we see that the right members of these equations will coincide if

$$\log F = \omega - \alpha, \quad \text{or} \quad F = e^{\omega - \alpha}. \tag{13}$$

Thus if we have two distinct I families and we choose one curve of each family passing through the same point in the same direction, the

difference of their geodesic curvatures is equal to the geodesic curvature of a curve through that point but in a perpendicular direction of a related N family. This is evidently true for every point and in every direction.

Considering our ∞^2 curves as composed of ∞^3 geodesic curvature elements (u, v, v', v''), and defining corresponding geodesic curvature elements on a surface as two elements which have the same initial point and the same direction, we may state the following result:

Given any two isogonal families. If in each direction through each point we construct a geodesic curvature element whose geodesic curvature is equal to the difference of the geodesic curvatures of corresponding elements of the two isogonal families, and then rotate each new element in the same direction through a right angle (keeping its geodesic curvature unchanged), the ∞^3 new elements will form a natural family.

If ω and α are the functions determining the two isogonal families, then the above transformation leads to the natural family whose characteristic function, F, is the exponential of $\omega - \alpha$.

According as we subtract the geodesic curvatures of the I_α curves from those of the I_ω curves or vice versa, we get the N family, $F = e^{\omega-\alpha}$ or $F_1 = e^{\alpha-\omega}$, so that $F = 1/F_1$; hence

Two isogonal families give rise, by the above mentioned transformation, to two natural families whose characteristic point functions are reciprocals, and such that corresponding geodesic curvature elements have their geodesic curvatures numerically equal but opposite in sign.

3. The analytic curvature transformation which changes an I family into an N family, is

$$(T)\ u_1=u,\ v_1=v,\ v_1'=-\frac{1}{v'},\ v_1''=-\frac{v''-[(\log\sqrt{\lambda}-\alpha)_v-(\log\sqrt{\lambda}-\alpha)_u v'][1+v'^2]}{v'^3}$$

where α is an *arbitrary* point function.

This changes

$$v'' = (\omega_u + \omega_v v')\ (1 + v'^2) \qquad (\textit{type } I) \tag{4}$$

into

$$v''=\{\,[(\omega-\alpha)_v+(\log\sqrt{\lambda})_v]-[(\omega-\alpha)_u+(\log\sqrt{\lambda})_u]v'\}\ \{1+v'^2\}\ (\textit{type } N) \tag{14}$$

and by comparison with (2), we have

$$\log F = \omega - \alpha, \quad \text{or} \quad F = e^{\omega-\alpha} \tag{15}$$

Hence (T) is the analytic statement of the geometric transformation described in §2.

It is interesting to note the results of repeated applications of the

transformation (T) on the I family, (4). (T^2), (T^3), and (T^4) lead respectively to

$$v'' = \{[(\alpha - \omega - \log\sqrt{\lambda})_u - (\alpha - \log\sqrt{\lambda})_v] + [(\alpha - \omega - \log\sqrt{\lambda})_v + (\alpha - \log\sqrt{\lambda})_u]\, v'\}\ \{1 + v'^2\}, \tag{16}$$

$$v'' = \{[(\alpha - \log\sqrt{\lambda})_u - \omega_v] + [(\alpha - \log\sqrt{\lambda})_v + \omega_u] v'\}\ \{1 + v'^2\} \tag{17}$$

$$v'' = (\omega_u + \omega_v\, v')\,(1 + v'^2). \tag{18}$$

Now equations (2) and (4) are special forms of a more general equation

$$v'' = (\psi - \phi\, v')\,(1 + v'^2), \tag{19}$$

which reduces to type I or type N according to the restriction

$$\psi_v + \phi_u = 0 \quad \text{or} \quad \psi_u - \phi_v = 0 \tag{20}$$

respectively. Applying the criteria (20) to equations (16), (17), and (18), we may draw the following conclusions:

Given any I family, (T) always transforms this into an N family, and (T^4) always gives the orginal I family. In general (T^2) and (T^3) give neither an I nor an N family; but if the auxiliary arbitrary function, α, is so chosen that the system $v' = \tan(\alpha - \log\sqrt{\lambda})$ is an isothermal system,[5] or if our surface is developable[6] and the system $v' = \tan\alpha$ is isothermal, then (T^2) gives an I family and (T^3) gives an N family.

Given any $N-I$ family, i.e., the isogonals of an isothermal system (cf. §4), (T) always transforms this into an N family, (T^2) gives neither an I family nor an N family, in general, (T^3) always gives an I family, and (T^4) always gives the original $N-I$ family. If the auxiliary arbitrary function is so chosen that the system $v' = \tan(\alpha - \log\sqrt{\lambda})$ is isothermal, or if our surface is developable and the system $v' = \tan\alpha$ is isothermal, then (T), (T^2), and (T^3) give $N-I$ families (not the original family).

4. If the N family

$$v'' = [(\log F\sqrt{\lambda})_v - (\log F\sqrt{\lambda})_u\, v']\,[1 + v'^2]$$

and the I family

$$v'' = (\omega_u + \omega_v\, v')\,(1 + v'^2)$$

coincide, then we must have

$$\left.\begin{array}{c} (\log F\sqrt{\lambda})_v = \omega_u \quad \text{and} \quad (\log F\sqrt{\lambda})_u = -\,\omega_v \\ \text{or} \\ \omega_{uu} + \omega_{vv} = 0 \quad \text{and} \quad (\log F\sqrt{\lambda})_{uu} + (\log F\sqrt{\lambda})_{vv} = 0 \end{array}\right\} \tag{21}$$

Therefore the curves $v' = \tan\omega$ and $v' = \tan(\log F\sqrt{\lambda})$ are isothermal systems, and the functions ω and $\log F\sqrt{\lambda}$ are conjugate harmonic. Thus the base system of our isogonals is isothermal, and if H is con-

jugate harmonic to ω, i.e., if $\omega + i\,H$ is a function of $u + i\,v$, then $F = \lambda^{-\frac{1}{2}} e^{H}$. Now for our parameter system, which is *any* isothermal system, $\omega = 0$ and therefore $H = 0$ and $F = \lambda^{-\frac{1}{2}} = F_0$, and we may write $F = F_0\, e^{H}$. Hence

If F_0 is the characteristic function corresponding to the isogonals of an isothermal system, an N family can be identified with an I family when and only when its characteristic function is the product of F_0 and the exponential of a harmonic function.[7]

If we have two isothermal systems, $v' = \tan\omega$ and $v' = \tan\alpha$, and ω and α are conjugate harmonic, they form the base systems of two $N - I$ families whose characteristic functions are

$$F_\omega = \lambda^{-\frac{1}{2}} e^{\alpha} \quad \text{and} \quad F_\alpha = \lambda^{-\frac{1}{2}} e^{\omega}$$

respectively; hence

$$\frac{F_\alpha}{F_\omega} = e^{\omega - \alpha} \tag{21}$$

On the other hand, the isogonals of the system $v' = \tan\omega$ are transformed by (T) into the N family whose characteristic function is $F = e^{\omega - \alpha}$. Comparing this with (21) we may write

$$F = \frac{F_\alpha}{F_\omega} \tag{22}$$

If $\omega + i\,\alpha$ is a function of the complex variable $u + i\,v$, it determines two isothermal systems, $v' = \tan\omega$ and $v' = \tan\alpha$, which are the base systems of two $N - I$ families. Either of these families may be transformed by means of (T) and the remaining family into an N family whose characteristic function is the ratio of the characteristic functions of the two given families.

[1] Kasner, E., *Trans. Amer. Math. Soc., New York*, 10, 1909, (201–219).

[2] Lipka, J., *Ibid.*, 13, 1912, (77–95).

[3] Lipka, J., *Ann. Math., Princeton, N. J.*, 15, 1913, (71–77).

[4] Throughout this paper, primes refer to total derivatives with respect to u, and literal subscripts to partial derivatives.

[5] The condition that the system $v' = \tan\beta$ be isothermal is $\beta_{uu} + \beta_{vv} = 0$.

[6] The condition for a developable surface is $(\log\lambda)_{uu} + (\log\lambda)_{vv} = 0$; cf. Note[3].

[7] Compare with Kasner, E., *New York, Bull. Amer. Math. Soc.*, 14, 1908, (169–172).

SOME PROBLEMS OF DIOPHANTINE APPROXIMATION: THE SERIES $\Sigma e(\lambda_n)$ AND THE DISTRIBUTION OF THE POINTS $(\lambda_n \alpha)$

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Communicated by E. H. Moore, December 5, 1916

1. In our previous writings on the subject of Diophantine approximation, which we refer to in a short note published in the October number of these Proceedings,[1] we alluded in several places to a series of further results which, we hoped, were to form the material for a third memoir in the *Acta Mathematica*. The prosecution of this work was delayed, in the first instance, by our occupation on a long memoir on the theory of the Riemann Zeta-function, now in type and shortly to appear there, and subsequently by other causes; and there is, under present conditions, little hope of its completion in the immediate future. The subject has since been reopened by the appearance of work by other writers,[2] and in particular of a very beautiful memoir by Weyl in the latest number of the *Mathematische Annalen*.[3] This paper contains allusions to our unpublished work: and it seems desirable that we should make some more definite statement than has appeared hitherto of our results and the relations in which they stand to Weyl's.

The main problems which we considered were three.

2. (*a*) The first problem was that of proving that, *if* $e(x) = e^{2\pi i x}$ *and*

$$\lambda_n = \alpha n^k + \alpha_1 n^{k-1} + \ldots + \alpha_k$$

is a polynomial in n with at least one irrational coefficient, then

$$s_n = \sum_1^n e(\lambda_k) = o(n).$$

We may plainly suppose that every α has been reduced to its residue to modulus unity: and there is no substantial loss of generality in supposing the *first* coefficient irrational.

This theorem we enunciated first, in the special case in which $\lambda_n = \alpha n^k$, in our communication to the Cambridge Congress, characterising the proof as 'intricate.' In our second memoir in the *Acta* we discussed in detail the case $k = 2$, using a transcendental method which leads to a whole series of more precise results; and we promised a proof of the more general theorem in the third memoir of the series. Weyl's memoir contains a complete statement and proof, both quite independent of ours, of the theorem in its most general form.

The limitation on the form of λ_n, which appears in the theorem as we stated it, was introduced merely for the sake of compactness of expression and does not correspond to any real simplification of the problem. Our argument indeed depends upon an induction which compels us to consider the problem generally. The most comprehensive result which appears in our analysis is as follows: *given any positive numbers* ϵ *and* η *we can determine* $\nu(\epsilon)$, $N(\epsilon, \eta)$, *and a system of intervals* j, *including all rationals whose denominators are less than* ν, *and of total length less than* η, *so that* $|s_n| < \epsilon n$ *for* $n > N$, *all values of* α *exterior to the intervals* j, *and all values of* $\alpha_1, \alpha_2, \ldots \ldots, \alpha^k$. From this result it follows at once that $s_n = o(n)$ for any particular irrational α, and uniformly in $\alpha_1, \alpha_2, \ldots \ldots, \alpha^k$.

Weyl's proof and ours are widely different, and each, we hope, may prove to have an interest of its own. The same is true of the deduction of the formula $\zeta(l + it) = o(\log t)$, made by Weyl as well as by ourselves.

3. (*b*) The second principal problem was, to use Weyl's phraseology, that of the 'uniform distribution' (*Gleichverteilung*) of the points (λ_n) where (x) is the residue of x to modulus unity. Suppose that n_j is the number of the first n such points which fall within an interval j of length δ. Then the points are said to be *uniformly distributed* if $n_j \sim \delta n$ for every such interval j. It is plain that a corresponding definition may be given of uniform distribution of an enumerable sequence of points in space of any number of dimensions.

That the points (λ_n) are uniformly distributed when $k = l$ and α is irrational was proved independently by Bohl, Sierpinski, and Weyl in 1909–10. The general result (with the same unessential limitation as to the form of λ_n) was stated by us in our first paper in the *Acta*. Our proof, which has never been published, proceeded on the same lines as that of the thoerem of §2. But Weyl has now established a 'principle' which renders such a proof entirely unnecessary, and which has led him to results in this direction far more comprehensive than any of ours. This 'principle' is expressed by the theorem: *if*

$$\sum_{1}^{n} e(m\lambda_h) = o(n)$$

for every positive integral value of m, *then the points* (λ_n) *are uniformly distributed in* (0, 1). The proof depends on a simple but ingenious use of the theory of approximation to arbitrary functions by finite trigonometrical polynomials; and there is a straightforward generalisation to space of any number of dimensions.

Weyl's 'principle' enables him to deduce, with singular ease and elegance, theorems of 'uniform distribution' from theorems of the character of that of §2, and to generalise them immediately to multidimensional space. It enables him to prove, for example, that *the points whose coordinates are*

$$(np\ \alpha_q)\ (p = 1, 2, \ldots, k;\ q = 1, 2, \ldots, l;\ n = 1, 2, 3, \ldots)$$

where $\alpha_1, \alpha_2, \ldots, \alpha_l$ *is any set of linearly independent irrationals, are uniformly distributed in the 'unit cube' of kl dimensions.* All that we had been able to prove was that the points were *everywhere dense* in the cube.

4. (*c*) Corresponding questions arise in connection with an arbitrary increasing sequence $\lambda_1, \lambda_2, \lambda_3, \ldots\ldots$ Are the points $(\lambda_n\ \alpha)$, for example, uniformly distributed? The answers to such questions in general involve an unspecified exceptional set of values of α of measure zero, instead of (as when $\lambda_n = n^k$) a specified set such as the rationals; they are, in other words, only 'almost always' true.

In our first paper in the *Acta* we proved quite generally that the set $(\lambda_n\ \alpha)$ is almost always everywhere dense. The corresponding theorem of 'uniform distribution' we discussed only in one especially interesting particular case, that in which $\lambda_n = a^n$, where a is an integer. The theorem is in this case substantially equivalent to results obtained by Borel,[4] from the standpoint of the theory of probabilities, and by Faber,[5] as a corollary of Lebesgue's theorem that a rectifiable curve has a tangent at almost every point. Our analysis however contains the first direct and general discussion of the problem, and leads to results notably more precise than that of mere uniformity of distribution. These results were afterwards made the subject of important generalisations by Fowler,[2] whose investigations covers all cases in which λ_n increases with tolerable regularity and as fast as an exponential of the type e^{n^ρ}. Weyl's 'principle' enables him to reduce this problem to a study of the series $\Sigma e\,(\lambda_n\ \alpha)$, and leads him to the following theorem, so far the most general of its kind. *If* $c > 0$, $\delta > 0$, *and* λ_n *increases by at least* c *whenever* n *increases from* h *by as much as* $h\ (\log h)^{-1-\delta}$, *then*

$$s_n = \sum_1^n e\,(\lambda_k\alpha) = o\,(n), \tag{1}$$

and the points $(\lambda_n\ \alpha)$ *are uniformly distributed, for almost all values of* α.

In our second paper in the *Acta* we stated that the equation could, in very many cases, be replaced by the much more precise equation

$$s_n = O\,(n^{\frac{1}{2}+\epsilon}) \tag{2}$$

for every positive ϵ. The publication of Weyl's work had led us to a re-examination of this question and to the following theorems.

A. *If* (i) $\lambda_{n+[n^\epsilon]} - \lambda_n \to \infty$,

(ii) $|a_1|^2 + |a_2|^2 + \ldots + |a_n|^2 = O(n^{1+\epsilon})$,

for every positive ϵ, *then*

$$\text{(iii)} \qquad s_n(\alpha) = \sum_1^n a_k e(\lambda_k \alpha) = O(n^{\frac{1}{2}+\epsilon})$$

for almost all α*'s and every positive* ϵ.

B. *If* (i) *is replaced by* (i′) $\lambda_{n+[n^{\beta+\epsilon}]} - \lambda_n \to \infty$, *where* $0 < \beta < 1$, *then* (iii) *may be replaced by*

$$\text{(iii}'\text{)} \qquad s_n(\alpha) = O\left(n^{\frac{1+\beta}{2}+\epsilon}\right).$$

To these two theorems Weyl's forms a completing third. It should be observed that (i) is certainly satisfied if $\lambda_{n+1} - \lambda_n \geq c > 0$, and in particular if λ_n is always integral, and (ii) if $a_n = O(n^\epsilon)$, and in particular if $a_n = 1$.

If λ_n is an integer, and we separate the real and imaginary parts in the equation (iii), we obtain a theorem concerning a particular system of normal orthogonal functions for the interval (0, 1), viz., the functions $\sqrt{2}\, cos\, 2\pi\lambda_n x$, $\sqrt{2}\sin 2\pi\lambda_n x$. Our argument is then directly extensible to a general orthogonal system, and we are led to a new and interesting proof of Hobson's[6] theorem that *if* $\phi_n(x)$ *is any normal orthogonal system, and* $\Sigma n^\delta |c_n|^2$ *is convergent for some positive* δ, *then* $\Sigma c_n \phi_n(x)$ *is convergent almost everywhere.*

Weyl's hypothesis concerning λ_n asserts, roughly, that the increase of λ_n is appreciably more rapid than that of $(\log n)^2$. It is easy to see that this hypothesis cannot be capable of *much* wider generalisation. For, when $\lambda_n = \log n$, s_n is definitely of order n. It seems probable, too, that the index $\frac{1}{2}(1 + \beta)$ of Theorem *B* is the correct one.

5. We conclude by correcting an error in our recent note. The results concerning the special case $\rho = 0$ are stated wrongly. It is not true that, when $\rho = 0$, $f(z)$ and s_n are bounded; all that we can assert is that they are of the forms $O\left(\log\frac{1}{1-r}\right)$ and $O(\log n)$ respectively. That $f(z)$ should be bounded would contradict a general theorem of Fatou,[7] in virtue of which a bounded function must tend to a limit, for almost all values of θ, when $z = re^{i\theta}$ tends to the circle of convergence along a radius vector. The error has no bearing on the general case.

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ON MOSELEY'S LAW FOR X-RAY SPECTRA

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Communicated by B. B. Boltwood, December 23, 1916

While engaged in making interpolations, by the method of least squares, of unknown from known wave-lengths of high frequency spectra I noticed certain systematic deviations from Moseley's law which led me to investigate three interesting questions that have not been previously discussed, probably because the older data did not seem to be sufficiently accurate to justify close mathematical analysis. These questions are: (i) How accurately does Moseley's law reproduce the observed wave-lengths? (ii) What empirical formula will represent the numerical data within the limits of experimental error? and (iii) What is the order of magnitude of the high frequency radiations of elements having small atomic numbers and of which the spectra have not yet been obtained? In the following paragraphs definite answers will be given to questions (i) and (ii), while a tentative solution of the third problem is necessitated by the fact that it involves extrapolation. The wave-lengths used in the computations were taken from the recent papers by M. Siegbahn, W. Stenström, and E. Friman. These data were chosen because they are the latest, they were all obtained in the same laboratory with the same spectrometers, and they constitute the most extensive, accurate and consistent set available.

Moseley's law is that, for any one series (α, β, γ, etc.), the square-root of the frequency of the lines is a linear function of the atomic numbers of the radiating elements. In symbols $\sqrt{\nu_N} = a + b\,N$, where $\nu_N \equiv$ frequency, $N \equiv$ atomic number, a and b are constants for one series. When a and b are calculated by the method of least squares, from the 48 known wave-lengths of the L-α_1 series, extending from zinc ($N = 30$, $\lambda = 12.346$ A) to uranium ($N = 92$, $\lambda = 0.911$ A), the values

obtained lead to wave-lengths which show a systematic deviation from the experimental data. In other words, a 'smooth' curve can be drawn through the points having as abscissae the atomic numbers and as ordinates the corresponding percentages by which the calculated wave-lengths are algebraically greater than the experimental values. The extreme deviations are +2.00%, −0.62%, and + 0.46% for *Zn*, *Ce*, and *U*, respectively. Since the glancing angles are said to be correct within 0.3% it follows that the extreme interval of deviation (2.62%) must be real and that Moseley's law does not hold exactly over the entire range. In the case of the L-β_1 series for which 46 wave-lengths from arsenic ($N = 33$, $\lambda = 9.449$ A) to uranium ($\lambda = 0.720$ A) are given, another smooth curve of departure is obtained, the extreme deviations being +13.35%, −3.06%, and + 5.84%, corresponding to *As*, *Nd*, and *U*, in the order named. The range 16.41% certainly cannot be accounted for as due only to experimental error. The data for the L-γ_1 series extend from zirconium ($N = 40$, $\lambda = 5.386$) to uranium ($\lambda = 0.615$) and comprise 36 wave-lengths. The deviations for the associated curve of departure are found to be +9.22%, −1.68%, and +3.96% for *Zr*, *Yb*, and *U*, respectively. Unfortunately, only a portion of the literature relating to the *K* series is at present accessible to me. Nevertheless, the 20 wave-lengths of the means of the K-α_1 and K-α_2 series extending from sodium ($N = 11$, $\lambda = 11.951$ A) to germanium ($N = 32$, $\lambda = 1.259$ A), exhibit departures from the linear relation which are greater than the probable errors of the experimental numbers. In this case the original investigators state, and also give data to show, that the wave-lengths for the same line obtained from different negatives agree to within 1 or 2 tenths of one per cent. Just as for the *L*-series, so here, the curve of departure with respect to the linear law deviates more and more as the smallest atomic number is approached. The deviation for magnesium was found to be +0.41%, and for sodium +0.74%. The answer to the first question is, therefore, that Moseley's law does not hold for the entire range of the *L*-series and that it seems to be slightly inexact for the most intense lines of the *K*-series.

The second question proposed may be answered at once. The empirical equation

$$\sqrt{\nu_N} = A + B\,N + C\,(D + N)^{-1}$$

was found to represent the *L*-series well within the limits of experimental error even when the values of the parameters a', b', C, and D were obtained by graphical processes and not by the method of least squares.

This equation results from adding the correction function $a' + b' N + C (D + N)^{-1}$ to Moseley's formula. From the mathematical point of view the correction terms mean that the residuals (δ) of Moseley's law conform to the hyperbola $N\delta + c_1\delta + c_2 + c_3N + c_4N^2 = 0$, which differs from the general conic solely in the omission of the term that would involve the square of the residuals. In the case of the L-β_1 series—for which the linear law shows the greatest departures from the experimental data—the deviations of the more complicated formula were only $+ 0.15\%$, -0.13%, and $+ 0.23\%$ for *As*, *Nd*, and *U*, respectively. In all cases examined B was positive while A, C, and D were negative. D was always an integer and, although the mathematical analysis does not preclude the possibility of fractional values, this peculiarity may be accidental and not may have physical significance.

Evidence in favor of the opinion that the wave-lengths of the K-series fall in the unexplored region between 600 A and 12 A will now be adduced. Substitution of unity for N in Moseley's formula leads to the ***infra-red*** wave-length 138.7μ, in the case of the mean K-α series. When the members of this formula are squared a parabolic equation ***with related coefficients*** is obtained. When N is replaced by 1 in the more general parabolic expression $\nu_N = a_0 + a_1N + a_2N^2$ it is found that $\lambda_1 = 366$ A. Under the same conditions the cubic $\nu_N = b_0 + b_1 N + b_2 N^2 + b_3 N^3$ gives $\lambda_1 = 130$ A. The last equation fits the known wave-lengths better than either of the preceding parabolic formulae, especially for the longest wave-lengths (smallest values of N). The probable error of one residual is $\pm$ 0.183% for Moseley's formula (squared) and $\pm 0.102\%$ for the cubic, that is, nearly twice as good on the whole. Since the extrapolated wave-length becomes shorter as the equation is made to conform more closely to the experimental data and since the values obtained from the power expansions with independent coefficients are both less than 400A it seems plausible to conclude that the experimental wave-lengths when found will fall in the ultra-Lyman region. Extrapolation for the L-series would be premature for the reasons that the interval from $N = 30$ to $N = 1$ is too great and the law of nature is not yet known.

In conclusion, I desire to express the hope that the present paper, which is tentative and by no means final, may stimulate experimental research in the difficult but very important region of the spectrum lying between the shortest wave-length (600 A) published by Lyman and the longest determined by Friman.

A NOTE ON THE FITTING OF PARABOLAS

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Communicated by Raymond Pearl, November 28, 1916

The formulae given by Pearson[2] (pp. 12–16) and Elderton[3] (pp. 30–31) for the fitting of parabolas by the method of moments assume the origin at the mid-point of the range. It being often more convenient to take the origin one unit below the first ordinate, as in working by the method of least squares, I have, at the suggestion of Dr. Raymond Pearl, worked out the formulae which result from such choice of origin.

Let l be the range for which the parabola

$$y = c_0 + c_1x + c_2x^2 + \ldots \ldots + c_nx^n$$

is to be fitted to the observations, and $M_r = S(yx^r)$ where the summation includes the values of x and y for every observation.

Then

$$M_r = \int_{\frac{1}{2}}^{l+\frac{1}{2}} (c_0 + c_1x + c_2x^2 + \ldots \ldots c_nx^n)\, x^r dx$$

$$= \frac{c_0}{r+1}[(l+\tfrac{1}{2})^{r+1} - (\tfrac{1}{2})^{r+1}] + \frac{c_1}{r+2}[(l+\tfrac{1}{2})^{r+2} - (\tfrac{1}{2})^{r+2}] + \ldots$$

$$+ \frac{c_n}{r+n+1}[(l+\tfrac{1}{2})^{r+n+1} - (\tfrac{1}{2})^{r+n+1}]$$

Substituting $r = 0, 1, 2, \ldots \ldots n$ in this formula we have $n+1$ simultaneous equations from the solution of which we may express the c's in terms of the moments and certain functions of l.

(i). $y = c_0 + c_1x,$
$c_0 = K_1M_0 - K_2M_1,$
$c_1 = -K_2M_0 + K_3M_1,$

where

$$K_1 = \frac{1}{l^3}(4l^2 + 6l + 3), \qquad K_2 = \frac{6}{l^3}(l+1), \qquad K_3 = 12/l^3.$$

(ii). $y = c_0 + c_1x + c_2x^2,$
$c_0 = K_4M_0 - K_5M_1 + K_6M_2,$
$c_1 = -K_5M_0 + K_7M_1 - K_8M_2,$
$c_2 = K_5M_0 - K_8M_1 + K_8M_2,$

where

$$K_4 = \frac{9}{l^5}\left(l^4 + 4l^3 + 7l^2 + 5l + \frac{5}{4}\right), \quad K_5 = \frac{9}{l^5}(l+1)(4l^2 + 10l + 5),$$

$$K_6 = \frac{30}{l^5}\left(l^2 + 3l + \frac{3}{2}\right), \quad K_7 = \frac{12}{l^5}(16l^2 + 30l + 15),$$

$$K_8 = \frac{180}{l^5}(l+1), \quad K_9 = 180/l^5.$$

(iii). $y = c_0 + c_1x + c_2x^2 + c_3x^3,$

$$\begin{aligned} c_0 &= K_{10}M_0 - K_{11}M_1 + K_{12}M_2 - K_{13}M_3, \\ c_1 &= -K_{11}M_0 + K_{14}M_1 - K_{15}M_2 + K_{16}M_3, \\ c_2 &= K_{12}M_0 - K_{15}M_1 + K_{17}M_2 - K_{18}M_3, \\ c_3 &= -K_{13}M_0 + K_{16}M_1 - K_{18}M_2 + K_{19}M_3, \end{aligned}$$

where

$$\begin{aligned} K_{10} &= \frac{1}{4l^7}(64l^6 + 480l^5 + 1680l^4 + 2840l^3 + 2460l^2 + 1050l + 175), \\ K_{11} &= \frac{15}{2l^7}(l+1)(16l^4 + 96l^3 + 188l^2 + 140l + 35), \\ K_{12} &= \frac{15}{l^7}(16l^4 + 104l^3 + 192l^2 + 140l + 35), \\ K_{13} &= \frac{70}{l^7}(l+1)(2l^2 + 10l + 5), \\ K_{14} &= \frac{75}{l^7}(16l^4 + 72l^3 + 120l^2 + 84l + 21), \\ K_{15} &= \frac{900}{l^7}(l+1)\left(3l^2 + 7l + \frac{7}{2}\right), \\ K_{16} &= \frac{420}{l^7}(4l^2 + 10l + 5), \quad K_{17} = \frac{180}{l^7}(36l^2 + 70l + 35), \\ K_{18} &= \frac{4200}{l^7}(l+1), \quad K_{19} = 2800/l^7. \end{aligned}$$

The values of the K's for values of l up to 30 are given in Table II.

The fitting of the following observations, given by Thiele[4] (p. 12) and used by Pearson[2] to illustrate his formulae for fitting parabolas, will serve as an example. Table I shows the calculations to obtain the moments and the resulting parabolas. It is obvious that these data are in no way suited to graduation by parabolas, being really a unimodal frequency distribution. They will, however, serve for illustration of method.

The origin for moments is taken at $X = 6$ and the successive moments corrected by Sheppard's formula (λ)[5] (p. 276).

TABLE I

X	y	x	yx	yx^2	yx^3	Parabolas 1st	2nd	3rd
7	3	1	3	3	3	57.5	11.0	−14.1
8	7	2	14	28	56	54.3	30.9	32.2
9	35	3	105	315	945	51.2	46.6	62.2
10	101	4	404	1,616	6,464	48.0	58.1	78.4
11	89	5	445	2,225	11,125	44.9	65.4	83.1
12	94	6	564	3,384	20,304	41.7	68.5	78.9
13	70	7	490	3,430	24,010	38.5	67.5	68.0
14	46	8	368	2,944	23,552	35.4	62.2	53.0
15	30	9	270	2,430	21,870	32.2	52.8	36.2
16	15	10	150	1,500	15,000	29.1	39.1	20.0
17	4	11	44	484	5,324	25.9	21.3	6.8
18	5	12	60	720	8,640	22.8	−0.7	−0.8
19	1	13	13	169	2,197	19.6	−26.9	−0.7
Totals......	500	......	2,930	19,248	139,490	....	....	....

$$M_0 = 501.031015 \qquad M_2 = 19190.3584$$

$$l = 13$$

$$M_1 = 2929.06288 \qquad M_3 = 138630.787$$

(i).

$$c_0 = 0.344561M_0 \quad - 0.0382340M_1 = 60.6459$$
$$c_1 = -0.0382340M_0 + 0.00546199M_1 = -3.15791$$

(ii).

$$c_0 = 0.935607M_0 - 0.275217M_1 + 0.0169273M_2 = -13.1046$$
$$c_1 = -0.275217M_0 + 0.100481M_1 - 0.00678709M_2 = 26.1762$$
$$c_2 = 0.0169273M_0 - 0.00678709M_1 + 0.000484792M_2 = -2.095379$$

(iii).

$$c_0 = 2.158569M_0 - 1.173878M_1 + 0.172060M_2 - 0.00738727M_3 = -79.062$$
$$c_1 = -1.173878M_0 + 0.760838M_1 - 0.120782M_2 + 0.00542834M_3 = 75.0781$$
$$c_2 = 0.172060M_0 - 0.120782M_1 + 0.0201633M_2 - 0.000937074M_3 = -10.53703$$
$$c_3 = -0.00738727M_0 + 0.00542834M_1 - 0.000937074M_2 + 0.0000446226M_3 = 0.401978$$

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TABLE II

VALUES OF THE K'S FOR VALUES OF l FROM 2 TO 30

l	K_1	K_2	K_3	K_4	K_5	K_6	K_7	K_8	K_9	K_{10}
2	3.875000	2.250000	1.500000							
3	2.111111	0.888889	0.444444	9.935185	10.518519	2.407407	12.296296	2.962963	0.740741	
4	1.421875	0.468750	0.187500	5.671143	4.790039	0.864258	4.582031	0.878906	0.175781	21.503281
5	1.064000	0.288000	0.0960000	3.819600	2.678400	0.398400	2.169600	0.345600	0.0576000	12.710160
6	0.847222	0.194444	0.0555556	2.827836	1.693287	0.214120	1.189815	0.162037	0.0231481	8.577156
7	0.702624	0.139942	0.0349854	2.223493	1.160945	0.127625	0.720414	0.0856786	0.0107098	6.293770
8	0.599609	0.105469	0.0234375	1.821876	0.842926	0.0819397	0.468384	0.0494385	0.00549316	4.888443
9	0.522634	0.0823045	0.0164609	1.537913	0.638622	0.0556318	0.321292	0.0304832	0.00304832	3.954524
10	0.463000	0.0660000	0.0120000	1.327613	0.499950	0.0394500	0.229800	0.0198000	0.00180000	3.297417
11	0.415477	0.0540947	0.00901578	1.166179	0.401686	0.0289660	0.169959	0.0134119	0.00111766	2.814296
12	0.376736	0.0451389	0.00694444	1.038674	0.329608	0.0218822	0.129196	0.00940394	0.000723380	2.446523
13	0.344561	0.0382340	0.00546199	0.935607	0.275217	0.0169273	0.100481	0.00678709	0.000484792	2.158569
14	0.317420	0.0327988	0.00437318	0.850682	0.233190	0.0133594	0.0796766	0.00502023	0.000334682	1.927824
15	0.294222	0.0284444	0.00355556	0.779570	0.200059	0.0107259	0.0642370	0.00379259	0.000237037	1.739302
16	0.274170	0.0249023	0.00292969	0.719203	0.173490	0.00874043	0.0525398	0.00291824	0.000171661	1.582725
17	0.256667	0.0219825	0.00244250	0.667349	0.151862	0.00721552	0.0435171	0.00228192	0.000126773	1.450834
18	0.241255	0.0195473	0.00205761	0.622348	0.134026	0.00602519	0.0364464	0.00180994	0.0000952599	1.338372
19	0.227584	0.0174953	0.00174953	0.582942	0.119147	0.00508259	0.0308275	0.00145390	0.0000726950	1.241451
20	0.215375	0.0157500	0.00150000	0.548160	0.106608	0.00432656	0.0263063	0.00118125	0.0000562500	1.157134
21	0.204406	0.0142533	0.00129576	0.517241	0.0959433	0.00371318	0.0226273	0.000969614	0.0000440733	1.083170
22	0.194497	0.0129602	0.00112697	0.489582	0.0867984	0.00321036	0.0196033	0.000803317	0.0000349268	1.017803
23	0.185502	0.0118353	0.000986274	0.464698	0.0788982	0.00279429	0.0170948	0.000671189	0.0000279662	0.959649
24	0.177300	0.0108507	0.000868056	0.442194	0.0720271	0.00244706	0.0149966	0.000565140	0.0000226056	0.907601
25	0.169792	0.00998400	0.000768000	0.421748	0.0660142	0.00215501	0.0132280	0.000479232	0.0000184320	0.860762
26	0.162893	0.00921711	0.000682749	0.403092	0.0607225	0.00190761	0.0117269	0.000409044	0.0000151498	0.818402
27	0.156531	0.00853528	0.000609663	0.386002	0.0560413	0.00169664	0.0104446	0.000351246	0.0000125445	0.779919
28	0.150647	0.00792638	0.000546647	0.370291	0.0518804	0.00151566	0.00934251	0.000303306	0.0000104588	0.744814
29	0.145188	0.00738038	0.000492025	0.355798	0.0481655	0.00135950	0.00839017	0.000263271	0.00000877572	0.712667
30	0.140111	0.00688889	0.000444444	0.342389	0.0448352	0.00122407	0.00756296	0.000229630	0.00000740741	0.683124

TABLE II—*Continued*

l	K_{11}	K_{12}	K_{13}	K_{14}	K_{15}	K_{16}	K_{17}	K_{18}	K_{19}
4	31.684113	13.200989	1.644897	50.267029	21.835327	2.794189	9.788818	1.281738	0.170898
5	15.802560	5.478720	0.564480	21.543360	7.845120	0.833280	2.960640	0.322560	0.0358400
6	9.211168	2.732071	0.239805	11.020287	3.454540	0.313572	1.125900	0.105024	0.0100023
7	5.942932	1.539282	0.117638	6.338558	1.744171	0.138208	0.500302	0.0407993	0.00339994
8	4.115850	0.945761	0.0639868	3.961551	0.971389	0.0682926	0.248823	0.0180244	0.00133514
9	3.002575	0.619821	0.0376126	2.633735	0.582379	0.0367930	0.134766	0.00878116	0.000585410
10	2.278939	0.426953	0.0234850	1.836457	0.369765	0.0212100	0.0780300	0.00462000	0.000280000
11	1.784351	0.305961	0.0153886	1.329925	0.245792	0.0129100	0.0476714	0.00258632	0.000143684
12	1.432497	0.226412	0.0104887	0.993184	0.169630	0.00821672	0.0304372	0.00152379	0.0000781429
13	1.173878	0.172060	0.00738727	0.760838	0.120782	0.00542834	0.0201633	0.000937074	0.0000446226
14	0.978564	0.133710	0.00534894	0.595470	0.0883023	0.00370142	0.0137817	0.000597646	0.0000265621
15	0.827653	0.105906	0.00396583	0.474636	0.0660333	0.00259336	0.00967638	0.000393306	0.0000163877
16	0.708755	0.0852712	0.00300121	0.384331	0.0503568	0.00186034	0.00695430	0.000265986	0.0000104308
17	0.613490	0.0696446	0.00231219	0.315508	0.0390650	0.00136234	0.00510120	0.000184238	0.00000682363
18	0.536035	0.0575996	0.00180963	0.262152	0.0307661	0.00101601	0.00381010	0.000130345	0.00000457352
19	0.472243	0.0481693	0.00143622	0.220159	0.0245572	0.000770110	0.00289189	0.0000939732	0.00000313244
20	0.419102	0.0406832	0.00115418	0.186662	0.0198376	0.000592266	0.00222680	0.0000689063	0.00000218750
21	0.374382	0.0346664	0.000937977	0.159620	0.0161987	0.000461488	0.00173705	0.0000513023	0.00000155462
22	0.336404	0.0297764	0.000770030	0.137551	0.0133568	0.000363869	0.00137102	0.0000387274	0.00000112253
23	0.303885	0.0257619	0.000637989	0.119365	0.0111114	0.000290006	0.00109375	0.0000296050	0.000000822362
24	0.275833	0.0224359	0.000533035	0.104245	0.00931844	0.000233421	0.000881109	0.0000228934	0.000000610491
25	0.251470	0.0196572	0.000448774	0.0915717	0.00787282	0.000189579	0.000716194	0.0000178913	0.000000458752
26	0.230180	0.0173180	0.000380503	0.0808698	0.00669688	0.000155255	0.000586964	0.0000141189	0.000000348614
27	0.211468	0.0153349	0.000324719	0.0717710	0.00573245	0.000128124	0.000484727	0.0000112425	0.000000267677
28	0.194938	0.0136501	0.000278782	0.0639863	0.00493549	0.000106487	0.000403131	0.00000902695	0.000000207516
29	0.180263	0.0121901	0.000240680	0.0572869	0.00427229	0.0000890893	0.000337474	0.00000730440	0.000000162320
30	0.167177	0.0109362	0.000208864	0.0514902	0.00371681	0.0000749931	0.000284239	0.00000595336	0.000000128029

AXES OF SYMMETRY IN GLOBULAR CLUSTERS

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Communicated by G. E. Hale, January 10, 1917

The distribution of stars in globular clusters has been extensively studied in the past, chiefly from the standpoint of the groupings and superficial arrangements of the brightest stars or on the basis of the variation of density with distance from the center.[1] The distribution with respect to direction as well as distance has been considered briefly for ten clusters by Bailey, who found a number of asymmetrical irregularities among the brighter stars.[2] But deductions relative to the basic structure of globular clusters cannot be founded safely on the most luminous objects alone, for investigations of the colors in these systems show that the brighter stars are peculiar and perhaps not at all representative of the enormous number of faint stars which constitute the greater part of the mass.

Several points wherein globular clusters resemble our galactic system of stars have been noted in recent papers, and it was suggested that planes of symmetry may characterize the distribution of stars in clusters, as does the Galaxy that of the stars in general.[3] Although the thousand brightest stars in Messier 13 gave no definite evidence of an elliptical distribution, a study with respect to direction from the center of the star-counts published for the bright southern cluster ω Centauri[4] did verify the photographic appearance of an elongated form. In this cluster 6400 stars were counted, covering a considerable interval of apparent (and absolute) magnitude, while for only two of the ten clusters discussed by Bailey did the number appreciably exceed one thousand.

The photographs of clusters with the 60-inch reflector at Mount Wilson afford material particularly suited to the study of this problem. Plates have been obtained for nearly twenty different systems, and for some clusters exposures varying in length from one minute to several hours are available. A few plates record stars fainter than the twentieth magnitude. The method and progress of the counting have previously been reported.[5] Briefly, the study of star density on each plate is based on counts made with the aid of a superposed reseau of small squares $31''.4$ on a side. The details of the observations and counts will appear eventually in the *Contributions of the Mount Wilson Solar Observatory*. The present paper indicates one of the most important results, considered somewhat in detail for the well-known Hercules

cluster, Messier 13, and more generally for the other clusters for which plates of sufficiently long exposure are available.

The data have been arranged in a system of twelve equal sectors, and these subdivided by a series of concentric circles, according to the scheme shown in figure 1. Except for the shortest exposures, the counts within a minute or two of the center are uncertain or impossible, and for the inner ring are generally ignored in the discussion. In general, results for all such crowded regions are less reliable because of possible influence of the Eberhard effect or of similar photographic phenomena. The tedious and difficult task of counting and tabulating more than 500,000 star images was performed by Miss Van Deusen

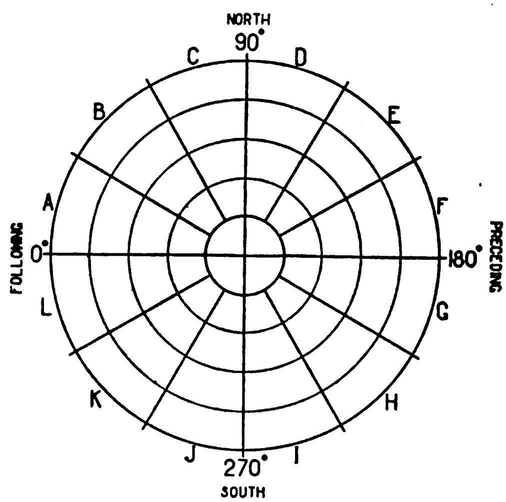

FIG. 1

of the Computing Division. Miss Richmond, also of the Computing Division, has assisted greatly in the arrangement and tabulation of the counts for the study of distribution with respect to direction from the center.

Table I and figure 2 show for Messier 13 the number of stars for successive sectors on several plates with increasing exposure time. The limits of distance from the center are 2′ and 10′, except for Plate 133, for which they are 3′ and 9′. The totals in the third column include the count or estimate of stars in the center, and also those farther from the center than the limiting distance used in the tabulation for the sectors in columns 4 to 15.

The two maxima and the two minima in each curve show immediately the presence of a well defined axis of symmetry. For plates containing only one or two thousand stars the elliptical form is little if at all

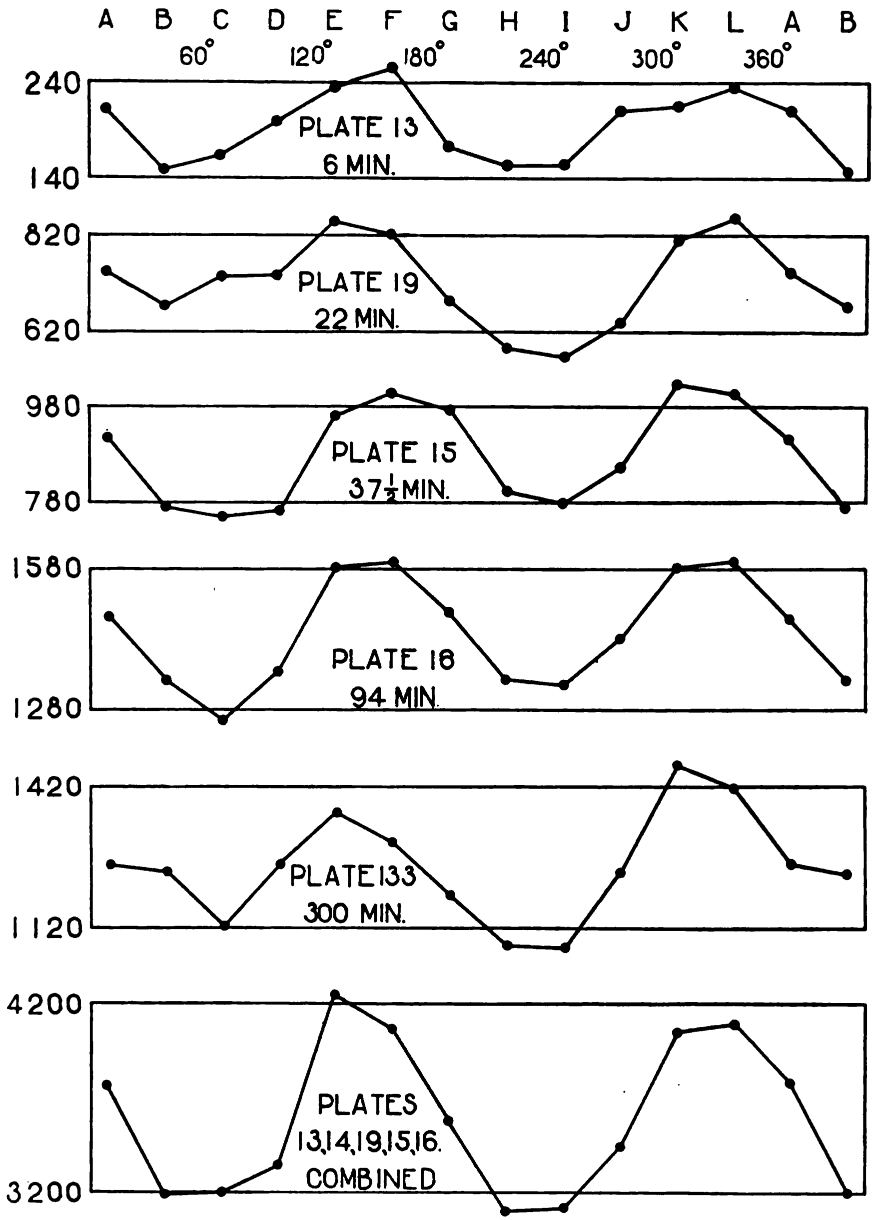

FIG. 2. AXIS OF SYMMETRY IN MESSIER 13. ORDINATES ARE NUMBERS OF STARS; ABSCISSAE ARE 30° SECTORS.

evident. To overcome the small or accidental groupings of the stars it appears necessary to have several hundred in each sector. No important systematic differences between diametrically situated sectors appear in figure 2, and, to eliminate possible errors of centering, the mean of the numbers for opposite sectors may be combined in discussing the results.

TABLE I

ELLIPTICITY FOR DIFFERENT EXPOSURES AND MAGNITUDE INTERVALS IN MESSIER 13

PLATE NO.	DURATION OF EXPOSURE	TOTAL NO. OF STARS	NUMBER OF STARS IN SECTORS											
			A	B	C	D	E	F	G	H	I	J	K	L
13	6m	5,800	211	149	163	198	235	256	174	154	154	213	214	235
14	15	7,700	433	264	296	305	309	401	259	230	284	314	420	396
19	22	14,150	744	672	734	738	852	825	684	583	569	638	814	859
15	37.5	16,600	913	770	749	763	963	1011	974	804	779	853	1026	1008
16	94	25,000	1475	1340	1261	1361	1580	1590	1486	1343	1338	1431	1580	1590
133	300	30,000	1254	1234	1126	1258	1368	1300	1187	1085	1079	1232	1463	1416
15 *minus* 13			702	621	586	565	728	755	800	650	625	640	812	773
16 *minus* 15			562	570	512	598	617	579	512	539	559	578	554	582

TABLE II

ELLIPTICITY AND DISTANCE FROM CENTER IN MESSIER 13

PLATE NO.	DISTANCE FROM CENTER	NUMBER OF STARS IN SECTORS											
		A	B	C	D	E	F	G	H	I	J	K	L
16	2′ to 4′	750	668	623	670	718	778	758	683	712	728	764	762
	4 to 6	423	361	358	394	479	438	402	352	330	386	476	464
	6 to 8	212	207	168	198	249	248	214	194	188	202	226	236
	8 to 10	90	104	112	99	134	126	112	114	108	115	114	128
133	3 to 5	624	640	560	662	712	658	629	597	586	642	788	684
	5 to 7	410	374	362	394	424	396	358	292	302	360	431	471
	7 to 9	220	220	204	202	232	246	200	196	191	230	244	261
	9 to 11	116	136	141	100	134	157	130	124	131	129	130	118

The relation of the ellipticity to brightness is also shown in Table I. Thus Plate 13 includes stars between photographic magnitudes 13 and 17.3; the next to last row of the table refers to the interval 17.3 to 19, and the last row to the interval 19 to 20. The magnitudes depend mostly on extrapolations and are subject to some uncertainty.

The plates of long exposure, Nos. 16 and 133, show a sufficient number of stars in each sector to permit a subdivision with respect to distance, the results of which are in Table II. Because of the relatively small number of stars, accidental deviations greatly mask whatever ellipticity exists in the outer ring. The counts of stars within distance 2′ on the plates of shorter exposure also show the elliptical form, and give the same general orientation.

So far as this analysis goes, therefore, the axis of symmetry in Messier 13 appears to be independent of magnitude, length of exposure, and distance from the center. It can be located visually on some of the photographs of the cluster, particularly in the burned-out central portion of the longer exposures.

TABLE III

EVIDENCE OF ELLIPTICITY IN OTHER GLOBULAR CLUSTERS

CLUSTER	PLATE NO.	DURATION OF EXPOSURE	TOTAL NO. OF STARS	NUMBER OF STARS IN SECTORS											
				A	B	C	D	E	F	G	H	I	J	K	L
M2	5	100.0m	6,500	421	404	463	508	528	521	458	416	429	458	511	464
N.G.C. 5024	102	180.0	10,100	655	686	729	730	738	699	686	692	724	679	730	656
M15	23	37.5	9,000	343	344	350	322	338	318	336	410	374	313	320	329
	24	94.0	20,500	1057	1068	1126	1070	1056	982	1103	1194	1158	1077	1044	1076
	25	312.0	26,000	1284	1357	1430	1366	1354	1316	1391	1446	1306	1322	1311	1278
M10	117	60.0	5,800	288	261	290	327	324	299	284	275	316	278	297	344
	114	180.0	12,200	810	839	801	777	818	814	798	792	836	816	830	861

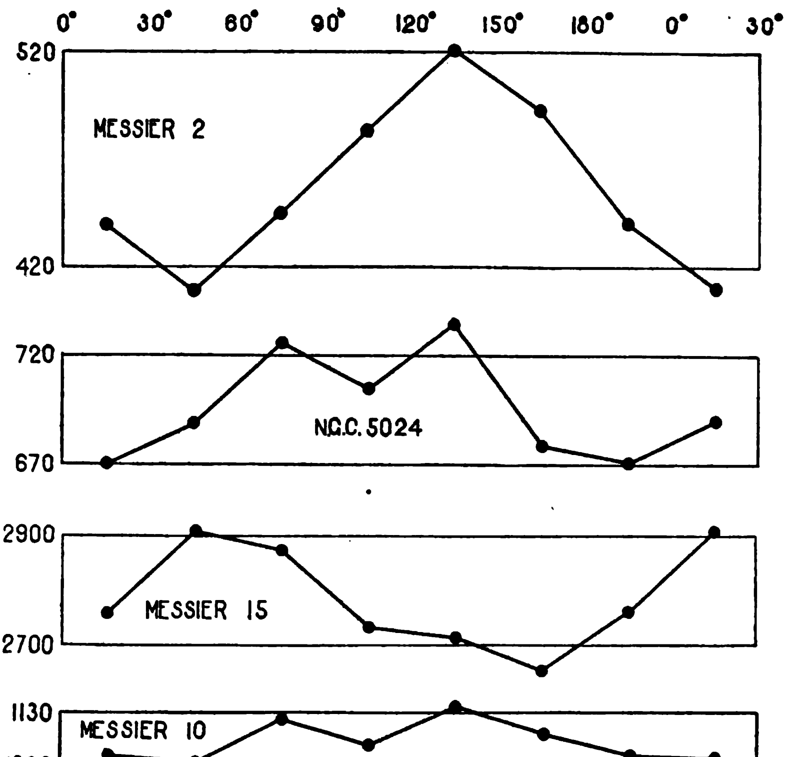

FIG. 3. AXES OF SYMMETRY IN GLOBULAR CLUSTERS. ORDINATES ARE NUMBERS OF STARS; ABSCISSAE ARE ANGLES OF DIRECTION.

An elliptical distribution of stars is not confined to the Hercules cluster. Counts for four other systems are summarized in Table III and plotted in figure 3. Three of them show unmistakable signs of elliptical form; the fourth, Messier 10, is a cluster with noticeably less condensation toward the center than usual. If the axes of symmetry in the others represent the projections of galactic-like planes, it is possible that in Messier 10 there is such a plane of symmetry inclined nearly 90° to the line of sight. The inclination to the equator of the projected major axis of Messier 13 is 152° (angle counted from *Following* through *North*). For Messier 2 and N. G. C. 5024 the inclination is 133° and 105°, respectively, while for Messier 15, which is across the Milky Way, it is 60° and is nearly parallel to the galactic plane.

[1] A bibliography of the more important of these investigations is given in *Mt. Wilson Contrib.* No. 115, (3–10), and No. 116, (4–8).

[2] Bailey, S. I., *Cambridge, Ann. Obs. Harvard Coll.*, 76, (43–82).

[3] Shapley, H., *Observatory, London*, 39, 1916, (452–456).

[4] Bailey, S. I., *Astr. and Astroph., Northfield, Minn.*, 12, 1893, (689–692).

[5] *Washington, Carnegie Inst., Year Book*, 12, 1913, (213); 13, 1914, (258).

THE SHARE OF EGG AND SPERM IN HEREDITY

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Read before the Academy November 13, 1916.

1. Assumed Equivalence of Inheritance from Both Parents.—Practically all students of heredity are agreed that there is a general equivalence of inheritance from father and mother, and O. Hertwig (1892) cites this as one of the evidences that the chromosomes only contain inheritance material, or 'Erbmasse,' since they alone come in approximately equal volumes from the two parents. Indeed phenomena of Mendelian inheritance demonstrate that, with respect to those characters which usually distinguish the two parents, there is equivalence of inheritance from each, and where offspring resemble one parent more than the other they are probably as frequently patroclinous as matroclinous. Furthermore, the distribution of chromosomes in maturation, fertilization and cleavage is exactly parallel to the distribution of Mendelian factors, which practically demonstrates that the chromosomes are the seat of these factors.

This conclusion has led many students of heredity to regard the cytoplasm of the germ cells as of no significance in heredity. Both egg and sperm contain cytoplasm which is differentiated, in the former for the

nutrition of the embryo, in the latter for bringing the sperm into union with the egg; but in neither case is this differentiated cytoplasm directly concerned in heredity. The highly differentiated cytoplasm of the spermatozoon is either left outside the egg when its nucleus enters, or it undergoes de-differentiation within the egg; at the same time the egg cytoplasm ceases to form yolk, while the yolk which has been formed is gradually used up in the nourishment of the embryo. Consequently since these particular differentiations of the germ cells disappear after the union of egg and sperm it has been generally supposed that all cytoplasmic differentiations of these cells are wiped out at this time, and that the first differentiations of the new individual begin at the moment of fertilization in a wholly undifferentiated cytoplasm.

In the higher animals at least most of the cytoplasmic differentiations of the spermatozoon are lost after it enters the egg, though some differentiations such as centrosome, plastosomes and archiplasm may persist; however there is the most positive evidence that many differentiations of the egg cytoplasm persist and play an important part in embryonic differentiation.

2. *Egg Differentiations which persist in Embryo and Adult.*—(*1*) *Polarity.* The polarity of the egg invariably determines the polarity of the embryo and adult. In all animals the chief axis of the egg becomes the chief axis of the gastrula, and this becomes the chief axis of the adult in sponges and coelenterates (protaxonia), or, as in all other metazoa (heteraxonia), this axis is bent on itself by the greater growth of the gastrula on its posterior side so that the chief axis of the adult is a modification of the gastrular axis. In either case the polarity of the unfertilized egg determines the localization of developmental processes and ultimately the polarity of the developed animal.

(*2*) *Symmetry.* In most animals the egg is spherical in shape and appears to be radially symmetrical, nevertheless observation and experiment show that such eggs are sometimes bilateral, as is probably the case in Amphioxus, ascidians, fishes and frogs. In the case of the frog's egg it was long believed that the plane of bilateral symmetry was determined wholly and exclusively by the path of the spermatozoon within the egg; more recently it has been shown by Brachet (1911) that primary bilateral symmetry is present before fertilization, though after fertilization the plane of symmetry may be shifted into the path of the spermatozoon. It is probable that all bilateral animals come from eggs which show a similar primary bilaterality and that this differentiation precedes fertilization. In cephalopods and some insects all the axes and poles of the developed animal are already recognizable

in the egg before fertilization. Symmetry, therefore, as well as polarity is derived from the egg and not from the sperm.

(*3*) *Inverse Symmetry* (*Asymmetry*). In many animals the right and left sides of the body are not completely alike, and this is especially true of internal organs. This asymmetry is especially well developed in gasteropods in which certain organs of one side of the body are entirely lacking; some species or individuals have these asymmetrical organs on one side, others on the other side, and correspondingly the snail shell coils in a clock-wise direction in one case, an anticlock-wise direction in the other. It was discovered by Cramptom (1894) and Kofoid (1894) that in sinistral species the direction of certain cleavages of the egg (*viz.* the third to the sixth) was the reverse of the corresponding cleavages in dextral species and Conklin (1903) showed that the first and second cleavages also were in opposite directions in dextral and sinistral snails. Consequently the 'inverse symmetry' of these snails may be traced back through the later and earlier cleavage stages to the unsegmented egg itself which is inversely symmetrical in sinistral as compared with dextral forms.

(*4*) *Types of Egg Organization.* The polar differentiation of an egg is manifested particularly in the localization of different kinds of materials in different parts of the egg. These materials may be inert pigment or yolk, but their localization by the activity of the cytoplasm indicates a definite pattern of organization in the cytoplasm. This pattern of egg cytoplasm differs greatly in certain phyla, there being a coelenterate type, an echinoderm type, an annelid-mollusk type, and a chordate type. The type of egg organization foreshadows the type of adult organization; in ascidians for instance distinct cytoplasmic substances are found in the egg in the same relative positions and proportions as the ectoderm, endoderm, mesoderm, notochord and nervous system of the embryo.

That the fundamental pattern of egg cytoplasm is not influenced by the spermatozoon is proved by the following facts:

a. It exists before fertilization, or it appears so soon after that it could not have been caused by the sperm.

b. In heterogeneous fertilization the pattern of the egg is not changed by the foreign sperm.

c. Natural or artificial parthenogenesis demonstrates that this pattern exists in the absence of fertilization.

These as well as other facts such as the correspondence between the size of the egg and the size of the embryo (Morgan); the transmission of chromatophores and peculiarities of leaf coloration by the fe-

male and not by the male germ cells in plants (Baur, Shull); the transmission in the egg cytoplasm of fat stains, chemical substances, immunizing bodies and possibly parasites prove that, "at the time of fertilization the hereditary potencies of the two germ cells are not equal, all the early development, including the polarity, symmetry, type of cleavage, and the relative positions and proportions of future organs being predetermined in the cytoplasm of the egg cell, while only the differentiations of later development are influenced by the sperm. In short, the egg cytoplasm fixes the type of development and the sperm and egg nuclei supply only the details" (Conklin 1908).

Ontogeny begins with the differentiation of the egg in the ovary and not at the moment of fertilization; at the latter time some of the most general and fundamental differentiations have already occurred. Indeed the cytoplasm of the egg is the more or less differentiated body of the embryo.

3. *Is Inheritance through the Egg Cytoplasm Non-Mendelian?*—Whenever a character is transmitted as such through the egg cytoplasm and not as factors in the chromosomes of egg and sperm it is not inherited in Mendelian fashion. Thus if chromatophores are transmitted from generation to generation in the cytoplasm of the egg and are at no time influenced by the sperm, their mode of inheritance is non-Mendelian. If the polarity, symmetry and pattern of the egg do not arise during oogenesis, but are carried over unchanged from generation to generation they are also non-Mendelian characters. With regard to the polarity of the egg, it is not certain whether it is transmitted in this manner or not; but its symmetry and pattern of organization are evidently developed anew in each generation. It is a significant fact that in oogonia and spermatogonia the volume of the nucleus is sometimes greater than that of the cytoplasm, and in all cases it is relatively greater in early stages of the genesis of the sex cells than in later ones. In general the relative volume of nucleus and cytoplasm is a good measure of the differentiation of the latter. Most of the cytoplasmic differentiations of the egg and sperm arise during the genesis of those cells, just as in the case of tissue cells. Nerve cells and muscle cells differentiate under the influence of maternal and paternal chromosomes, and undoubtedly the same is true of most of the differentiations of egg and sperm; but while some of these egg differentiations persist in the new individual those of the sperm do not. Consequently, in each generation the egg contributes more than the sperm to ontogeny. There is cytoplasmic inheritance through the female only, but these cytoplasmic characters are themselves of biparental origin.

This is Mendelian inheritance though somewhat complicated by the fact that every ontogeny has its beginnings in the preceding generation. Therefore, the conclusion which I recently expressed (Conklin 1916) *viz.* that polarity, symmetry and pattern of egg organization are non-Mendelian characters, is not justified.

Somewhat similar phenomena have been described by McCracken (1909); and Toyama (1913) in silk worms and by several authors (Locke, Castle, *et al*) in the seed characters of maize, wheat, etc. McCracken found that when two races of the silk worm are crossed one of which produces one brood a year (univoltin) and the other two (bivoltin), the F_1 offspring are all like their mother and in the F_2 generation "the broods fail to follow both parents in the expected proportions. Therefore," she concludes, "the Mendelian law does not hold in this case." Castle (1910) has criticised this conclusion and has explained these results on the ground that voltinism is inherited through the egg and that univoltinism is a Mendelian dominant to bivoltinism. Toyama (1913) has described in detail the mode of inheritance of several egg characters of silk worms and has shown that whereas these seem to be non-Mendelian they are in reality Mendelian, "the cause of disturbance of the proper order being due to the fact of maternal inheritance, in which paternal characteristics remain dormant, even though dominant in the egg stage."

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A CONTRIBUTION TO THE PETROGRAPHY OF THE ISLAND OF BAWÉAN, NETHERLANDS INDIES

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Communicated, December 21, 1916

The mountainous island of Bawéan, between Java and Borneo, was formerly a group of volcanoes, now extinct and considerably eroded. It is about 9 miles in diameter, and the highest peaks are 2000 feet above the sea. The original form of the cones and craters has disappeared,

and sharp peaks with steep spurs and deep valleys, covered with tropical vegetation, constitute the dominant features of this picturesque and thickly populated island.

Verbeek, who with Fennema described Bawéan in their report on Java and Madura, noted the presence of leucitic and nephelitic lavas, together with trachytes, andesites and intermediate varieties. During a visit of four weeks in 1914 one of us collected rocks from numerous localities and studied the relations of the lavas to one another, so far as the covering of soil and vegetation would permit. Some of the specimens collected have been analyzed by Dr. Morley, and are the first rocks from Bawéan to be analyzed, so far as known to the writers.

The bulk of the lavas appear to be dark basaltic rocks with small phenocrysts of augite. They vary somewhat in habit, owing to the size and prominence of the phenocrysts and the denseness of the groundmass. They occur mostly as breccias, or tuffs containing blocks of various sizes, and to a less extent they are massive flows and dikes. They appear to constitute the body of the central mountains, and to be the older, or main mass of erupted material. With these basaltic rocks are associated lighter colored massive lavas, phonolites and trachytes, which form hills, spurs and ridges in all parts of the island. They appear to have been erupted from fissures, possibly from parasitic cones, on the flanks of the basaltic volcanoes, and probably were the latest eruptions in the district. However, no definite relations, or contacts, of the various bodies of lava were observed, the relative ages of the basaltic and phonolitic lavas being indicated by their modes of occurrence, which are like those of similar lavas in other volcanic districts in this part of the world.

Upon microscopical study it is found that the basaltic lavas are almost wholly leucite-bearing olivine-augite rocks with subordinate amounts of feldspar, both calcic plagioclase and alkalic feldspar, in part anthoclase, besides nephelite. These minerals vary in relative amounts in different lavas, and are variously developed according to the degree to which the rocks have been cystallized. In some instances the constituents of the groundmass are so minute as to be obscure and identifiable with difficulty. In such cases it is the alkalic feldspar and nephelite or leucite that is ill-defined. Judging by the better crystallized rocks, three of which have been analyzed chemically, analyses 1, 2, 3, the basaltic rocks are mostly vicoites, that is, leucite-nephelite-basalts with subordinate amounts of alkalicalcic feldspar and orthoclase. Some have more plagioclase and approach leucite-basanites or leucite-tephrites.

No rocks were collected which could be classed as andesites, nothing resembling the andesitic lavas so common on Java.

The rock, whose chemical composition is shown in analysis 1, is dark colored with abundant phenocrysts of augite, which are green in thin section, and with fewer of olivine. The groundmass is holocrystalline, and consists of leucite and prismoids of plagioclase, partly inclosed in poikilitic orthoclase and nephelite. There is some magnetite. The mode is richer in leucite than the norm, and poorer in nephelite, owing to the presence of albite molecules in the lime-soda-feldspars. There is less orthoclase also in the mode than in the norm.

The rock, from which analysis 2 was made, is from the same locality as the first, but differs from it somewhat in habit, being more distinctly porphyritic, with phenocrysts of green augite which are markedly zonal in thin section. The groundmass consists of augites, small olivines, pronounced leucites, with less conspicuous prismoids of plagioclase, and some magnetite. The ill-defined matrix probably contains orthoclase and nephelite. The mode of this rock is much richer in leucite than the norm, with other corresponding differences in the felsic constituents. These two rocks occur at the Falls of Grotjokan, near the center of the island.

The rock of analysis 3 occurs on the southwest coast of the island. It is light gray, with small, inconspicuous, phenocrysts of augite and scattered olivines. The holocrystalline groundmass consists of anhedral, poikilitic, lime-soda-feldspars, with microcline-like polysynthetic twinning, besides poikilitic orthoclase. Both kinds of feldspar inclose euhedral microlites of leucite and nephelite. Thic rock is more feldspathic than the vicoites just described, and its chemical analysis and norm show that it is transitional to the rock of analysis 1, and that it belongs in the magmatic division II. 7. 2. 3 of the Quantitative System of Classification. It is proposed to name this division, II. 7. 2. 3, baweanose, for, although the rock from Tandjung Anjer is transitional in composition, other varieties of lava will probably be found on Bawéan which will be still more salic, and will have a composition corresponding to the symbol II. 7. 2. 3.

The phonolitic lavas of the island belong to several varieties. Most of them are nephelite rocks with small phenocrysts of orthoclase and nephelite in a groundmass of alkalic feldspars, nephelite and pale green augite, with some sodalite; others, with similar phenocrysts, consist of minute enhedral nephelites, anhedral, microperthetic, alkalic feldspar, and clusters of poikilitic aegirite-augite with small prismoids of what is

TABLE OF CHEMICAL ANALYSIS AND NORMS OF BAWÉAN LAVAS

	1	2	3	4	5	6
SiO_2	45.85	45.81	47.76	55.89	55.25	53.58
Al_2O_3	13.88	15.61	17.29	20.93	22.58	21.30
Fe_2O_3	3.89	3.98	2.89	1.82	1.85	2.16
FeO	4.81	4.94	3.55	0.42	0.58	1.17
MgO	7.69	4.78	4.22	0.32	0.06	0.71
CaO	11.62	11.66	8.06	1.23	1.35	2.89
Na_2O	3.08	3.27	4.70	10.06	8.85	5.31
K_2O	4.32	4.31	6.77	6.18	7.29	4.74
H_2O+	0.64	0.35	0.97	0.87	0.82	3.56
H_2O-	0.50	0.40	0.76	0.26	0.08	2.34
TiO_2	1.09	0.96	0.75	0.23	0.19	0.37
ZrO_2	0.00	0.00	0.01	tr.	tr.	0.00
CO_2	0.50	1.91	0.11	0.10	0.00	0.00
P_2O_5	0.93	0.75	0.78	0.18	0.11	0.23
Cl	0.15	0.26	0.25	0.33	0.39	0.59
F	0.03	0.03	0.09	0.09	0.12	0.02
S	0.05	0.05	0.03	0.06	0.05	0.06
Cr_2O_3	0.06	0.02	0.03	tr.	0.01	tr.
MnO	0.31	0.20	0.41	0.96	0.20	0.61
BaO	0.14	0.17	0.12	0.06	tr.	0.13
SrO	0.07	0.07	0.12	tr.	0.05	0.13
	99.61	99.53	99.67	99.99	99.83	99.90
Norms						
or	17.79	23.35	24.46	36.70	43.37	27.80
ab				20.96	18.86	40.35
an	11.12	15.01	6.12		.28	14.18
ne	14.20	15.05	21.58	28.12	30.39	2.27
lc	6.10	1.74	12.21			
C						2.35
ac				5.08		
ns				1.46		
di	32.85	30.34	23.10	4.77	0.22	
wo					2.20	
ol	5.60	1.39	2.36	0.44		1.87
mt	5.80	5.80	4.18		1.86	3.25
hm					0.64	
il	2.13	1.82	1.52	0.46	0.46	0.76
ap	2.02	2.02	2.02	0.34	0.34	0.34
etc	1.93	3.02	2.25	1.71	1.52	6.57
	99.54	99.54	99.80	100.04	100.14	99.74

1. Vicoite, shonkinose-cascadose, III. '7. 2. 3, Grotjokan Falls.
2. Vicoite, ourose-shonkinose, 'III. 6. 2(3). 3, Grotjokan Falls.
3. Vicoite, janeirose-baweanose, II (III). 7. (1)2. 3, Tandjung Anjer.
4. Tinguaite, miaskose-laurdalose, (I)II. 6'. 1. '4. S. of Limpang Kopeng.
5. Tinguaite, beemerose-miaskose, I. 6'. 1. (3)4, S. W. spur Gunung Besar.
6. Sodalite-trachyte, pulaskose-laurvikose, I'. 5. 2. (3)4, West side of Pulu Mepuri.

probably colorless pyroxene, possibly jadeite. This tinguatic phonolite forms fissile massive bodies, having a chemical composition shown by analyses 4 and 5; one from south of Limpang Kopeng in the east central part of the island, the other from a southwest spur of the central mountain, Gunung Besar.

Other phonolitic rocks have a trachytoid groundmass of prismoid alkalic feldspars with small leucites in some varieties, and minute enhedral sodalites in others. Still fewer are trachytes with almost no recognizable feldspathoid constituent.

Analysis 6 is of a sodalite-trachyte which appears to contain some kaolinite, but the thin section does not show the presence of any hydrous mineral. The chlorine in the analysis corresponds to 8% of sodalite.

The rocks from Bawéan so far analyzed are strongly alkalic, with relatively high potash in the vicoites. They are chemically similar to the leucitic rocks of Mount Mourah in Java, and to the leucitic and nephelitic rocks of the Maros district in Celebes, but they differ in petrographical characters from the rocks of both these districts.

THE PHYLOGENETIC DEVELOPMENT OF SUBAPTEROUS AND APTEROUS CASTES IN THE FORMICIDAE

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Read before the Academy, November 14, 1916

It is generally admitted that each of the four groups of social insects—the social bees, social wasps, ants and termites—has had an independent phyletic origin and history and that the similarities in their habits are due to parallelism, or convergence, of which, indeed, they exhibit striking examples. In both the fertile and sterile females of social wasps and bees the wings show no signs of reduction, whereas these appendages are well-developed in the fertile females (females proper) of the great majority of ants, at least prior to fecundation, but are normally always absent in the sterile females, or workers. Paleontology proves that identical conditions have long existed in the Formicidae as a family, since they are clearly shown in the abundant and beautifully preserved ants of the Baltic amber from the Lower Oligocene Tertiary.[1]

Writers also agree that the ants must be descended from certain

primitive wasps belonging to the families Scoliidae, Mutillidae and Thynnidae, but authorities differ as to which of these families should be selected as the most probable ancestors. Emery[2] believes that the ants arose from the Mutillidae, Forel from the Thynnidae and Handlirsch[3] from the Scoliidae. But as all three families are so closely related to one another that authorities fail to assign them the same limits, the differences of opinion are after all not very pronounced.

Handlirsch[4] advances the opinion that the ants first made their appearance during the Cretaceous, but I am inclined to seek their origin in an earlier period, during the Jurassic or possibly even as far back as the Triassic. According to Schuchert,[5] these were periods of maximum continental emergence and aridity and would therefore present what I conceive to be the optimum environmental conditions for the development of the family Formicidae. The insects most closely related to the ants (Thynnids, Scoliids, Mutillids) are very decidedly xerothermic and hence confined to deserts or to hot sandy and gravelly situations, and the ants present a number of peculiarities which seem to indicate more or less clearly that they originally lived and developed in the same kind of a habitat. They are at the present time extraordinarily abundant in species and individuals in the desert regions of the globe (Australia, North Africa, Sonoran Regions of North America) and as a group seem to show in their inherited small average stature the stunting effects of an arid environment. The great majority of species have retained the primitively terrestrial and fossorial habit, which is an obvious adaptation to avoiding intense heat, insolation and evaporation during the summer months and the cold of nights and of the winter. The aptery of the workers and deälation in the females are closely connected with such habits. Most of the species, moreover, are decidedly petrophilous and many are exquisitely hypogaeic. The marriage flight, to which the males and females of most of the species so rigidly adhere, would seem to be a habit that had originated in open, unobstructed country. The adaptations, though numerous and intricate, to living in mesophytic and hygrophytic forests (Amazonian and East Indian rain-forests) are clearly secondary and of much more recent origin.

That the workers of ants originally possessed wings like the females is shown by the presence of minute vestiges of these organs in the larval and pupal stages[6] and by the occasional, pathological development of very small wings in the adult. This condition occurs in the 'pterergates' found by Donisthorpe[7] and myself[8] in certain species of *Myrmica*, *Cryptocerus* and *Lasius*. The development of aptery, with the concomitant

simplification of thoracic structure (micronoty) so universal in the workers, is evidently a phylogenetic process, which was completed in most ant-genera before the Lower Oligocene. *Aptery* is, of course, to be carefully distinguished from *deälation*, the dropping of the wings by the female ant immediately after fecundation. Deälation is really a form of mutilation (autotomy) which has been practiced by female ants for millions of years without necessarily entailing any modification or diminution in the development of wing structures. Compared with this case of the non-inheritance of mutilations, the cases usually cited in the

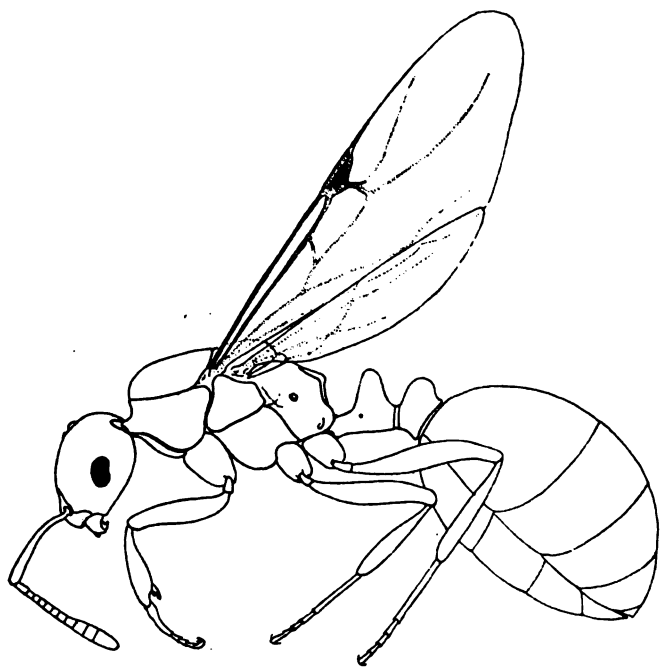

FIG. 1

Winged, macronotal female of *Monomorium rothsteini* Forel var. *humilius* Forel, lateral view.

text-books, such as circumcision and the docking of tails in mice, are insignificant, because they refer to such limited series of generations.

There are a few genera of ants, especially in the subfamily Myrmicinae, in which it is possible to trace all the transitions in thoracic structure from that of the female to that of the worker, except that, in all cases hitherto recorded, the wings show no transitions, but are perfectly developed in the typical female and entirely lacking in all the other forms of the series. Good examples are certain species of *Myrmecina* (*M. graminicola*) and *Leptothorax* (*L. emersoni*), but of all the genera

I have studied *Monomorium* is the most instructive in this connection, because at least two of its species exhibit a normal and hitherto unsuspected condition of subaptery in the female. Some years ago Forel[9] described a *Monomorium rothsteini* from worker specimens received from Queensland, Australia, and later distinguished two varieties of it, *humilius*,[10] from Tennant Creek, Central Australia and *leda*,[11] from northern Queensland. During December, 1914, I observed many colonies of *rothsteini* in various localities in Queensland. The workers of this species store their subterranean nests with small grass-seeds and are therefore harvesters, like the species of *Holcomyrmex*, now regarded as a subgenus of *Monomorium*. Among a lot of ants recently received from the Museum of South Australia I find a large series of cotypes of the var. *humilius* comprising 14 specimens of the female, which was unknown to Forel. This phase measures 7–7.5 mm. and has long, well-developed wings (6.8–7 mm.). In the same collection I find three series of a closely allied species, which I shall describe in another place as *M. subapterum* sp. nov. Two of the series, representing the typical form of the species, comprise numerous workers, 10 males and 10 females (5 deälated) taken by Mr. W. D. Dodd on the Harding River in North West Australia and one female and several workers taken by the same collector at Derby in the same region. The remaining series represents a color variety of *subapterum*, which I shall call *bogischi* var. nov., comprising several workers and four females (three deälated) from Point Wakefield, South Australia (G. P. Bogisch). The winged females in all three series measure 6.5–7 mm. and agree in having very small wings, measuring only 3 mm., and a distinctly smaller thorax than *rothsteini*. There are other specific differences which need not be discussed at the present time. Figs. 1 and 2, drawn to the same scale, show the differences in wing and thoracic structure between the females of the two species, corresponding to what may be called the macronotal, winged and mesonotal, subapterous types respectively (1 and 2, p. 114). The males of *M. rothsteini* and *subapterum* show no differences in the size and development of the wings, which in both are large and of the usual structure. There can be no doubt that the deälated females of two of the series of *subapterum* originally bore small wings like the nondeälated individuals. The fore wings have a singular truncated apical border, as if they had been trimmed with scizzors, and this appearance is accentuated by the fact that the membranes are as thick at the border as at the base. It is certain, however, that the wings have not been reduced to their present form by the mandibles of the ants. The venation is abortive, only the submarginal vein being distinct, although the fore wings show faint

traces of the basal portions of some of the other veins. Brues[12] distinguishes three classes of vestigial wings among insects: those having essentially a pupal character, those essentially normal, except for their smaller size and less complex venation and those consisting of little more than a hollow bag, without venation. The wings of *M. subapterum* evidently belong to the second class.

In six specimens of a pale variety of *Monomorium rubriceps* Mayr (*cinctum* var. nov.) taken by Mr. Albert Koebele in Victoria, Australia, I find the following graduated series of forms connecting the subapterous female with the typical worker. One specimen (fig. 3*a* and *b*), measuring 6.5 mm. has the ocelli well-developed, the thorax rather small but with distinct mesonotum, scutellum, metanotum, paraptera and

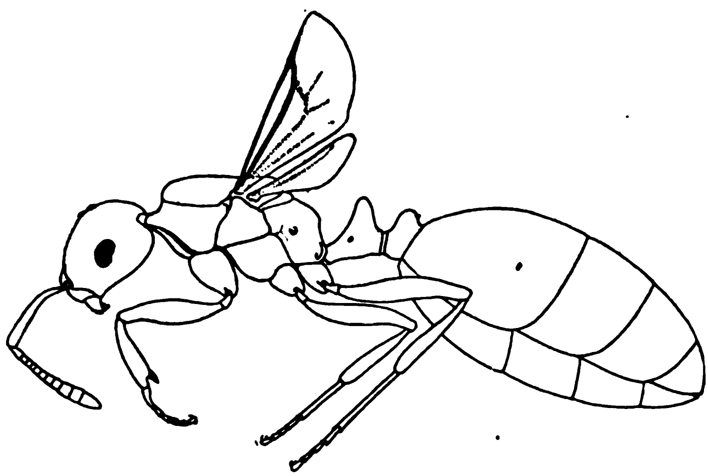

FIG. 2

Subapterous, mesonotal female of *Monomorium subapterum* sp. nov., lateral view.

tegulae, with stumps of wings and on the right side one nearly entire posterior wing. The latter is evidently vestigial, though its tip is lacking. The remaining vestigial wings have evidently been lost by deälation. This specimen is therefore a partially deälated female in a more advanced stage of subaptery than the female of *M. subapterum*. A second specimen (fig. 3*c*) is slightly smaller and has a similar thoracic structure, except that the tegulæ and paraptera are lacking and the thorax has never borne wings. A third and fourth specimen (fig. 3*d*) measure 4.5–5 mm. and have small ocelli and the thorax is still more reduced and worker-like, but the mesonotum, though small and flat, is distinct and there is a visible promesonotal suture and a metanotal sclerite. The two remaining specimens, 3.5–4 mm. long (fig. 3*e*), are normal

workers, without distinct mesonotal and metanotal sclerites and without ocelli. The first specimen is therefore of the subapterous mesonotal type, the second is apterous and stenonotal, the third and fourth are ergatogynes. Thus the three species, *M. rothsteini*, *subapterum* and *rubriceps* together represent all the principal stages from the perfect female to the worker.

My study of the large genus *Monomorium* shows that in some species the only females are stenonotal and apterous (*M. floricola*, *carbonarium*, etc.), whereas in others they are ergatogynes (*M. venustum*, *schurri*, etc.). In still other species both winged and ergatogynic forms occur (*leœ*, *dichroum*, etc.). This "morphological restlessness" of the female is evidently a survival of a condition which was once common to all ants but which has disappeared in most genera through a survival of the two extremes of the graduated series of forms, the macronotal, winged form (female proper) and the micronotal, apterous form (worker), and the suppression of all the intermediate phases. That the species of *Monomorium* should retain so complete a picture of the various stages that have led up to the development of the worker caste is not surprising when we stop to consider that the genus is one of the most primitive in the subfamily Myrmicinæ. This is shown by its simple morphological characters, the present cosmopolitan distribution of its species, their dominance in the ant-faunas of regions noted for the archaic character of their biota (Africa, Southern Asia, Australia and New Zealand), and the known geological age of the genus, which is represented in the Baltic amber by two species differing but slightly from existing forms. Space forbids a discussion of a few other Formicid genera in which a similar diversity of females is known to occur. Among the ants as a family we may conveniently recognize the following female types which at the same time represent stages in the phylogenetic development of the worker:

1. The macronotal, winged female.—Most ants.

2. The mesonotal, subapterous female.—*Monomorium subapterum* and *rubriceps* var. *cinctum*.

3. The stenonotal apterous female.—Some species of *Monomorium*, *Anochetus*, *Myrmecia*, *Odontomachus hastatus*, etc.

4. The micronotal female, or ergatogyne.—Some species of *Monomorium* and *Crematogaster*, *Polyergus refescens*.

5. The ergatoid female.—*Leptogenys*, *Acanthostichus*, *Sphinctomyrmex*, *Onychomyrmex*, *Paranomopone*. These forms grade into the 'dichthadiigynes' of the Dorylinæ.

6. The gynaecoid worker.—*Ocymyrmex*, *Leptomyrmex*, *Diacamma*, *Rhytidoponera*, etc.

In this connection it is interesting to note that a series of forms between the winged female and worker closely paralleling those described in the preceding paragraphs, may be developed in ants as the result of parasitism and therefore under pathological conditions. Mrázek,[13] Donisthorpe[14] and I[15] have shown that small-winged or subapterous females ('mermithogynes') are produced in *Lasius niger* by the presence

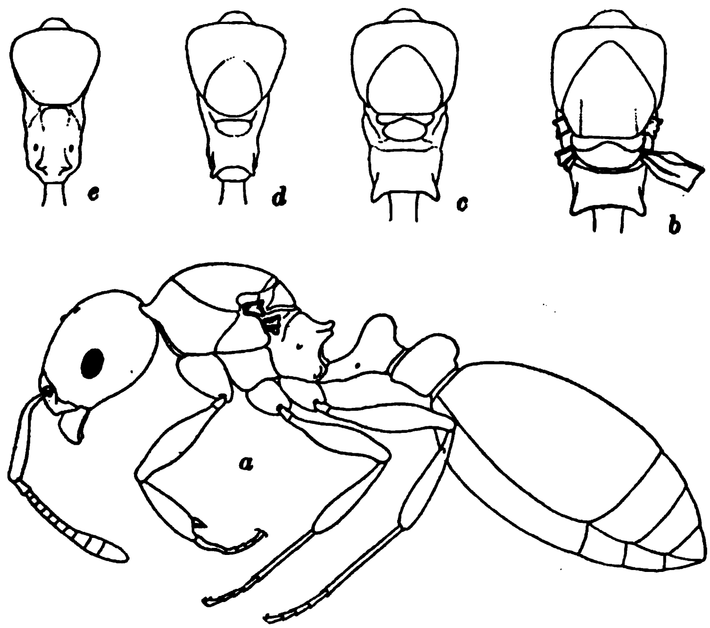

FIG. 3

Monomorium rubriceps Mayr. var. *cinctum* var. nov. *a*, Mesonotal, incompletely deälated, subapterous female, lateral view; *b*, thorax of same, dorsal view; *c*, thorax of stenonotal apterous female, dorsal view; *d*, thorax of ergatogyne (micronotal female), dorsal view; *e*, thorax of worker, dorsal view.

of Nematode worms (*Mermis* sp.) in the abdominal cavity, and Wasmann[16] and others have demonstrated that 'pseudogynes,' i.e., forms closely resembling ergotogynes and ergatoids, are produced by the parasitism of Lomechusine beetles (*Lomechusa*, *Atemeles*, *Xenodusa*) on the ant colony as a whole. Gynaecoid workers, finally, may be developed by what really amounts to a pathological disturbance in the trophic status of the colony when it loses its queen.

Attention may also be called to a parallel tendency to aptery and micronoty in the males of certain ant-genera. Thus the male of *Sym-*

myrmica chamberlini has lost its wings, but apparently so recently that it still retains the typical male structure of the head, antennae and thorax, even to the development of the Mayrian furrows. In several other genera (*Formicoxenus*, *Cardiocondyla*, *Ponera*), however, the males have acquired the same structure of the head and thorax as the worker, so that they can be distinguished only by their genitalia and the number of their antennal joints. In one species (*Anergates atratulus*) the apterous male degenerates still further into an almost pupoidal condition.

The facts briefly presented in the preceding paragraphs seem to me to have an important bearing on the question of continuous variation *versus* mutation in the production of organic forms. In most species of ants the constant and striking structural differences between the different castes would, at first sight, suggest that such forms as the apterous females, apterous males, soldiers and workers, must have arisen as so many saltatory variations, or mutants and that they survived and secured representation in the germ-plasm, because they happened to fulfill specialized and useful functions in the life of the colony. I believe, however, that this view of the castes, at least so far as their origin is concerned, cannot be maintained, because all the available evidence points to their being merely the surviving extremes of graduated and continuous series of forms, the annectant members of which have suffered phylogenetic suppression or extinction. This is most clearly seen in the case of the soldier and worker. Only within comparatively recent time, i.e., probably since the middle Tertiary, has the originally monomorphic worker caste become polymorphic in certain genera (*Camponotus*, *Atta*, *Pheidologeton*, some species of *Pheidole*), i.e., developed a complete series of workers ranging from huge-headed major workers or soldiers (macrergates, dinergates) through intermediates of various sizes (desmergates) to small workers (micrergates). There is much evidence to show that in some genera (e.g., *Pheidole*, *Oligomyrmex*, etc.) all the forms in this series, except the dinergates and micrergates, have been suppressed, so that a marked dimorphism of the worker caste, simulating an origin of one or both of the forms by mutation, has been produced. In other genera (*Carebara*, many *Solenopsis*) the soldier form has also been suppressed, so that the worker caste has again become monomorphic through the survival of nothing but the smallest forms (micrergates) of the originally graduated series. Finally, in certain parasitic ants, (*Anergates*, *Anergatides*, *Epœcus*, *Wheeleriella*, etc.) the last traces of the worker caste have vanished, just as in several Australian genera (*Leptomyrmex*, *Rhytidoponera*, *Diacamma*)[17] and the South African *Ocymyrmex*[18] the

female caste has been abolished and its reproductive function transferred to gynaecoid workers, i.e., to forms differing from ordinary workers only in their ability to produce worker as well as male offspring. It is very probable that even this abolition of whole casts has been accomplished by very slow and gradual processes and not by sudden variations, or mutations.

[1] Wheeler, The Ants of the Baltic Amber, *Königsberg Schr. physik. Ges.*, 55, 1914, (1-142).

[2] *Zool. Jahrb. Abth. Syst., Jena*, 8, 1895, (774).

[3] *Die Fossilen Insekten, Leipzig*, 1908, (p. 1283).

[4] *Loc. cit.*, (p. 1285).

[5] *Bull. Geol. Soc. Amer.*, 20, 1910, (427-606), and Climates of Geologic Time, *Carnegie Inst. Washington Pub.* No. 192, (pp. 263-298).

[6] Dewitz, *Zs. wiss. Zool., Leipzig*, 30, 1878, (78-105), Pl. 5.

[7] *British Ants*, 1915, (p. 131 and 221), Fig. 50.

[8] *Bull. Amer. Mus. Nat. Hist., New York*, 21, 1905, (405-408), 1 pl.; Ants, etc., 1910, (p. 99), Fig. 63.

[9] *Rev. Suisse Zool.*, 10, 1902, (444).

[10] *Ibid.*, 18, 1910, (27).

[11] *Ark. Zool.*, 9, 1915, (71).

[12] *Biol. Bull.*, 4, *Woods Hole, Mass.*, 1903, (180).

[13] *Acta Soc. Ent. Bohemiæ*, 5, 1908, (139-146), 4 figs.

[14] *British Ants* 1915, (p. 220), Fig. 47.

[15] *J. Exper. Zool.*, 8, 1910, (421), Fig. 7.

[16] *Zs. wiss. Zool.*, 114, 1915, contains a full bibliography of the author's papers on the Lomechusini.

[17] Wheeler, *Boston, Proc. Amer. Acad. Arts Sci.*, 51, 1915, (257).

[18] Arnold, *Ann. S. Afric. Mus.*, 14, 1916 (195).

REFRACTIVITY DETERMINED IRRESPECTIVE OF FORM, BY DISPLACEMENT INTERFEROMETRY

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Communicated December 13, 1916

1. *Introductory.*—Some time ago[2] I made a number of experiments on the use of curilinear compensators in connection with the displacement interferometer. It is obvious that the curvature in such a case must be very small, so that single lenses for the purpose are not easily obtained. The use of a doublet of two lenses of the same glass but respectively convex and concave, meets the case fairly well, the necessary refracting power being obtained by spacing the doublet. Lenses of about one diopter each gave the best results, bringing out fringes of quasi-elliptic and hyperbolic symmetry in great variety.

Later it appeared as if plates of different varieties of glass, as for instance crown and flint, if placed in the two interfering beams would

produce the same phenomena. The flint plate used, however, proved to be inadequately plane, so that the result is in doubt.

More recently I have endeavored to secure similar results by submerging the lens (convex or concave) in a liquid of about the same index of refraction. This method would seem to be interesting in other respects; for it is probable that the index of the solid may be determined in this way irrespective of form.[3] If for instance the liquid and the solid have the same index, one would be tempted to infer that the latter may be removed or inserted, without displacing the center of ellipses at the particular wave length under consideration. The index of the liquid in place is then also determinable by the interferometer, to a few units in the fourth place. Unfortunately the problem is not so simple.

If experiments of the present kind are to be accurate, it is obvious that the walls and cavity of the trough in which the lenses are to be submerged must be *optically* plane-parallel. Otherwise some compensating adjustment must be made at the opaque mirrors of the interferometer and for this no adequate allowance can be made. It did not however seem worth while to provide expensive apparatus, before the method had been worked out in detail. Accordingly the present experiments were conducted with troughs of ordinary plate glass put together by myself, and little attention was given to absolute values of index of refraction, as such.

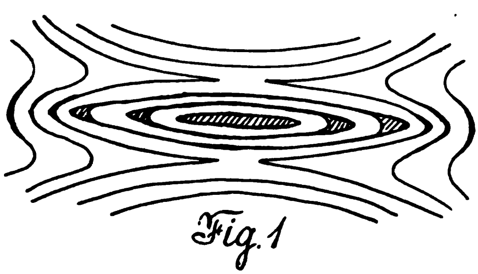
2. Preliminary experiments.—The first experiments were made on a large linear interferometer with distances of nearly 2 meters between the mirrors. The rays in such a case are all very nearly parallel. Sunlight, arc light or the Nernst filament were each available for illumination. With a very long collimator (2 meters) and wide single lens objective (10 cm. or more), the Nernst filament may be used directly in place of the slit.

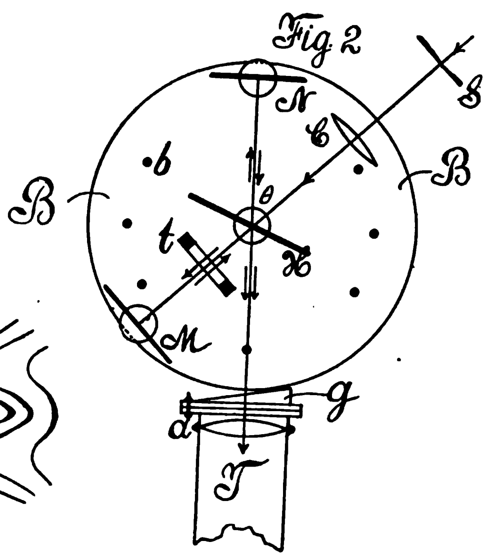
On inserting the trough with a thickness of 1.3 cm. of CS_2 solution normally into one component beam, the original very large ellipses were reduced in size and rounded as usual to smaller circles. Submerging a convex lens (1 diopter) into the liquid until the beam passed symmetrically through it, changed these circles to very long horizontal spindles. A concave lens similarly produced horizontally very eccentric hyperbolae. All these fringes lie considerably in front of the principal focal plane of the telescope and the abnormal forms are necessarily relatively faint. They change in shape and intensity with the focal plane observed.

On mixing CS_2 with kerosene (about equal parts) types of fringes shown in figure 1, but with many more lines, were obtained. This is

a combination of both spindles and hyperbolae. Probably three layers of liquid are chiefly in question, viz., kerosene, kerosene + CS_2, CS_2, and the three stages of form and the sinuous lines correspond to them. Fringes were sharp only if viewed in front of the principal focal plane of the telescope.

3. *Apparatus.*—To obviate the tremor of apparatus which is inevitable in the case of the long distance interferometer, the parts were now screwed down at short distances in the cast iron block *B* figure 2. Here the ranges *MH*, *HN* of half silvered plate, *H*, and opaque mirrors *M*, *N*, did not exceed 14 cm., but this gives ample room for the manipulation of the trough *t* placed normally in the beam *MH*. White light en-





ters by way of the collimator *SC* at any convenient angle θ (as this does not occur in the equations) and $\theta = 60°$ was used. The opaque mirrors *M* (and preferably also *N*) are on micrometers with screws normal to their faces and each must be provided with adjusting screws relatively to horizontal and vertical axes. An elastic fine adjustment (not shown) is desirable. The block contains a number of screw sockets, *b*, for attaching subsidiary apparatus. The trough, *t*, should preferably be attached to an independent supporting arm, not connected with *B* and be revolvable about two axes normal to each other. In such a case the position *normal* to the beam of light may be found from the reverse of motion of the interference rings, while the trough is slowly rotated in a given sense.

The telescope T (relatively much enlarged in the diagram) is not attached to the block. It is to be used both as a simple telescope for the adjustment of the white slit images to horizontal and vertical coincidence, and as a direct vision spectroscope. The most convenient attachment for this purpose is the direct vision prism grating, G (film grating), just in front of the objective of T. Two perforated thin discs of brass are useful for this purpose, one disc being firmly attached (like the cap) to the objective, the other to the flat face of the grating with the prism outward. A swivel bolt, a, between the discs, thus allows the observer to throw out the grating and use the telescope. A stop arrests the motion of the grating when it is rotated about a, back again, for viewing the spectrum. This plan worked very well and the ellipses obtained were magnificent. It was now almost possible to control the micrometer M manually and all hurtful quiver is absent. The fiducial line to which centers of ellipses, etc., are to be returned is always the sodium doublet, present in sunlight or the arc and artificially supplied by an interposed burner in case of the Nernst lamp. The telescopic lens need not be more than 2 cm. wide and cross hairs are not needed. For measuring dispersion the Fraunhofer lines B, C, D, E, b, F, were used.

4. *Equations.* The useful equation for present purposes are given in a preceding report[4] and the following cases only need be repeated here. If e is the thickness of glass plate of index of refraction ω for the wave length λ and if the equation $\mu = A + B/\lambda^2$ where A and B are constants be taken as sufficient

$$\mu - 1 = \Delta N/e - 2B/\lambda^2, \tag{1}$$

where ΔN is the displacement of the micrometer at the opaque mirror M or N due to the insertion of the plate normally to the component beam in question. To determine μ, B must be known at least approximately. It may be measured in the same adjustment, however, if two Fraunhofer lines are used fiducially. Let δN be the displacement of micrometer to pass the center of ellipses from wavelengths λ to λ'. Then if e' is the thickness of the half silvered plate H, and R the angle of refraction within it,

$$\delta N = B\left(e + e' \cos R + 2\left(e + \frac{e'}{\cos R}\right)\right)\left(\frac{1}{\lambda^2} - \frac{1}{\lambda'^2}\right), \tag{2}$$

If $e = 0$,

$$\delta N_a = B\left(e' \cos R + \frac{2e'}{\cos R}\right)\left(\frac{1}{\lambda^2} - \frac{1}{\lambda'^2}\right) \tag{3}$$

which may be called the corresponding air displacement. Hence

$$B = \frac{\delta N - \delta N_a}{3e\left(\frac{1}{\lambda^2} - \frac{1}{\lambda'^2}\right)} \tag{4}$$

Here $\delta N = \delta N_a = N - N' - (N_a - N'_a) = N - N_a - (N' - N'_a)$

so that the difference of the corresponding positions of micrometer for a given Fraunhofer line, with and without the plate, are to be found. The method is quite accurate as will be seen below. More than two constants A and B may be taken if desirable, with 3 Fraunhofer lines.

To return to equation (1), remembering that $2B/\lambda_2$ is small, it is seen that the percentage accuracy of $\mu - 1$ and ΔN are nearly equal. Now ΔN for a plate 5 to 6 mm. thick and ordinary glass is about 0.3 cm. This may be measured within 10^{-4} cm. or 3 parts in 10^4 of ΔN or one or two units in the fourth place of μ the index of refraction of the glass. A much more serious consideration is the consistent measurement of the *thickness* of plate e, which must be given to less than 10^{-4} cm. if the same accuracy is wanted. Naturally this presupposes optic plate. Hence the data will be inaccurate as to absolute values from this cause. The plates used frequently showed increases of thickness of several 10^{-3} cm. within a decimeter of length. Absolute values were however without interest in this paper.

To show that less than 10^{-4} cm. is guaranteed on the micrometer in the placing of elliptic centers at the sodium line, the following pairs of results, made at different times and with entirely independent adjustments may be cited. The screw pitch was 0.025 cm. and the drum divided into 50 parts with a vernier to 0.1 part.

FRAUNHOFER LINE	PITCH	DRUM	PITCH	DRUM	DIFFERENCE
					cm.
B	×85	17.1	74	33.2	0.25000+0.01695
C	×85	23.3	74	39.3	0.25000+0.01700
D	×85	41.0	75	7.0	0.25000+0.01700
E	××86	14.9	75	30.9	0.25000+0.01700
b	××86	19.2	75	35.2	0.25000+0.01703
F	×××86	36.6	76	2.6	0.25000+0.01703

× ellipses long horizontally ×× circles ××× ellipses long vertically.

The total difference is less than 10^{-4} cm. and probably due to the width of the Fraunhofer lines with deficiency of light at the ends of the spectrum. Apart from the measurement of the thickness e therefore, the method is guaranteed for fourth place work.

A large number of experiments were made with lenses, etc., submerged in mercury iodide solution, data for which there is no room here. The curious result appeared that if the solution refracts more strongly than the submerged glass, and the lens is well centered as to the beam of light, the ellipses remain strong and clear. Hence the refraction of the solution and of the glass need not be identical. The dispersion B is particularly well determinable. The refraction at λ in this case will be subject to

$$K' = \mu' - 1 + 2B'/\lambda^2 = \mu - 1 + 2B/\lambda^2 + \Delta N/e$$

when ΔN is the displacement produced on submerging the lens of thickness e at its middle and primes refer to the solution. The following is an example of results. K' for the solution is found as a whole from the full and empty trough.

Lens	e cm.	K'	ΔN cm.	μ	
1 diopter	0.138	0.6140	0.0078	1.5315	Ellipses strong
2 diopters	0.200	0.6140	0.0107	1.5345	Ellipses strong
5 diopters	0.248	0.6140	0.0187	1.5126	Ellipses faint
10 diopters	0.447	0.6343	0.0205	1.5625	Ellipses vague

The first two cases are good, the last two untrustworthy. To perfect this method a solution must be found whose dispersion constants are not so enormous compared with glass as the mercury iodide solution.

[1] Note from a Report to the Carnegie Inst. of Washington.

[2] *Washington, Carnegie Inst., Pub.*, No. 249, 1916, chap. ix, cf. *Amer. J. Sci., New Haven*, **40**, 1915, (299–308).

[3] Mr. R. W. Cheshire, *Phil. Mag., London*, **32**, 1916, (409–420), has recently used Töpler's method for the same purpose with marked success.

[4] *Washington, Carnegie Inst., Pub.*, No. 229, 1915, § 40, 41, 42.

THE FOOD OF DROSOPHILA MELANOGASTER MEIGEN

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Communicated by W. M. Wheeler, December 26, 1916

In May, 1916, while rearing the banana fly on artificial media of fermented banana agar,[1] I observed that visible fungus growths seldom occurred in the presence of many larvae. Such growths did appear, however, after pupation or if only a few larvae were present. Examination showed the surface growths to be largely yeast cells.

Further investigation showed that adult flies, pupae, larvae, and eggs

invariably carried yeast cells and often had a rod-like bacterium upon them. This made it appear that an internal symbiotic relationship existed comparable to the notable cases of the flesh fly,[2] in which a micrococcus is passed through the egg and aids the larvae in the digestion of albumins, etc., the cockroach,[3] which has certain cells of the fat-body reserved for yeast, and many plant bugs,[4] which have caeca of the alimentary tract containing certain bacteria.

In the following experiments undertaken to show that microorganisms are not transmitted through the egg of Drosophila, the first precaution was to free the insect from external microorganisms. Usually eggs were used for this purpose but their small size makes this a difficult procedure. As it is well known that the lining of the digestive tract of larvae is thrown off upon pupation, pupae were selected for sterilization.

(1) The pupae were submerged in 85% alcohol for ten minutes and then introduced aseptically into sterile slant culture tubes of agar-agar and fermented banana filtrate. If no yeast developed around the pupae which were placed on the food the tube remained sterile after the emergence of adults, oviposition and hatching of larvae. The sterility of the tube was later tested by introducing a few loopfuls of the medium into a sterile tube of similar food. It had previously been determined that yeast developed readily on fermented banana agar. This is probably due to the catalytic action of dead yeast,[5] and the abundance of nutrition supplied by the banana.[6]

(2) Larvae which had been feeding on media containing living yeast cells were submerged and washed in 85% alcohol and then introduced into sterile culture tubes. In all cases yeast developed on the new media. Cultures from the digestive tracts of the larvae gave similar results. Apparently many cells escape digestion in the stomach as is the case with seeds or insect eggs in birds.

(3) Eggs were sterilized by soaking in 85% alcohol for ten minutes. The larvae which hatched were always aseptic.

From the foregoing experiments we may conclude that yeast is not present in the eggs or pupae of Drosophila. However, a loose symbiosis exists between the two organisms. As mentioned above, surface fungus growths disappear in the presence of larvae which often seemed to be more numerous at this point. From these observations I inferred that the larvae fed upon the microorganisms present.

The problem was attacked by the use of several different media. Two dozen large ripe bananas were fermented overnight by adding 24 grams of compressed bread yeast separated in a few cubic centimeters

of water. The banana was thoroughly mashed before fermentation. This material was pressed through a sugar sack giving 1200 cc. of fluid.

Medium A consisted of this fluid, diluted twice with enough powdered agar-agar added to make up to 1.5%, then boiled, poured into test tubes and plugged with nonabsorbent cotton and sterilized in the autoclave for twenty minutes at 19 pounds pressure with vacuum at start.

Medium C consisted of 12 grams yeast per 200 cc. tap water and 1.5% agar-agar.

Medium D consisted of tap water and 1.5% agar-agar.

Medium J consisted of grape sugar, cane sugar, ammonium tartrate, citric acid, K_2HPO_4,$MgSO_4$,H_2O (proportions as used by Loeb[7]), 1.5% agar-agar. This was sterilized three successive days in Arnold Sterilizer.

The rates of development of larvae on these media were as follows:

	Media	Average number of days	Growth
A	Aseptic fermented banana agar	28	From hatching to death
A	Septic fermented banana agar	12	From hatching to pupation
C	Aseptic yeast agar	9	From hatching to pupation
D	Aseptic agar	5	From hatching to death
D	Septic agar	26	From hatching to pupation
J	Aseptic synthetic medium	5	From hatching to death
J	Septic synthetic medium	10	From hatching to pupation

Aseptic larvae on A developed at half of the rate of septic larvae on the same medium. The growth of these retarded individuals could be greatly accelerated by adding two loopfuls of septic A to their food. This was done in the case of larvae which in twenty-six days have only reached a length of 2.5 mm. After three days the larvae were 4 mm. long and had doubled in diameter; another millimeter was gained the next day and on the sixth day the larvae formed pupae from which large adults emerged. Since larvae grow more rapidly on abundant dead yeast (C) than on less abundant living yeast, the fungus is merely the food of the insect.

It is apparent from the foregoing that the rate of growth is dependent on the abundance of yeast in the media. Dead yeast serves equally well in determining this rate. We may therefore conclude that yeast serves as food for Drosophila larvae. There is also evidence that other microorganisms may furnish nutrition for these flies. For example flies were reared from egg to adult on agar-agar (D) which had been contaminated by a living anaërobic bacterium as yet undetermined.

After the initiation of these experiments I found that Delcourt and Guyenot[8] and Guyenot[9] had already shown that yeast and most bacteria are of value to Drosophila larvae, especially when the larvae are

in large numbers. These investigators finally obtained aseptic flies by the use of media unfavorable to the different forms of bacteria. Yeast persisted longest but was finally eliminated by using food consisting of sterile yeast, water and cotton. Potato media were occasionally intercalated in the series as a test of the absence of living yeast cells. The transference of flies was made aseptically by the use of an ingenious suction apparatus. However, females from stock which had been 'aseptic' for several generations occasionally deposited septic eggs and died in the center of a yeast colony. It is possible that living yeast was constantly present and that usually an insufficient number of cells was carried to the potato medium. In such a case due to the absence of bios,[9, 10,] growth would not take place. Guyenot later[11] reported that he had been able to raise Drosophila on sterile (unfermented) potato, but the percentage of larvae that pupated was small. Salomon[12] has described the use of yeast as a food for man and a considerable literature deals with its use as food for farm animals. Schultze[13] has suggested that Drosophila larvae may feed upon bacteria and yeasts in strange media such as sap, feces, formol, tumors, etc., from which the fly has been recorded. Henneberg[14] believes that these larvae feed upon microorganisms at the surface of vinegar.

It is probable that the food relations pointed out in my experiments are common to a number of organisms. The house fly, for example, oviposits only in the presence of the odor of fermentation, always has a certain form of bacteria on its body in great abundance and the larva is unable to survive in garbage which gives an acid reaction. Those insects which live in strange media such as strong salt water and petroleum may be associated with microorganisms which have unusual power of oxidation. It is likely that the vinegar eel will be found to be similarly associated with bacteria or fungi.

In 1915 Loeb[7] raised Drosophila on a synthetic medium consisting of inorganic salts, sugars and ammonium tartrate. He believed the synthetic power of the fly to be as great as that of bacteria. Later[15], however, he pointed out that the medium used "is a well known culture medium for certain microorganisms; e.g., yeast cells which are capable of synthesizing their proteins and other complicated organic compounds from ammonium salts."[16]

It is very evident from my experiments that the food of Drosophila larvae is yeast. The insect depends upon these cells for its proteins and has no greater synthetic power than is common to higher animals. Adult flies do not require proteins but survive for a much longer period on sugar agar than upon yeast agar. This difference between the nu-

tritional requirements of larva and adult is probably due to the rapid growth in the former which requires proteins and leads to fatal changes in their absence.

Owing to the habit of the larvae of constantly agitating the surface and carrying yeast cells throughout the medium, fermentation is greatly increased and the increase in alcohol resulting may serve as a protection to the larvae against destructive molds and putrefactive bacteria. The anaërobic conditions resulting may increase the percentage of the albumin in the yeast cells[17] and thus increase their food value.

The number of larvae present in a culture determines the degree to which the yeast will be spread through the medium. If few larvae are present the cultures become contaminated with injurious molds and bacteria; if a large number of larvae is present the food remains 'sweet.'

The depth to which the larvae work their way below the surface is an important factor in this connection. This depth is determined by the consistency of the medium, as the larvae will go as deep as a bubble of air will remain attached to the two projecting posterior spiracles. The instant this bubble is lost the larva 'backs-up' until another is found. It would seem therefore that a medium of a jelly-like homogeneous consistency would be best, and I believe the geneticists are striving for this end when they mash and boil their banana.

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[16] After sending this paper to press, I learned that Loeb and Northrop, *J. Biol. Chem., Baltimore*, **27**, 1916, (309–312), had already published a confirmation of Guyenot's conclusion.
[17] Schoenfeld, F., and Hirt, W., *Wochenschr. Brau., Berlin*, **29**, (157–159, (174–178)).

TEMPERATURE OPTIMA FOR HUMAN ENERGY

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Communicated by W. M. Davis, December 26, 1916

As a step toward the more exact determination of climatic optima for human species I have made a study of temperature in relation to (1) the death-rate, (2) the amount of work done by piece workers in factories, and (3) the strength of individuals as measured by the dynamometer. Although the matter is still in its early stages, the results show that the field of investigation is marvelously rich, and that only the surface has yet been scratched.

(1) *The death-rate* is an excellent means of determining climatic optima. In most parts of the United States two maxima of deaths occur under widely different climatic conditions, one in winter and one in summer. Between the maxima come two minima both of which appear to be associated with essentially the same climatic conditions whether they occur in spring or fall. At first sight this may not be obvious since in the northeastern United States one minimum generally comes in June when the mean temperature is 65° to 70°F and the other in October when the temperature is 10° or 15° lower. Conditions of health, however, obviously lag behind the climatic conditions with which they are connected. An interval must elapse between the time when people fall sick and the time when they die. If allowance is made for this, the true optima would be found in May and September, two months which have about the same temperature.

Since it is difficult to determine the exact amount of lag between given climatic conditions and the death-rate which they determine another method may be employed for determining the optimum temperature. The months may be arranged according to their temperature without regard to the seasons of the year. If the figures thus obtained are smoothed, a lag in one direction is neutralized by a lag in the other. The method is illustrated in table 1.

A glance at the last column shows that deaths in New York are numerous at low temperatures. They decline quite steadily until an optimum is reached when the mean outside temperature is about 65° F. Then they increase once more. The true optimum, however, is a trifle lower than 65°. Many people leave New York in summer and some of them die, but these deaths do not appear in the city records. Consequently the recorded deaths in July and August and to a less degree in September and June are below the number actually occurring in the city's

TABLE 1

MEAN TEMPERATURE AND DEATHS IN NEW YORK CITY, 1900–1911

MONTH	MEAN TEMPERATURE		DEATHS PER DAY	
	Actual	Smoothed*	Actual	Smoothed*
January	30.2	(30.2)	256	(256)
February	30.7	31.5	266	257
December	34.4	34.3	238	253
March	37.5	38.4	270	248
November	44.0	43.4	215	242
April	48.1	48.9	267	240
October	55.5	54.6	210	231
May	59.3	60.2	239	227
September	66.5	65.2	218	225
June	68.5	68.9	224	226
August	72.2	71.6	236	241
July	73.5	(73.5)	266	(266)

* The smoothing is done by the formula $\frac{a + 2b + c}{4} = b$

entire population. This alters the apparent optimum, but not more than a degree or two, for relatively few people are away in September and June. Therefore, on the basis of the death-rate the optimum temperature in New York City appears to be 63° or 64°F.

Table 2 shows the optimum temperature for three groups of American cities and for a group of foreign countries, all being reckoned in the same way as New York.

Aside from New Orleans the optima range from 58° at San Francisco to 70° at St. Louis. Local differences arise from humidity, epidemics, the season at which vacations are taken, and other minor causes. Nevertheless the averages for the four groups differ surprisingly little, the range being only from 62.7° to 65.8°. This seems to indicate that whether we are dealing with northwestern Europe, the northern, central, or southern United States, or an Asiatic country such as Japan, deaths are most numerous at about the same temperature. When allowance is made for the effect of summer vacations in the American cities, it appears that the optimum is probably about 63° F. Since we are dealing with the mean temperature of day and night, such an optimum means that man is physically at his best when the temperature rises to about 70° at noon, but not when it stays all day at that level.

(2) *Daily work.* People's daily work is perhaps the best test of their physical condition. During the past few years I have collected statistics of the work of piece-workers in factories from Connecticut to Florida. A comparison of their work with the mean out-of-door temperature gives the results shown in table 3.

TABLE 2

OPTIMUM TEMPERATURE ON THE BASIS OF THE DEATH RATE

PLACE	YEARS	OPTIMUM TEMPERATURE
Group I. *Northern cities of the United States.*		
Boston	1900–1911	66°
Buffalo	1900–1914	61°
Chicago	1900–1911	67°
Cleveland	(17 years)	64°
New York	1900–1911	65°
Philadelphia	1900–1914	67°
Seattle	1900–1914	62°
Average		64.6°
Group II. *Central cities of the United States.*		
Baltimore	1900–1911	63°
Cincinnati	1900–1911	69°
Pittsburg	1900–1914	67°
St. Louis	1900–1914	70°
San Francisco	1900–1914	58°
Washington	1900–1914	68°
Average		65.8°
Group III. *Southern cities of the United States.*		
Atlanta	1900–1911	64°
Birmingham	1900–1914	
Los Angeles	1900–1911	68°
Memphis	1900–1914	60°
New Orleans*	1900–1911	81°
Average		65.0°
Group IV. *Foreign countries.*		
Belgium*	1861–1910	above 63°
Finland*	1900–1912	above 61°
Germany	1912	64°
Japan	1899–1908	64°
Stockholm	1906–1914	60°
Average		62.7
Grand average		64.5

* Belgium and Finland are so cool in summer that no entire month has a mean temperature as high as the optimum. New Orleans is peculiar because so large a part of the population moves away in summer. Hence these places are omitted in computing the averages.

TABLE 3

OPTIMUM TEMPERATURE IN FACTORIES

PLACE	NUMBER OF PEOPLE	OPTIMUM TEMPERATURE
1. Connecticut	300 men	59°
1. Connecticut	200 women	60°
2. Pittsburg	8800 men and women	63°
3. Tampa	1200 men	67°
Average		63°

This seems to suggest a slight rise in the optimum as one goes from cooler to warmer climates, but it is doubtful whether the inference is justified. The mean temperature of Pittsburgh averages only 3° F. warmer than that of southern Connecticut, and it is not likely that so small a difference could cause an equally great difference in the optimum. The relatively high optimum in Florida may arise partly from the fact that the people whose work was tested at Tampa were Cubans coming from a climate even warmer than that of Florida. Little weight can be placed on such a conclusion, however, because the Cubans were engaged in cigar making and in this work high temperature, especially when accompanied by moisture, makes the tobacco pliable and hence enables people to work rapidly. Thus the apparent optimum is raised a little. Moreover, in the present study no account is taken of the effect of humidity upon human energy. This factor is of much importance and probably accounts for a large part of the variation in the apparent temperature optimum from place to place.

(3) *Strength tests.* In Denmark Lehmann and Pedersen some years ago carried on a series of tests of the strength of school children at all seasons. When due allowance is made for the normal growth of the children it appears that they were strongest when the temperature averaged not far from 59°. For 16 months during 1915 and 1916 the writer was fortunate enough to secure the cooperation of the teachers at Hampton Institute, Virginia, The strength of 11 negro students was tested daily with a dynamometer. The young men were strongest at an average temperature of 61°. An equal number of young women were tested in the same way, but they were working under abnormal conditions in a hot, steamy laundry. Accordingly their apparent optimum was at an average outside temperature of only 52°.

Summing up the results of these various methods it appears that the death-rate of millions of individuals indicates an optimum at 63° or 64°. The factory work of thousands of people points to the same temperature as the best for human activity. Tests of the strength of individuals suggest a slightly lower optimum, at about 60°, but the number of cases is small. The investigations of Rosenau and Thompson also indicate an optimum at 62° or 63°. Since Finns, Japanese, Germans, and Americans all show essentially the same response, it seems probable that the optimum temperature for physical activity among people in all sorts of climates does not vary far from 63° F. Even a long residence in a given climate apparently has little effect in causing people to become adjusted to their environment. The Finn still seems to find his climate always too cool, while the Cuban finds his too warm.

It appears as if climatic environment were able to cause changes in pigmentation and bodily form more easily than in the more direct physiological conditions which determine the relation of the organism to temperature.

There is probably, however, a slight difference in races, as appears from a comparison of the deaths among colored and white people in the twelve American cities where colored people are most numerous. Unfortunately the figures for the two races have been published separately only since 1912, and only the years 1912–1914 are available. The results are given in table 4.

TABLE 4

OPTIMUM TEMPERATURE AMONG WHITE AND COLORED PEOPLE DURING THE YEARS 1912–1914 ON THE BASIS OF THE DEATH RATE

Place	White	Col'd	Place	White	Col'd	Place	White	Col'd
I. *Northern cities*			II. *Central cities*			III. *Southern cities*		
New York	68.5	71.4	Baltimore	60.4	69.2	Atlanta	59.2	72.4*
Chicago	65.5	69.4	Louisville	68.0	74.5	Birmingham	73.3	72.2
Philadelphia	68.0	74.0	St. Louis	70.8	74.9	Memphis	68.2	76.8
			Washington	65.3	69.2	Richmond	66.4	66.4
Average	67.3	71.8	Average	66.6	72.0	Average	66.8	72.0

Weighted Grand Average: White 67.5, Colored 71.6

* This figure is doubtful. A much more pronounced minimum of deaths occurred at a temperature of 44°. This apparently means that a good many colored people come to Atlanta in the summer and go away in the winter.

Because of peculiar weather conditions, epidemics, or other causes the optimum during the years 1912–1914 was 2° or 3° higher than during the longer series of years indicated in a preceding table. Such differences are inevitable and need not here concern us. The important point of the present table is that as a rule the negroes seem to be at their best at a temperature 4° or 5° higher than that which is best for white people. This, however, is insignificant compared with the difference of 40° between the mean temperature of the Baltic home in which the white race probably developed and the African home of the negroes. Moreover, the average optimum temperature in northern, middle, and southern cities is almost identical. There is not the slightest hint that either the whites or the colored people by residing in the north or the south have become adjusted to a particular temperature. So far as these facts go, therefore, they suggest that man's adaptation to temperature is so deep seated and of such remote origin that it changes very slowly. Untold thousands of years of the contrasted environments of northwestern Europe and central Africa appear to have produced a

permanent racial difference of no more than 5° and possibly less, while the short time that the American people have been in their present surroundings appears to have caused no differentiation.

In spite of the apparent fixity of the optimum there appears to be a marked adaptation to other conditions. This is illustrated in table 5, where the smoothed death-rate for New York and Los Angeles at intervals of 2.5° is given in percentages of the average per year. The table brings out the far greater range of temperature at New York than at Los Angeles. It also brings out the curious fact that while the worst

TABLE 5

DEATHS IN VARIABLE AND UNIFORM CLIMATES

MEAN TEMPERATURE F.	NEW YORK CITY	BALTIMORE	JAPAN	SAN FRANCISCO	LOS ANGELES
30.0°	107.0	.....	.....	.....	.....
32.5°	106.5	109.5	.....	.....	.....
35.0°	105.5	106.0	.....	.....	.....
37.5°	104.5	104.5	107.0	.....	.....
40.0°	103.0	101.5	102.0	.....	.....
42.5°	102.0	100.0	100.5	.....	.....
45.0°	101.0	98.5	98.5	.....	.....
47.5°	100.5	97.5	97.5	.....	.....
50.0°	99.5	96.5	96.5	117	.....
52.5°	98.5	95.5	96.0	108	117
55.0°	97.5	94.0	95.0	102	113.0
57.5°	96.5	92.5	94.0	90	103.5
60.0°	95.5	91.0	93.0	91	97.0
62.5°	95.0	90.0	91.5	.....	93.0
65.0°	94.5	89.5	92.0	.....	91
67.5°	95.5	90.0	95.0	.....	86?
70.0°	98.0	91.0	98.0	.....	92
72.5°	106.0	94.5	102.0	.....	.....
75.0°	.....	102.0	105.5	.....	.....
77.5°	.....	118.0	109.5	.....	.....
80.0°	.....	.....	115	.....	.....
Range from highest to lowest	11.5	20	23.5	27	31

months at New York have only 107 deaths where the average is 100 and the best only about 94, the range at Los Angeles is from 117 to 86. With less than half as great a range of temperature Los Angeles has two and one-half times as great a variation in the death-rate. The health seekers who visit southern California in the winter account for a small part of this difference, but by no means for all of it. At the range of temperature prevailing in Los Angeles the New York death-rate varies only about 4% or less, while that of Los Angeles varies 31%. If the influx of health seekers explained the matter they would have to increase the population of Los Angeles by more than a quarter of its normal propor-

tions every year. Moreover, San Francisco where the number of health seekers is comparatively small, shows a contrast from season to season practically the same as at Los Angeles. I do not feel sure how to explain this. It looks, however, as if the variable climate of the east causes people to become comparatively resistant to changes of temperature, while the uniform climate of the Pacific coast although delightful in itself, causes people to lose the power of resistance and hence to succumb quickly under adverse conditions. In table 5 localities are arranged according to the variability of their climates. The range between the highest and lowest death rates for the year is least in New York where the climate is most variable, and greatest in Los Angeles where the climate is most uniform.

THE PARALLAX OF THE PLANETARY NEBULA N. G. C. 7662

By Adriaan van Maanen

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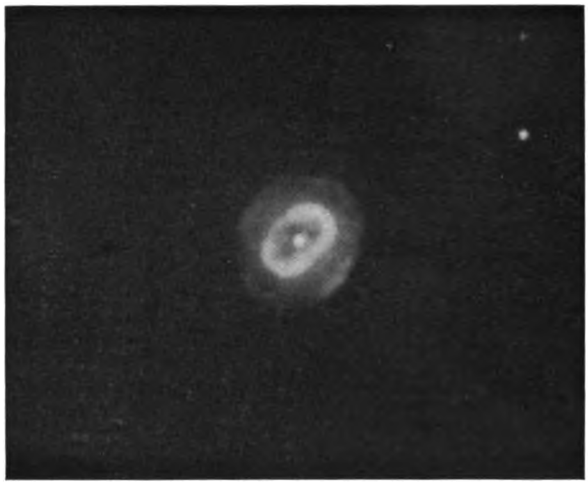
Communicated by G. E. Hale, December 24, 1916

The first results for the determination of stellar parallaxes obtained with the 80-foot focus arrangement of the 60-inch reflector were communicated to these PROCEEDINGS about two years ago.[1] The promising outcome of that investigation was fully confirmed by later results; the small probable error and the apparent absence of large systematic errors showed the desirability of extending the work to some of the nebulae. The parallaxes available for these interesting bodies are as yet extremely rare and the few existing results are far from trustworthy. For instance, about half a dozen determinations for the large spiral nebula in Andromeda have been published, the results ranging from $-0''.32 \pm 0''.12$ to $+0''.171 \pm 0''.051$[2].

Six nebulae were accordingly added to the Mount Wilson parallax program. In most cases the necessary exposure times are considerable and for several fields the work cannot be finished for some time. The planetary nebula N.G.C. 7662 (=H IV 18 Andromedae), which has a sharp stellar nucleus, gives good measurable images in a 25 minutes exposure (see Plate). Sixteen exposures were secured, the details of which are given in table 1. The successive columns contain the plate number, the date, the hour angle in degrees, the initials of the observer (H = Hoge, vM = van Maanen), the quality of the plates, the parallax-factor, the factor for annual proper motion, and finally some remarks.

The plates were measured with the monocular arrangement of the stereocomparator and reduced in exactly the same way as described in *The Photographic Determination of Stellar Parallaxes with the 60-inch Reflector*.[3] The eight pairs of plates with the necessary details are



N. G. C. 7662. PHOTOGRAPHED WITH THE 80-FOOT FOCUS ARRANGEMENT OF THE 60-INCH REFLECTOR, NOVEMBER 4, 1915. EXPOSURE 30 MINUTES, SEED 23 PLATE. SCALE 1 MM. = 1".6; ENLARGEMENT FROM ORIGINAL NEGATIVE, 5.3 DIAMETERS.

TABLE 1

N. G. C. 7662		$\alpha=23^h21^m6^s$			$\delta=41°59'$ (1900)		
Plate No.	Date	t	Obs.	Qy.	p	m	Remarks
	1915						
749	November 2	$-7^s.7$	vM	f	−0.699	+0.84	Hazy, 30^m exp.
750	2	+ 3.0	H	fg	−0.699	+0.84	Hazy, 30^m exp.
758	4	−13.2	H	fg	−0.719	+0.85	30^m exp. trail of Boss 6602
759	4	− 2.7	vM	fg	−0.719	+0.85	30^m exp.
	1916						
1050	July 23	− 5.7	H	f	+0.701	+1.56	
1061	August 3	−14.2	H	f	+0.574	+1.59	
1062	3	− 7.0	vM	f	+0.574	+1.59	
1063	3	+ 1.2	H	f	+0.574	+1.59	
1079	5	− 6.5	H	fg	+0.549	+1.60	
1080	5	0.0	vM	fg	+0.549	+1.60	
1081	5	+ 8.2	H	g	+0.549	+1.60	
1082	5	+15.0	vM	g	+0.549	+1.60	
1126	October 21	0.0	vM	f	−0.575	+1.81	
1127	21	+ 8.7	H	fg	−0.575	+1.81	30^m exp.
1143	November 16	+ 4.5	vM	g	−0.822	+1.88	Clouds, 28^m exp.
1163	17	− 5.5	H	fg	−0.827	+1.88	Hazy, 35^m exp.

given in table 2, whose successive columns contain the plate numbers of the pair, the difference in the parallax-factors, the difference in the factors for annual proper motion, the result of the measures (in 0.0005 of a revolution of the micrometer screw as unit), the residuals, and the difference in the hour angles.

Eight comparison stars were used; Table 3 gives their brightness, their co-ordinates in right ascension and declination, and their relative parallaxes with the probable errors of these parallaxes.

TABLE 2

MOR.-EV.	Δp	Δm	M	v	Δt
1061– 758	+1.29	+0.74	+29	+15	−1°.0
1062– 749	+1.27	+0.75	+ 3	−11	+0.7
1079–1163	+1.38	−0.28	+13	− 6	−1.0
1050– 759	+1.42	+0.71	+20	+ 3	−3.0
1080–1126	+1.12	−0.21	+ 8	− 6	0.0
1063– 750	+1.27	+0.75	+10	− 4	−1.8
1081–1143	+1.37	−0.28	+31	+13	+3.7
1082–1127	+1.12	−0.21	+10	− 4	+6.3

TABLE 3

COMP. STAR	Br	x	y	π	P. E.
1	bf	+0'.7	+0'.4	+0''.019	±0''.010
2	bf	+4.1	+2.1	− 3	6
3	f	+2.9	−2.3	− 2	9
4	f	+1.5	−2.2	− 11	8
5	f	−0.5	−3.3	+ 11	7
6	f	−2.3	−1.8	− 1	14
7	f	−3.0	+2.5	− 24	9
8	bf	−2.9	+5.1	+0.014	±0.005

The material in Table 2 gives the following normal equations for the determination of the parallax of the nebula:

$$+2.41\mu_\alpha + \ 2.61\pi = +\ 28$$

$$+2.61\mu_\alpha + 13.18\pi = +161$$

From these

$$\pi_{\text{rel.}} = +12.7 = +0''.021 \pm 0''.004$$

$$\mu_\alpha = -\ 2.1 = -0''.003 \pm 0''.008$$

Only one other determination of the parallax of this nebula is available, viz., that by Wilsing.[4] His material comprised 31 plates, taken with the 80-cm. photographic refractor at Potsdam. His conclusion was that the parallax of N.G.C. 7662 cannot be larger than 0''.1 or 0''.2.

Huss, rediscussing this material[5] derived a parallax of $-0''.063 \pm 0''.050$. It should be remarked that on most plates Wilsing measured, not the nucleus, but the whole nebula, which has a diameter of about 26″.

Professor Barnard has stated,[6] that the central star of the nebula is variable "to an extent upwards of three magnitudes; at times it has appeared as a bright yellowish star of about the 12th magnitude." The star appears to have practically the same magnitude on all my plates, varying from equality with comparison star 1 to a trifle brighter than comparison star 2. The visual magnitude of comparison star 1 is given by Barnard as 12.6.

To derive the absolute parallax of the nebula we must add approximately $0''.002$ to the value given above; the resulting parallax of $+0''.023$ would place the nebula at a distance of about 140 light-years. As the angular diameter of the whole nebula is about 26″, its linear diameter would thus be of the order of nineteen times that of the orbit of Neptune.

These results seem to be extremely promising and the program will accordingly be enlarged.

[1] Maanen, A. van, these PROCEEDINGS, 1, 1915, (187–189).
[2] Bigourdan, G., *Bul. Astr., Paris*, 26, 1909, (331).
[3] Maanen, A. van, *Mt. Wilson Contrib.*, No. 111, 1916, (1–33).
[4] Wilsing, J., *Astr. Nachr., Kiel*, 136, 1894, (349–352).
[5] Huss, E., *Ibid.*, 178, 1908, (95–98).
[6] Barnard, E. E., *London, Mon. Not. R. Astr. Soc.*, 68, 1908, (465).

ADULT HYMENOPTEROUS PARASITES ATTACHED TO THE BODY OF THEIR HOST

By C. T. Brues

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Communicated by W. M. Wheeler, December 21, 1916

Entomophagous parasites exhibit many remarkable adaptations which enable them more easily to locate the hosts necessary for the development of their young. Most of them depend upon the acuteness of their senses and we find consequently that they are usually very active and commonly exhibit complicated instincts to aid them in their search. Many forms deposit their eggs directly within or upon the bodies of their host, seeking either the eggs or the larvæ of the host for this purpose. When oviposition in larvæ occurs, it is necessary of course that these be located by the parasites after they have attained the requisite size. When the eggs of the parasites are placed within the eggs of the host, the latter must be located soon after they have

been deposited. Among the parasitic Hymenoptera there are many species which oviposit in the eggs of the host insect. In such cases the adult female parasite usually discovers the eggs of the host after they have been left upon the foodplant, or wherever they are to develop. The location of the host eggs in this manner must involve great difficulties as they are frequently well concealed.

A unique method of eliminating some of these difficulties has recently come to my notice in connection with some small parasitic Hymenoptera of the family Scelionidæ. Some of the members of this group develop in the eggs of locusts, and occasionally the adults attach themselves to the body of the locust. They are thus carried about by the host, and when the locust deposits its eggs, they undoubtedly seize the opportunity and leave it to deposit their own in those freshly laid by the locust.

The accompanying photographs illustrate an example which recently came into my possession, where the parasites remain upon the body of the locust. There are four parasites, all females, each still firmly attached by the jaws to the abdomen of the locust, notwithstanding the fact that the latter was placed in alcohol and subsequently sent me by parcel post from India. The mandibles are imbedded in the body at the sutures between the abdominal plates, and in each case the posterior margin of the forward segment is pushed back distinctly at the point of attachment, affording a very secure hold for the mandibles. Quite possibly the insects may be able to cling more tightly by reason of a sharp tooth which projects from each cheek, although this tooth does not occur in related forms with similar habits.

I do not believe that this habit is very general among the Scelioids which have been bred from locust eggs, although it has already been noticed. Ashmead in his Monograph of the North American Proctotypidæ[1] having taken a female specimen of *Sceliomorpha bisulca* Ashm. "holding on to the elytron of a short winged locust," and he suggests that it is there for the purpose of locating the place where the eggs of the host will be deposited. In this case the observation was made in Florida, but the species of locust is not indicated.

The Indian example figured in the present note comes from Walajanagar in South India, and is the common Jola or Deccan Grasshopper (*Colemania sphenarioides* Bolivar) which is a widely distributed species in India. Coleman who has studied this locust refers[2] to a small Hymenopterous parasite found in the eggs and it is probable that these parasites are the same species as the ones which I have received attached to the locust. As the species has not been described, a description is appended to the present note.

There seems to be no doubt that the behavior of these Hymenoptera in attaching themselves to the body of the locust is for the purpose of finding the eggs of the host more readily and it seems strange that the same method has not been adopted by other egg parasites. Possibly other large insects do not lend themselves so readily to this purpose, and it may be noted in this connection that both species of locusts which have been found with attached parasites are forms which do not have fully developed wings or tegmina in the female. Winged species might more easily disturb the parasites which cling only by the mandibles, and on account of their rapid movements might more easily evade the parasites.

In other groups of insects and also among Arachnids, there are familiar examples of small species which attach themselves to larger insects for the apparent purpose of transportation only. Thus certain mites are regularly found on the body of the housefly and similar flies, which presumably afford a convenient vehicle for migration, and also probably a source of food while in transit. In the case of certain mites of the genus Greenia which occur on large oriental bees (Xylocopa) a very peculiar relation exists between the two as the mites occupy a pocket in the basal segment of the bee's abdomen. Here, however, it has been assumed that the mites feed upon the pollen which adheres to the body of the bee.[3] Certain myrmecophilous beetles (e.g. Thorictus) regularly travel attached to the antennæ of the ants, and similarly the remarkable myrmecophilous cockroaches of the genus Attaphila commonly travel on the bodies of the leaf-cutter ants with which they live.

The attachment by the mandibles of the small Scelionids described in the present note suggests strongly that they may secure nourishment by sucking the juices of the locust. If such be the case, the phenomenon is interesting in connection with the fact that many minute parasitic Hymenoptera regularly feed upon the drop of liquid which exudes from the puncture made by the ovipositor when they deposit their eggs in the eggs or larvæ of the host. In this case the feeding precedes the act of oviposition and bears no direct relation to it since Scelionids are parasitic upon the eggs and not the imagines of the locust.

It seems probable that the Scelionid belongs to the genus Lepidoscelio Kieffer[4] which is based upon a species from Madagascar. As the type specimen of Kieffer's species had lost its antennæ and as the postscutellar scale appears to be less highly developed in the Indian species, I cannot feel sure that the two forms are congeneric.

? Lepidoscelio viatrix sp. nov.

♀ Length 4 mm. Black; legs, including coxæ, and antennæ, except club, honey-yellow; tegulæ rufous, apical margin of first abdominal segment fuscous; wings hyaline, venation very pale. Head strongly narrowed below the eyes, as high as broad above, but only half as broad at the base of the clypeus. Eyes one-half the head-height, with a very few short hairs. Ocelli in a low triangle, the lateral ones removed by

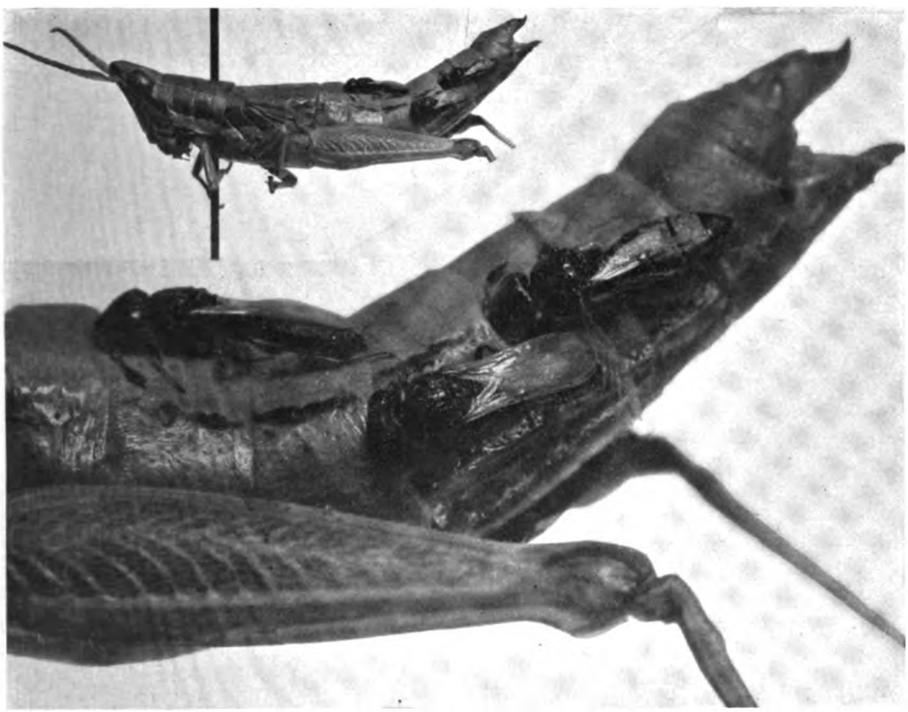

FIG. 1. SPECIMENS OF *LEPIDOSCELIO VIATRIX* SP. NOV. ATTACHED TO THE BODY OF THE DECCAN GRASSHOPPER (*COLEMANIA SPHENARIOIDES*).

less than their diameter from the eye-margin. Front on each side below the eyes with fine longitudinal aciculations, medially raised, the raised portion with sharp parallel sides anteriorly and projecting forward between the antennæ beyond the margin of the head; front between the eyes with somewhat irregular longitudinal striæ which run together above to form a smaller number above the median ocellus. Head behind with a carina extending from the occiput to the cheeks where it ends at the level of the lower eye-margin in a sharp tooth; just behind the eye is another weaker carina. Mandibles rather long,

acute at apex and with a single acute tooth inwardly before the apex; at the base they are attached at their inner angle so that when open the rectangular outer basal angle extends laterally beyond the head, but when closed the basal edge forms an extension of the sides of the head. Antennæ 12-jointed; scape half as long as the head height; pedicel half as thick as long; first three flagellar joints very small, the first triangular, the others transverse; club six-jointed widest at the base, club joints transverse except the last which is oval. Head deeply excavated behind on the occiput. Pronotum coarsely reticulate. Mesonotum without furrows, but with about nine somewhat irregular longitudinal carinæ. Scutellum convex, reticulate. Post-scutellum with a small vertical scale-like protuberance which is emarginate medially. Propodeum nearly flat above, emarginate at the middle on each side and with its posterior angles rounded; coarsely reticulate at the centre, very finely so at the sides. Pleuræ sparsely punctate. Abdomen broadly lanceolate, finely longitudinally aciculated, broadest at the apex of the third segment which is a little longer than the fourth; first shorter than the second, the two together as long as the third; fifth as long as the second, sixth very small. Wings extending to the tip of the abdomen; costal vein two-fifths as long as the wing; stigmal enlargement well developed, but very light yellow in color; stigmal vein very weak, oblique.

Described from four females received from Walajanagar, North Arcot District, South India.

The form of the mandibles and head is unusual, and may be associated with the peculiar habit of traveling on the body of the locust as described on a preceding page.

[1] *Washington, Smithsonian Inst., Nation. Mus. Bull.*, No. 45, 1893. (241).

[2] *Madras, Mysore State Dept. Agric. Entom.. Bull.*, (Ser. 2), No. 2 1911, (26).

[3] Since this article was written Dr. Joseph Bequaert has called my attention to a most remarkable adaptation of the same kind exhibited by certain Eumenid wasps. In some species of Odynerus (e. g., *O. conformis* Sauss.) there is a space between the dorsal plates of the first and second segments of the abdomen which is regularly occupied by a large colony of small mites. This flattened pocket can be opened or closed by the flexion of the wasp's abdomen.

[4] *Bruxelles, Ann. Soc. roy. Sci.*, 29, pt. 2, 1905, (129).

VOLUME 3 MARCH, 1917 NUMBER 3

PROCEEDINGS

OF THE

NATIONAL ACADEMY OF SCIENCES

OF THE

UNITED STATES OF AMERICA

Publication Office: Williams & Wilkins Company, Baltimore
Editorial Office: Massachusetts Institute of Technology, Cambridge
Home Office of the Academy: Washington, D. C.

Entered as second-class matter at the Post Office at Baltimore, Md., under the Act of August 24, 1912

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PROCEEDINGS

OF THE

NATIONAL ACADEMY OF SCIENCES

Volume 3 MARCH 15, 1917 Number 3

THE CONDENSATION AND EVAPORATION OF GAS MOLECULES

By Irving Langmuir

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Communicated by A. A. Noyes, January 11, 1916

Several years ago,[1] I gave evidence that atoms of tungsten, molybdenum, or platinum vapors, striking a clean, dry glass surface in high vacuum, are condensed as solids at the first collision with the surface. Subsequently, similar evidence[2] was obtained in connection with a study of chemical reactions in gases at low pressures. It was concluded that in general, when gas molecules strike a surface, the majority of them "do not rebound from the surface by elastic collisions, but are held by cohesive forces until they evaporate from the surface." In this way a theory of adsorption was developed[3] which has been thoroughly confirmed by later experiments. It was stated: "The amount of material adsorbed depends on a kinetic equilibrium between the rate of condensation and the rate of evaporation from the surface. Practically every molecule striking the surface condenses (independently of the temperature). The rate of evaporation depends on the temperature (van't Hoff's equation) and is proportional to the *fraction* of the surface covered by the adsorbed material."

R. W. Wood[4] described some remarkable experiments in which a stream of mercury atoms impinges upon a plate of glass held at a definite temperature. With the plate cooled by liquid air, all the mercury atoms condense on the plate, but at room temperature all the atoms appear to be diffusely reflected.

The whole question of the evaporation, condensation, and possible reflection of gas molecules has been discussed at some length in two recent papers[5,6]. It was pointed out that, in Wood's experiments, there are excellent reasons for believing that the mercury vapor actually condenses on the glass at room temperature, but evaporates so rapidly

that no visible deposit of mercury is formed. Further evidence of the absence of reflection is furnished by the operation of the 'Condensation Pump."[7]

In a second paper, Wood[8] gives an account of some still more striking experiments. A stream of cadmium atoms, striking the walls of a well exhausted glass bulb, does not form a visible deposit unless the glass is at a temperature below about −90°C. If, by cooling the bulb for a moment with liquid air, a deposit is started, this continues to grow in thickness even after it is warmed to room temperature. From these and similar observations, Wood concludes that:

1. Cadmium atoms all condense on cadmium surfaces at any temperature.

2. Cadmium atoms condense on glass only if it is at a temperature below about −90°C. At higher temperatures, nearly all the atoms are reflected.

This viewpoint leads to no explanation of the changes in the reflection coefficient. The results of Wood's experiments may, however, be explained by the theory that all the atoms, striking either the glass or the cadmium surface, condense, and that subsequent evaporation accounts for the apparent reflection.

Cadmium atoms on a glass surface are acted on by totally different forces from those holding cadmium atoms on a cadmium surface. When a thick deposit of cadmium which has been distilled onto glass in vacuum, is heated quickly above its melting-point, the molten cadmium gathers together into little drops on the surface of the glass. In other words, molten cadmium does not wet glass. Therefore cadmium atoms have a greater attractive force for each other than they have for glass. Thus, single cadmium atoms on a glass surface evaporate off at a lower temperature than that at which they evaporate from a cadmium surface. It is not unreasonable to assume that in Wood's experiments, even at −90°C., the cadmium evaporated off of the glass as fast as it condensed upon it.

This theory possesses the advantage that it automatically explains the apparent reflection of cadmium atoms from a glass surface at room temperature, and indicates why this effect should be absent at low temperatures.

We shall see, moreover, that this condensation-evaporation theory explains many other facts incompatible with the reflection theory.

Let us examine for a moment the essential differences between these two theories. Wood describes his remarkable experimental results, but he has not attempted to discuss the mechanism of the underlying

processes. It is clear that Wood uses the term 'reflection' merely to express the fact that under certain conditions no visible deposit is formed when the atoms strike a surface. From this point of view, condensation followed by evaporation is the same as reflection. In considering the possible mechanisms of the process, however, we must sharply distinguish between the two theories.

When an atom strikes a surface and rebounds elastically from it, we are justified in speaking of this process as a reflection. Even if the collision is only partially elastic, we may still use this term. The idea that should be expressed in the word 'reflection' is that the atom leaves the surface by a process which is the direct result of the collision of the atom against the surface.

On the other hand, according to the condensation-evaporation theory, there is no direct connection between the condensation and subsequent evaporation. The chance that a given atom on a surface will evaporate in a given time is not dependent on the length of time that has elapsed since the condensation of that atom. Atoms striking a surface have a certain average 'life' on the surface, depending on the temperature of the surface and the intensity of the forces holding the atom. According to the 'reflection' theory, the life of an atom on the surface is simply the duration of a collision, a time practically independent of temperature and of the magnitude of the surface forces.

To determine definitely which of the two theories corresponds best with the facts, I have repeated Wood's experiments under somewhat modified conditions. A small spherical bulb, together with an appendix containing cocoanut charcoal, was heated to 600°C. for about four hours while being exhausted by a condensation pump. A liquid-air trap was placed between the pump and the bulb. Some cadmium was purified by distillation and was distilled into the bulb, which was then sealed off from the pump. The cadmium in the bulb was then all distilled into the lower hemisphere. By heating this lower half of the bulb to about 140°C., the upper half remained clear, but by applying a wad of cotton, wet with liquid air, to a portion of the upper hemisphere, a uniform deposit formed within less than a minute and continued to grow, even after the liquid air was removed.

When the liquid air was applied only long enough to start a deposit, it was found in the first experiments that the deposit did not grow uniformly, but became mottled, or showed concentric rings. The outer edges of the deposit were usually much darker than the central portions. By cooling the cocoanut charcoal in liquid air, this effect disappeared entirely and the cadmium deposits became remarkably

uniform in density. It is thus evident that traces of residual gas may prevent the growth of the deposit, particularly in those places which have been the most effectively cooled. This is probably due to the adsorption of the gas by the cooled metal deposit. This gas is apparently retained by the metal, even after it has warmed up to room temperature, so that vapor condensing on the surface evaporates off again at room temperature.

These results indicate how enormously sensitive such metal films are to the presence of gas. However, by using liquid air and charcoal continually during the experiments, most of these complicating factors were eliminated.

If all the cadmium is distilled to the lower half of the bulb and this is then heated to 220° in an oil bath while the upper half is at room temperature, a fog-like deposit is formed on the upper part of the bulb in about fifteen seconds. This deposit is very different from that obtained by cooling the bulb in liquid air. Microscopic examination shows that it consists of myriads of small crystals. According to the condensation-evaporation theory, the formation of this fog is readily understood. Each atom of cadmium, striking the glass at room temperature, remains on the surface for a certain length of time before evaporating off. If the pressure is very low, the chance is small that another atom will be deposited, adjacent to the first, before this has had time to evaporate. But at higher pressures this frequently happens. Now if two atoms are placed side by side on a surface of glass, a larger amount of work must be done to evaporate one of these atoms than if the atoms were not in contact. Not only does the attractive force between the cadmium atom and the glass have to be overcome, but also that between the two cadmium atoms. Therefore the rate of evaporation of atoms from pairs will be much less than that of single atoms. Groups of three and four atoms will be still more stable. Groups of two, three, four, etc., atoms will thus serve as nuclei on which crystals can grow. The tendency to form groups of two atoms increases with the square of the pressure, while groups of three form at a rate proportional to the cube of the pressure. Therefore the tendency for a foggy deposit to be formed increases rapidly as the pressure is raised or the temperature of the condensing surface is lowered.

On the other hand, according to the reflection theory, there seems to be no satisfactory way of explaining why the foggy deposit should form under these conditions.

Experiments show clearly that when a beam of cadmium vapor at very low pressure strikes a given glass surface at room temperature,

no foggy deposit is formed, although when the *same quantity* of cadmium is made to impinge against the surface in a shorter time (and therefore at higher pressure) a foggy deposit results. This fact constitutes strong proof of the condensation-evaporation theory.

A deposit of cadmium of extraordinary small thickness will serve as a nucleus for the condensation of more cadmium at room temperature. Let all the cadmium be distilled to the lower half of the bulb. Now heat the lower half to 60°C. Apply a wad of cotton, wet with liquid air, to a portion of the upper half for one minute, and then allow the bulb to warm up to room temperature. Now heat the lower half of the bulb to 170°C. In about thirty seconds a deposit of cadmium appears which rapidly grows to a silver-like mirror. This deposit only occurs where the bulb was previously cooled by liquid air.

The question arises: how much cadmium could have condensed on the bulb in one minute while the lower part of the bulb was at 60°C.?

The vapor pressure of cadmium has been determined by Barus[9] between the temperatures 549° and 770°C. If the logarithms of the pressures are plotted against the reciprocals of the temperature, a straight line is obtained from which the following equation for the vapor pressure (in bars) is obtained as a function of absolute temperature

$$\log p = 11.77 - \frac{6060}{T} \tag{1}$$

At 60°C. the vapor pressure of cadmium is of the order of magnitude of 4×10^{-7} bars. Now the number of molecules of gas which strike a square centimeter of surface per second is

$$n = 2.65 \times 10^{19}\, p/\sqrt{M\,T} \tag{2}$$

Substituting $M = 112$, $T = 333°$, and $p = 4 \times 10^{-7}$, we find that with saturated cadmium vapor at 60°C., $n = 5 \times 10^{10}$ atoms per second per square centimeter.

The maximum number of atoms of cadmium which can condense in *one minute* on a spot cooled in liquid air when the lower part of the bulb is at 60°C. is therefore 3.0×10^{12} atoms per square centimeter. The diameter of a cadmium atom is approximately 3.1×10^{-8} cm., so that it would require 1.0×10^{15} atoms to cover 1 square centimeter with a single layer of atoms.

Therefore the deposit which forms in one minute with the vapor from cadmium at 60°, contains only enough cadmium atoms to cover 3/1000 of the surface of the glass. Yet this deposit serves as an effective nucleus for the formation of a visible deposit.

If the lower part of the bulb is heated to 78° instead of 60°, the

nucleus formed by applying liquid air for one minute causes a visible deposit to grow more rapidly (with the lower part of the bulb at 170°). But the nucleus obtained with temperatures above about 78° are not any more effective than those formed at 78°.

A calculation similar to that above shows that the deposit formed in one minute at 78° contains 2.5×10^{13} atoms per square centimeter, or enough to cover 25/1000 of the surface. If we consider that the surface of the glass contains elementary spaces each capable of holding one cadmium atom, the chance that any given cadmium atom will be adjacent to another is $1 - (1 - 0.025)^7$, or 0.16. When the surface is allowed to warm up, the single atoms evaporate, but the pairs remain. The surface is then covered to the extent of 16% of 25/1000, or 4/1000. About 2% of the atoms striking such a surface will fall in positions adjacent to those atoms already on the surface. With cadmium vapor at 170°, 1.4×10^{15} atoms per square centimeter strike the surface each second, so that 2.8×10^{13} would condense in the first second around the 4×10^{12} atoms remaining on the surface. Thus in only a few seconds the whole surface becomes covered with a layer of cadmium atoms. This explains why a surface only partially covered with cadmium atoms can serve so effectively as a nucleus. If a much smaller fraction than 0.025 of the surface is covered, however, there is a long delay in completing the first layer of atoms, so that the visible deposit is formed much more slowly.

The above experiments prove that the range of atomic forces is very small and that they act only between atoms practically in contact with each other. Thus a surface covered by a single layer of cadmium atoms behaves, as far as condensation and evaporation are concerned, like a surface of massive cadmium. This absence of transition layer is in accord with my theory of heterogeneous reactions.[10]

One of the best proofs of the correctness of the condensation-evaporation theory was obtained in experiments in which nuclei formed at liquid air temperature, were not allowed to warm up to room temperature, but only to −40°C. In this case the nuclei were formed in one minute from cadmium vapor at 54°C. The nuclei which were kept at −40°C. developed rapidly into cadmium mirrors in cadmium vapor at 170°, while those at room temperature developed extremely slowly. A still more striking demonstration of the theory was obtained when one of the nuclei was allowed to warm up to room temperature and then cooled to −40° before exposure to cadmium vapor at 170°. This nucleus did not develop nearly as rapidly as that which had not been allowed to warm up to room temperature.

These experiments prove that single cadmium atoms actually evaporate off of a glass surface at temperatures below room temperature, although they do not do so at an appreciable rate from a cadmium surface.

This theory affords a very satisfactory explanation of Moser's breath figures on glass and the peculiar effects observed in the formation of frost crystals on window panes. In fact, the theory appears capable of extension to the whole subject of nucleus formation, including, for example, the crystallization of supercooled liquids.

The final paper will be submitted to the *Physical Review* for publication.

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THE NINTH SATELLITE OF JUPITER

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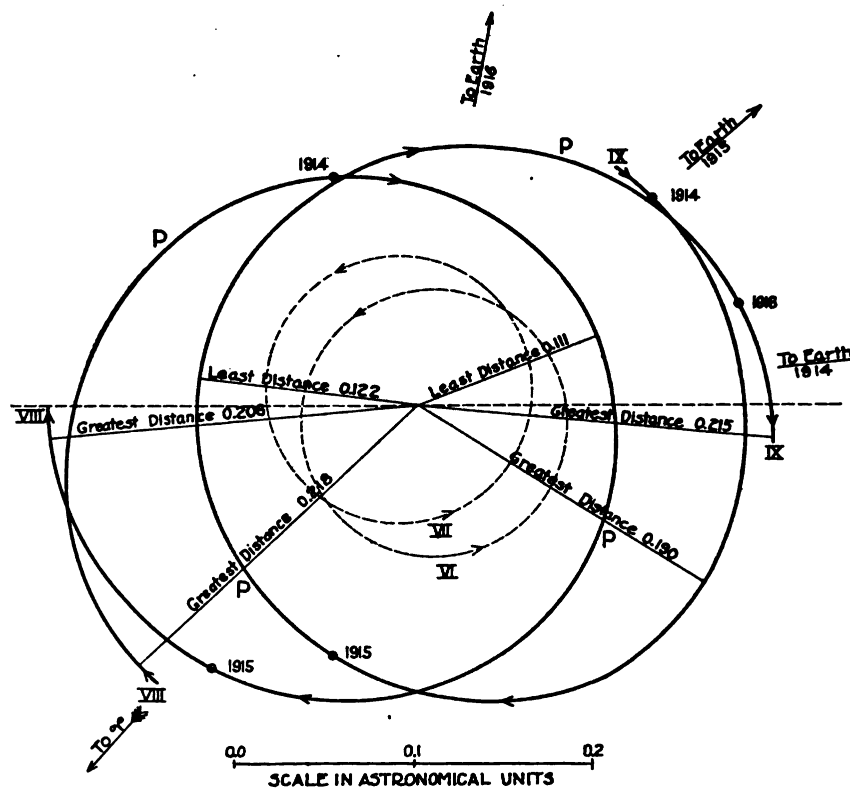
Communicated by G. E. Hale, January 18, 1917

The Ninth Satellite of Jupiter, whose discovery was announced in these PROCEEDINGS, 1, 1915, (12), was rediscovered at the Lick Observatory during the 1915 opposition and the observations secured then were forwarded to me before publication. On the basis of these positions the preliminary orbit,[1] derived from observations in 1914 at the time of discovery, was approximately corrected so that the satellite could be more easily located at the 1916 opposition. Four images of it have been found on plates taken here by Mr. Shapley with the 60-inch reflector. Although the computed position was in error by about 2′ there can be no doubt as to the identification of the object.

Observations are now available for three oppositions and the orbit is being corrected to satisfy them. Although the final elements will differ considerably from those now available, our present knowledge of

the satellite's motion is sufficient to give a fair idea of the principal characteristics of the orbit. These are best exhibited by the accompanying diagram which shows the orbit projected on a plane (inclined to the ecliptic at an angle of 24°) from which it does not deviate greatly during 1914 and 1915. At the end of 1916, however, the satellite as seen from Jupiter was about 4° from this plane. The dotted line in the diagram shows the intersection of the projection plane with the plane of the



ORBITS OF JUPITER'S EIGHTH AND NINTH SATELLITES

ecliptic. The feathered arrow points in the direction of the vernal equinox. Since the motion of the satellite is retrograde, the corresponding values are for i and Ω are 156° and 310°, respectively. For comparison, the orbit of the Eighth Satellite from the investigation by A. C. D. Crommelin[2] is also shown, projected on the same plane as the orbit of the Ninth Satellite. The angle between the two orbit planes is about 10°. The points where the satellites pass above and below the plane of projection are designated by the letter P. The small circles indicate the positions of

both satellites at the times of Jupiter's opposition in 1914 and 1915 and of the Ninth Satellite in 1916. The corresponding directions of the earth are shown by the arrows, and the relative positions of the earth and the two satellites explain how in 1914 the new satellite was found on the same plate as the Eighth. The orbits of the Sixth and Seventh Satellites, which are also shown in the figure, are drawn to scale but have not been projected into the plane of the figure.

The perturbations produced by the sun are very large for both satellites on account of their great distance from the primary. The orbits, therefore, are not even approximate ellipses; their points of greatest and least distance from Jupiter are indicated. It is of course impossible from the data now available to say much about the mean elements of the Ninth Satellite, but the mean period is likely not far from 745 days. The eccentricity appears to be a little less than that of the Eighth Satellite.

The magnitude of the Ninth Satellite was found by Mr. Shapley to be 18.3 on October 18 and 19, 1916,[3] which corresponds to 18.6 at mean opposition. With reasonable assumptions for the albedo and color index, this would indicate a diameter of about 15 miles. As seen from Jupiter the satellite at full phase would be between the 11th and 12th magnitudes, depending on its distance from the planet.

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AORTIC CELL CLUSTERS IN VERTEBRATE EMBRYOS

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Communicated by A. G. Mayer, December 1, 1916

Aortic cell clusters in mammals were first described by Maximow[1] (p. 517) in rabbit embryos. Minot[2] (p. 523) subsequently described similar structures in human embryos of from 8 to 10 mm. length and in rabbit embryos. Emmel[3] reported aortic cell clusters in rat embryos, rabbit embryos, and in pig embryos of from 6 to 15 mm. Jordan[4] discovered these clusters in pig embryos (10 to 12 mm.) at about this same time, and reported their presence also in mongoose and turtle embryos. Emmel[5] later published a detailed description of the aortic clusters of the pig embryo. Meanwhile I had observed them also in chick embryos of 3 to 4 days' incubation. Dantschakoff[6]

had already reported similar structures in the chick. Aortic cell clusters would seem to be a common feature of certain early stages of vertebrate development.

All of the above-named investigators agree in interpreting the constituent cells of the clusters as hemoblasts, variously designated as 'lymphocytes,' 'mesameboids,' progenitors of macrophages ? hemoblasts. Minot[2] alone disagrees with the otherwise unanimous conclusion that they represent endothelial differentiation products. That they are endothelial derivatives, however, rather than accretion products of hemoblasts from the circulating embryonic blood is easy of demonstration. Aortic cell clusters represent one phase of the general hemogenic capacity of embryonic endothelium.

The doctrine of the partial endothelial origin of hemoblasts from embryonic endothelium has become associated with the monophyletic hypothesis of blood cell origin (Maximow)[1] that of the non-hemogenic capacity of endothelium with the polyphyletic and diphyletic hypotheses (Stockard).[7] The question of the genetic relationship between endothelium and certain cellular elements of the embryonic blood touches also the 'angioblast' theory of His. The two chief tenets of this theory are : (1) the inability of intraembryonic mesenchyme to produce blood vascular tissue, and (2) the incapacity of endothelium to differentiate into blood cells. Abandonment of the first tenet has been forced largely through the experimental work of Hahn,[8] of Miller and McWhorter,[9] of Reagan,[10] and of Stockard;[7] and the morphologic studies of Schulte[11] on the cat embryo, those of McClure[12] on the trout embryo, and the studies of Huntington[13] on the development of the lymphatics in amniotes. The surrender of this portion of the original theory is gradually being made by some of its staunchest advocates (vide Sabin[14]). The disproof of the second tenet is the chief burden of this investigation.

The material studied includes three mongoose embryos of 5, 6, and 7 mm. respectively, a series of pig embryos of from 8 to 12 mm., chick embryos of the third and fourth day of incubation, and a series of twenty loggerhead turtle embryos ranging from the second to the thirty-second day of incubation.[15] These embryos are variously preserved and stained, the several methods including fixation with Helly's fluid and staining with Giemsa's solution.

This description confines itself almost exclusively to the 5 mm. mongoose embryo and the 12 day turtle embryo. This selected material is at just the proper stage of development to furnish the key for the correct interpretation of the larger aortic clusters of the 10 mm. pig embryos.

The study was approached by way of the yolk sac of the mongoose embryo. The endothelial origin of hemoblasts can here be readily demonstrated. These observations on the mongoose yolk-sac confirm my[16] previous findings regarding the hemogenic rôle of yolk-sac endothelium in the 10 mm. pig embryo.

The second step involved a search for similar intraembryonic phenomena. It seemed reasonable to expect that, since the yolk-sac mesenchyme could differentiate directly into blood cells and into endothelium, and since the endothelium could subsequently transform into blood cells then the same order of events should probably follow also in the intraembryonic mesenchyme; and further, since mesenchyme is the fundamental hemogenic tissue, and since both endothelium and mesothelium in the embryo are only slightly modified mesenchyme (chiefly by means of the mechanical factor of pressure—vide Huntington,[13] p. 265), then embryonic mesothelium and endothelium should both in the only slightly differentiated condition be capable of producing cellular blood elements (hemoblasts).

That mesothelium can differentiate into vascular tissue has been shown by Bremer[17] in the case of the body stalk of a 1 mm. human embryo. Examination of the intraembryonic endothelium in the pig and mongoose revealed, in the smaller pericerebral blood channels, an occasional endothelial cell rounding up and taking on hemoblast features and finally separating from the endothelial wall; and led to the discovery and detailed study of the aortic clusters of hemoblasts, with the origin and significance of which this paper is largely concerned. Moreover, investigation of the pericardial mesothelium disclosed very similar clusters, both attached to the visceral and the parietal pericardium, and lying free within the pericardial cavity. Emmel[18] has recently described comparable structures in the 12 mm. pig embryo. Occasional individual cells can also be seen in process of separation from the visceral pericardium in the mongoose embryo.

The suggestion has been made that what is interpreted as a hemoblast in the act of differentiation and separation from the endothelium, is simply an endothelial cell in preparation for division. Many dividing endothelial cells from the pericerebral mesenchyme were observed with this suggestion in mind. But no endothelial cell, even at metaphase of mitosis, appeared rounded up in the same fashion as the differentiating hemoblast, nor to the same degree; at most it was merely a stout fusiform body, without the distinct unilateral bulge equatorially which is characteristic of the hemoblast during the later stages of its separation from the endothelium. Moreover, the nucleus of even the

stoutest endothelial hemoblast at this stage is in the typical resting condition.

As regards the aortic cell clusters, the 5 mm. mongoose embryo shows admirably various early stages in their origin and development, and so furnishes the key to the interpretation of the later products. And the 12 day loggerhead turtle embryo shows besides, the peculiar intravascular encapsulated cell clusters, and the endothelial strands, recently noted also for a 12 mm. pig embryo by Emmel[5] in a footnote to his paper (p. 407) on aortic cell clusters in mammals; and the conditions in this respect also are such as appear to solve the mystery of their genetic significance.

The aortic cell clusters in the mongoose embryo of from 5 to 7 mm. range from such as are composed of only a single cell to those composed of a score or more. Single cells or groups of two or three can be seen separating from the endothelium at any point, even along the mid-dorsal line. Larger groups are found only in the ventral and ventrolateral portions, frequently in more or less close relation to the mouths of the lateral mesonephric branches or the ventral intestinal rami. This proliferative activity of the aortic endothelium is present only in the abdominal portion of the aorta, approximately coextensive with the mesonephroi. Single endothelial cells may round up and take on hemoblast features and separate from the wall in exactly the same manner as that by which the hemoblasts are derived from the endothelium of the yolk-sac vessels and in the pericerebral vascular channels. The process is the same in the yolk sac and the embryo, and indicates a common hemogenic capacity of embryonic endothelium.

The mongoose material shows also the initial stages in the formation of the larger cell clusters. Throughout the ventral half of the abdominal aorta, the endothelium at certain points appears to buckle into the lumen. This invaginated area may be more or less extensive, and may include a considerable portion or none of the subjacent mesenchyme. The cause of the buckling remains obscure, though the suggestion lies close to hand that it may be related to the caudal shifting of the embryonic representatives of the celiac, superior mesenteric and inferior mesenteric arteries; a process dependent in part at least upon the presence of a less rigid and less differentiated endothelium ventrally, permitting thus of an inequality of growth as between the ventral and dorsal walls or allowing for the formation of successively lower connecting vascular segments for the migrating definitive stems.

The endothelium seems to be lacking centrally underneath the cell clusters. This is explained by the fact that the larger clusters arise by

an invagination of the endothelium over an area of some extent rather than by process of proliferation of one or several differentiating endothelial cells. Proximally the clusters show transition stages between endothelial cells and hemoblasts (laterally) and between mesenchymal cells and hemoblasts (centrally). The subjacent mesenchyme of the larger clusters may become thickened, sometimes assuming the features of a stratified endothelium, and the nuclei are relatively much more abundant, smaller, and less differentiated. In the larger clusters, the peripheral cells, some of which show early erythroblast features, begin to separate from the central group. Within the clusters some of the cells are in mitosis, while the nuclei of others may appear at some phase of amitotic division; and an occasional cell may show phagocytic properties. Sometimes the core of the cluster shows transition stages between the endothelium or mesenchyme and hemoblasts. Many of the nuclei subjacent to the cluster appear at some phase of amitotic division. The absence of mitotic figures in the subjacent endothelium constituted the strongest of the three objections made by Minot[2] against the interpretation of the aortic clusters as endothelial derivatives. It would seem that the method of proliferation is here largely amitotic.

The aortic cell clusters of the mongoose embryo originate from the cells of an invaginated area of endothelium; they enlarge by intrinsic growth and differentiation, not by accretions from the circulating blood. Similar clusters appear also in the superior mesenteric artery. In a 10 mm. pig embryo a large aortic cluster, 130 microns in diameter, appears near the mouth of the superior mesenteric artery and consists of a hundred or more cells. Clusters appear also along the greater length of this definitive aortic stem.

In the 12 day loggerhead turtle embryo, encapsulated clusters and extensive strings of hemoblasts attached to the endothelium appear in the inferior vena cava, near the point of fusion of the original paired subcardinal veins; and in the jugular veins. The endothelial strands, some of the cells of which bear hemoblast features, are most probably only another aspect of the general hemogenic capacity of young endothelium. Emmel[5] saw similar strands in the proximal portion of the left umbilical artery, and in the aorta of this level, in two 12 mm. pig embryos, and suggests that they may be related to the fusion of the two original dorsal aortae. In the case of the development of the inferior vena cava, the coincident fusion between the originally separate post—and sub-cardinal veins involves the formation of young, less differentiated, endothelium and so offers a favorable site for hemoblast production by endothelium.

The encapsulated cluster present in this same region of the inferior vena cava may be explained as follows: Subjacent to such clusters the mesenchyma appears to be differentiating into hemoblasts; this observation may give the clue to the correct interpretation of these clusters. If the invaginating area of endothelium included a considerable portion of such differentiating (vascularising) mesenchyme, then the peripheral cells might possibly be so far outstripped in the expression of their hemogenic potentiality as to be forced, perhaps principally by reason of internal pressure from the differentiating and proliferating cells, to continue development along the line already begun, namely into definitive endothelium.

The whole series of phenomena above described seems to me to signify only various aspects of the same process, the hemogenic activity of embryonic endothelium. Though perhaps not essential, it appears nevertheless to be a normal process, consequent to the inherent capacity of endothelium to produce hemoblasts.

Emmel[5, 18] interprets the endothelial and mesothelial desquamation products, both cells and clusters, in terms of the stimulative effect of a pathologic factor upon the endothelium; a toxin whose source is in the degenerating cells of atrophying redundant ventral aortic rami, and in degenerating erythrocytes in the serous cavities in the case of the mesothelia.

That atrophying vascular stems are present at this stage, both in relation to the aorta, and the inferior vena cava, cannot be disputed. In the 7 mm. mongoose embryo the solid regressive ventral aortic stems are especially conspicuous. At least a portion of the caudal shifting of the three large aortic rami is due to a progressive atrophy of upper portions of a connecting net of vessels. But coincident with this phase of a regressive development among the upper roots, there may possibly be a new formation of lower roots. I incline to see the cause of cluster formation in the latter possibility rather than in the former fact.

Great stress is laid by Emmel upon the structure of the atrophying rami. Some of these are occluded by intravascular collections of hemoblast-like cells, both in the 10 mm. pig embryo and in the 7 mm. mongoose embryo. With these intra-arterial cell masses some of the aortic clusters are intimately related. Emmel ascribes the presence of this intra-arterial mass to the stimulative action of a dilute toxin, presumably liberated by the regressive aortic branches. This explanation is suggested by an alleged comparable pathologic process where endothelium is believed by certain pathologists (e.g., Mallory[19]) to be stimulated to the formation of 'endothelial leukocytes' ('large mononuclear leuko-

cytes') by dilute toxins such as are produced by typhoid and tubercle bacilli. A more likely interpretation, it seems to me, would attribute the presence of the intra-arterial cell mass of the smaller rami to the relatively slightly differentiated character of the endothelium. The occlusion of the rami and the degeneration (karyorrhexis) of the cells would thus be a secondary effect of the constriction of the regressive atrophying vessels. In other words, the intra-arterial cell mass is not the *result* of the action of a toxin; but the occlusion and degeneration (and the possible formation of a 'toxic' substance) are all the related common sequalae of the shrinking of the atrophying vessel around a previously present, normally produced, mass of hemoblasts.

But the most damaging countervailing evidence to the interpretation of endothelial cell-cluster derivatives as the result of a stimulative toxin, consists in the fact that in the definitive superior mesenteric artery of the 10 mm. pig embryo numerous cell clusters appear, even in the middle third of the vessel which is a level that could not be directly affected by a possibly later further caudal migration of this artery through atrophy of its present connecting aortic root; and which shows not the slightest indication of atrophy or degeneration, either of the vessel wall or the included blood cells, other than the presence only of hemoblast clusters.

In all the forms studied (pig, mongoose, chick, turtle) the superior mesenteric artery also contains both clusters and individual cells in process of differentiation into hemoblasts. Moreover, not all the ventral rami which ultimately disappear contain clusters or intra-arterial cell masses; and occasional clusters are relatively far removed from the mouths of any aortic branches.

If some toxin were correctly held responsible for the endothelial activity in the formation of isolated and aggregated hemoblasts, it becomes very difficult to explain its localized effect. The same objection holds regarding the clusters in the superior mesenteric (vitelline) artery. If the source of the toxic substance is here supposed to be degenerating cells of the yolk sac, its influence should be felt far beyond this vessel itself, or the ventral portion of only the abdominal segment of the dorsal aorta. A certain amount of cell disintegration no doubt occurs over wide areas of embryonic vascularised tissue, and numerous primitive venous channels also disappear; but, though extensively studied by various investigators, no cell clusters have been reported except in the ventral portion of the dorsal aorta (and in this paper, in the inferior vena cava near the point of fusion of the subcardinal veins), levels where a less differentiated endothelium is present as well as re-

gressive vascular channels. Furthermore, no atrophy of vessels nor any uncommonly extensive disintegration of erythrocytes have appeared in the yolk sac at these early stages (7 mm. in mongoose embryo; 10 mm. pig embryo) and yet the endothelial derivation of hemoblasts is very active.

It may be emphasized that as regards the endothelial origin and the composition of the aortic cell clusters, and as regards the mesothelial origin of cellular elements of the serous fluids, Emmel and I are in essential agreement. But Emmel views these structures as the result of the presence of a stimulating toxin; I see in them only the expression of a normal inherent capacity of embryonic endothelium to produce blood cells. The explanation of the limited distribution of the clusters is to be found in a relationship to young or newly formed, only slightly differentiated, endothelium; rather than in a connection with regressive blood vessels and an associated toxic substance.

All the facts seem to fit better the hypothesis that the hemogenic activity of embryonic endothelium is a normal function at a certain stage of embryonic development, than that the causative stimulus is a toxin derived from degenerating vascular tissues.

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RHEOTROPISM OF EPINEPHELUS STRIATUS BLOCH

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Communicated by E. L. Mark and read before the Academy, November 14, 1916

An unusual, but orderly, arrangement displayed by several groupers or hamlets (a marine fish, *Epinephelus striatus* Bloch), confined in a cage through which flowed a current of fresh seawater, called my attention to their peculiar rheotropism. The tails of all were directed *into* the current. When this was shut off their arrangement became promiscuous, indicating that their novel posterior orientation was a true rheotropic response.

This phenomenon led me to investigate in detail the behavior of these fishes both in groups and individually, in order to determine whether this posterior orientation to a current—which, so far as I have been able to learn, is undescribed—is a normal response of the grouper. For this purpose a number of fishes were placed in the cage and a record was made of the positions which they assumed at two-minute intervals. These observations were made both at night and during different parts of the day. In one record, which is fairly typical, the positions of each of 7 fishes at 30 successive intervals—in all 210 observations—were noted. Of the 210 observations 141 showed the fishes to be tail into the current (posterior orientation), 67 side to the current (lateral orientation), and only 2 head into the current (anterior orientation). In order to determine whether posterior and lateral positions indicate different responses by individual fishes, or are simply phases of one reaction, fishes were studied singly. For this I used a small aquarium (30 × 20 inches) across which a moderate current of water flowed diagonally. Each of the fishes tested remained most of the time near the inlet in the region of the strongest current. It assumed in succession slightly different positions, chiefly by rotating the long axis of the body through an arc of 90° to 180° around its own center, which preserved a comparatively fixed position in the axis of the current, so that either one side of the body or the tail was at any given instant directed toward the current, into which, however, the head was never pointed. After assuming approximately a dozen such temporary positions, which required about three or four minutes, the grouper tailed directly into the current and remained in this position for about three minutes. It then began a second series of changes similar to the first. In this sequence of positions it is perhaps most natural to regard as a single

period the time occupied from the beginning of one set of 'swinging motions' to the beginning of the next; thus the posterior orientation, which is of longest duration, is the last phase of the orienting process. A characteristic reaction, then, of *Epinephelus striatus* is posterior or lateral orientation to a current. Since in a majority of cases the orientation is posterior, and since this is maintained longer than any other position, it is a fair inference that this is the significant reaction.

Various regions of the body were explored with a localized current (1/28 liter per second) directed through a long glass tube, the experimenter being invisible to the fish. The following areas were found to be sensitive, but the response varied in promptness. Stimulation of the lips (if prolonged it causes a relatively violent reaction) brought forth a response in 7 seconds; the caudal fin in 16 seconds; the dorsal fin in 22, the cheek and operculum in 25, and the side of the body in 30 seconds. From these observations it is clear that the lip region is much the most sensitive part of the surface of the body, and the possibility is at once suggested that the posterior orientation of the fish is a reaction which serves to protect this region from the action of the current. This seems the more probable from the fact that prolonged stimulation of this region by the current employed in the experiment always caused a rather extraordinary and violent reaction.

Several end organs have, at one time or another, been regarded as the receptors in the case of rheotropic responses: lateral-line organs[6] (Schulze, '70); the organs concerned in response to pressure[7] (Verworn, '97, p. 445ff., barotaxis), and the ear[6] (Tullberg, '03).

Two other views have been advocated, first, that it is the tactile corpuscles which are stimulated by currents[3,4] (Parker, '03a, '03b); secondly, that it is chiefly the eyes which are stimulated, this being due to the transportation of the fish through the water. According to this view[2] (Lyon, '04), rheotropic reactions are chiefly optic reflexes, which serve to compensate the apparent motion of the visual field. This is an indirect effect, a direct stimulation being produced only when the fish is in contact with some part of the solid environment. This, in the case of blind fishes, Lyon thinks acquaints the animal with its transportation and a compensatory swimming results.

In studying the question of rheotropic end organs, observations—to be described elsewhere—were made which confirm the idea that sense cells of pressure and equilibration are unaffected by water currents. The lateral-line organs and the eyes, which can be rendered functionless by appropriate operations, are also unaffected. The skin was next removed from certain body areas and the underlying tissue was

found to be insensitive to localized currents. This indicates that the rheotropic end organs are cutaneous; of these only the tactile corpuscles were found to be of significance. This is shown by the following experiments. Under normal conditions stimulation of the lips by a glass rod produces a very violent negative reaction, and so, too, does a current of water of the sort just described. The lips of a normal fish were anaesthetized by the application of a 0.1% solution of cocaine. As a result the reaction (and assumably the sensitivity) to tactile stimulus disappeared completely, and also the reaction to the water current. Not only that, but also the parallelism between the effects of the two sorts of stimuli at any instant, both during the gradual benumbing of the lips by the reagent and during their progressive recovery from insensitivity, seemed to be complete. These facts indicate that the end organs of tactile sensitivity serve also as the essential and organs of rheotropic sensitivity. Other sensory cells, while they may in some cases be affected by currents, apparently play no necessary part in the reaction here described.

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STUDIES OF THE GENUS PHYTOPHTHORA

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Communicated by R. Pearl, January 10, 1917

Although the actual number of species of Phytophthora is small, geographically they are very widespread, their presence having been recorded from the tropics as well as the temperate regions.

The morphological similarities between the different species, together with the great variation in the same species make the identification and separations of the species belonging to this genus exceedingly difficult. With a view to remedying this situation and determining characters of diagnostic value, nine out of the eleven described species were collected, grown in pure culture on artificial media, and studies made from a systematic and biometrical standpoint. It was not possible to procure material of *P. thalictri* and *P. colocasiae*. The following is a list of the cultures used:

P. infestans,	P. arecae,
P. cactorum,	P. parasitica,
P. phaseoli,	P. faberi,
P. nicotianae,	P. jatrophae,
P. syringae,	P. fagi, given as a synonym of P. cactorum in Saccardo.[1]

The characters to be relied on in taxonomic work must be constant under all conditions and not correlated with certain adaptations to habitat. In the work here set forth separation and relationship of species are made on the aggregate of characters, it being borne in mind that the proportionate value to be attached to each character must necessarily vary.

The most valuable characters that can be employed are doubtless those which are exhibited by the reproductive organs, and which furnish means of separating the genus into different groups. Other characters which have been found can be conveniently used in separating the various species, arranged as nearly as possible in the order of their importance, are (1) the size, and morphology of the sexual reproductive bodies, conidia, chlamydospores and other structures; (2) shape of the terminal papillum in the conidium; (3) the ratio of the length to the width of conidia; (4) variation in the mycelium, and (5) the macroscopic growth, time of appearance of spores and kind of spore forms produced on various media.

As artificial media were most generally employed, it seemed important to determine whether these media influenced the morphology and size of the various structures to any appreciable extent. Preliminary trials were made with *P. infestans* on potato foliage and artificial media, but no appreciable differences between the spore production on the potato foliage and those grown on artificial media could be detected. As far as possible, all the forms were subjected to identical conditions.

Measuring spore forms to delineate species in the various fungous groups is a well established practice. In forms less variable than those of Phytophthora a small number of such measurements might be sufficient to determine the average size. The conidia of Phytophthora, however, are so variable that a small number of measurements is apt to be misleading. For this reason four-hundred measurements were made of each form, including the length and width of conidia, the ratio of the length to the width and diameter of oospores and chlamydospores. On account of the great variability in the individual measurements, biometrical methods were used to obtain constants more readily com-

parable than a large mass of individual measurements. The constants obtained are the mean, median, mode, standard deviation and skewness. The measurements were made at the time of the first appearance of conidia and oospores, at which stage the cultures most nearly approach the normal. The cultures were kept at approximately 20°C.

After making a number of measurements of conidia and observing the great variation in size, it occurred to me that by continual plantings from large selected individuals cultures could finally be obtained which would produce only large conidia and vice versa. After making a large number of dilution plates and selecting single large spores for five generations, it was found that there was no material difference between the percentage of large and small conidia in the original culture and the culture resulting from the last selection.

The following summarizes the results of the above studies:[2]

1. The various forms reacted differently on different media as regards rate of growth and spore forms produced.

2. The temperature at which cultures are grown is a factor in the production of normal and comparable cultures.

3. As a minor character, the macroscopic appearance of the growth on a given medium is of some value.

4. The time of appearance of the spore forms from different strains of the same species on a given medium may vary, but eventually the same forms appear.

5. On oat agar the mycelia of the various species cannot be distinguished with any degree of certainty; on potato agar *P. syringae* can be distinguished from the remaining species by the fact that it produces characteristic tuberculate mycelia. *P. nicotianae* can likewise be distinguished to a certain extent by the more gnarled mycelia and greater abundance of globoid particles of a fatty or glycogen nature within the threads.

6. Measurement of the conidia can be employed to aid in delineating species, *provided* a sufficiently large number are measured. It is suggested that at least 200 should be measured and the different measurements grouped into classes.

7. For the purpose of obtaining a quantitive measure of the shape of the conidia the ratio of the length to the width of the conidia should be obtained and the ratios likewise grouped into classes. A comparison of the conidia of *P. parasitica* and *P. nicotianae* illustrates this point. Heretofore the differences in shape in the conidia of these two species would be expressed only qualitatively, calling the former

long and ellipsoidal, while the latter would be short and globose. Expressed quantitively, as a result of measuring the length and width of 400 individuals of each species and obtaining the ratios of the length to the width, the conidia of *P. parasitica* group themselves about a predominating ratio of 2, while *P. nicotianae* are found at 1.2. Similarly the ratios of the conidia of all the remaining species will vary from 1 + to 2 +.

8. The degree of the development of the papillum is a good character to be employed in taxonomic work.

9. In their germination of conidia by means of swarm spores, at least some of the species of Phytophthora liberate their zoospore mass into a bladder or vesicle, thus showing a greater relationship to Pythium than had been suspected.

10. Certain of the species of Phytophthora produce chlamydospores either terminally or intercalarly.

11. The measurement of the chlamydospores can be used to good advantage in delineating species. A sufficiently large number should be measured as in the case of the conidia.

12. The relation of the antheridium to the oogonium, that is whether produced at the base or on a side, can be used in separating the genus into groups, viz.: Cactorum group and Phaseoli group.

13. An additional group (Faberi group), analogous to the Fungi Imperfecti group, and embracing forms in which antheridia are absent or where their relation to the oogonium is yet unknown, is tentatively established.

14. In one of the several strains of *P. infestans* oospore-like bodies resembling those observed by other investigators were produced. Antheridia, however, were either absent, or of a very doubtful nature if present.

15. As in the case of conidia and chlamydospores, a sufficiently large number of measurements of the oospores should be made if the measurements are to be used in identifying a species.

16. On account of the variability in the size of the spore forms belonging to this genus, a more ready comparison can be made by the use of biometrical constants than by a mass of individual measurements. Such biometrical constants were calculated and arranged into tables. By the use of these constants the identity and relationship of the species are further confirmed.

17. In closely related genera, such as Pythium, Pythiacystis, Peronospora, Plasmopora, and Sclerospora, the identification and separation of species are made with difficulty. Many of the diagnostic

characters enumerated in this paper can be employed to good advantage in the identification of species belonging to these genera.

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A POSSIBLE FUNCTION OF THE IONS IN THE ELECTRIC CONDUCTIVITY OF METALS

By Edwin H. Hall

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Read before the Academy, November 14, 1916

The following paper is a development, in certain particulars, of a suggestion concerning electric conduction in metals which I made in *Il Nuovo Cimento* for January-February, 1915. It rejects the free electrons within the metal as the vehicle of the electric current and puts in their place for this function the metal ions, necessarily equally numerous with the free electrons. Briefly stated, the idea of conduction which I have had in mind is the following:

At very low temperatures the atoms of a metal are packed so closely that electrons pass readily from one to another in a continuous procession through the metal, if there is an applied electromotive force to maintain progression in one direction. Free electrons and metal ions, if indeed ionization exists at the lowest temperatures, need not be called into action here.

With rising temperature the heat vibrations separate the atoms so that they are not always in conductive contact, and not very many degrees above the absolute zero they are, on the average, so far apart that conductive contact between them is exceptional.

In this state of things an electron will not in fact pass from one metal particle to another, even when they are in the closest contact of a collision, unless one of these particles is an atom and the other an ion. Figures 1 and 2 illustrate the fundamental difference, from the point of view of conduction, between an atom-atom collision and an atom-ion collision. In figure 1 the electric force tending to carry the extra electron of the left-hand particle back to the positively charged right-hand particle, just before the particles separate, must be enormously larger[1] than the greatest conteracting force we can apply from without. But in figure 2 the extra electron may belong as much to one particle as to the other, when once the particles meet, and a comparatively small general gradient of potential in the metal, due to an electromotive force

applied from without, may well determine what course the electron in question will take.

The experiments and arguments of Professor Bridgman now make it seem probable that I have greatly underestimated the range of temperature through which the normal atoms may, in spite of the heat vibrations, be regarded as remaining in conductive contact with each other, and that, accordingly, the important function which my theory has given to the ions may not belong to them, except perhaps at relatively high temperatures or in the liquid state of the metals.

Nevertheless, the potentiality of the ions for conduction is so great, under certain conditions which do not appear impossible, that it seems worth while to make some exposition of it.

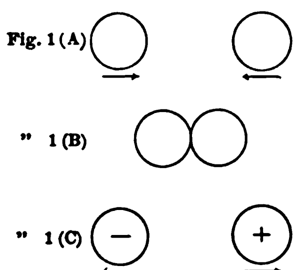


FIG. 1. TRANSMISSION COLLISION BETWEEN TWO ATOMS. IMPROBABLE.

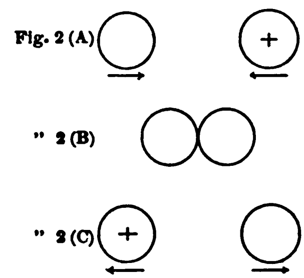


FIG. 2. TRANSMISSION COLLISION BETWEEN AN ATOM AND AN ION. NOT IMPROBABLE.

Three questions will be considered:

1. How numerous must the ions be in order to maintain currents of great density?

2. Would the conductive action of the ions conform to Ohm's law?

3. What should be the temperature relations of conductivity, if it is due to ions?

1. Requisite Number of Ions per Unit Volume.—The number of ions required for maintenance of electric currents, even currents of great intensity, may be small compared with the number of atoms—that is, the degree of ionization may be low. To show this let us consider, for example, copper at 300°C. absolute.

German investigators have shown that the specific heats of metals at low temperatures can be satisfactorily accounted for on the hypothesis that the frequency of to and fro vibration of the atoms is nearly independent of the temperature. Grüneisen takes from Nernst and Lindemann 320 as the value of $\beta\nu$ for copper, where ν is the frequency in question and $\beta = 4.8 \times 10^{-11}$. Accordingly we have

$$\nu = 320 \div 4.8 \times 10^{-11} = 6.7 \times 10^{12}.$$

The distance from center to center of copper atoms at 0° absolute and zero external pressure must be about 2×10^{-8} cm., and we may take this as the normal value of the atomic diameter. At ordinary room temperatures the distance from center to center is probably only about 0.5 of 1% greater.

Let us now, taking for simplicity conditions which must be modified later, imagine a row of atoms and ions arranged along a straight line in the direction of the potential gradient, maintained from without, in the metal, the mean distance from center to center being 2×10^{-8} cm., and let us suppose each particle to vibrate in its heat motion exactly in the straight line of the row in question, from collision at the left to collision at the right and back again, 6.7×10^{12} times per second. Let us suppose every ion in the row to capture an electron at every left-hand collision, thus transferring the ion condition to its left-hand neighbor.

All these assumptions lead to the conclusion that from every ion in the row the ion condition would travel along the row at the rate of $2 \times 10^{-8} \times 13.4 \times 10^{12}$, or 268,000, cm. per second. This is equivalent to one electron traveling with the same velocity. Acting thus one ion per linear centimeter would enable a conductor to maintain a current of $268{,}000 \times 15 \times 10^{-21}$, about 4×10^{-15}, electromagnetic units; and 10^{15} such ions per linear centimeter would carry a current of 40 amperes.

In a copper rod of unit cross-section there are about 88×10^{21} atoms per linear centimeter—that is, 3.5×10^{6} times the number of ions required to carry a current of 1000 amperes, if these ions were all as effective as they are in this preliminary calculation supposed to be.

We must now let fall some of the favoring assumptions of this calculation, in order to approach more nearly to actual or possible conditions. Instead of assuming every vibration to be in the line of the potential gradient, we must imagine the progress of the ion condition to follow a zig-zag course, the mean distance of travel in the direction of the potential gradient being not 2×10^{-8} cm., but, let us say, 10^{-8} cm. The great departure, however, from numerical truth was made in assuming that every ion gains an electron at its left-hand collision—that is, at the negative-potential end of its heat path. This is equivalent to assuming that all the existing ions are moving in one direction, from + toward −, through the metal. In fact, the gradient of potential, even if very steep, would probably determine or inhibit the passage of an electron in only a very small proportion of all the collisions between atoms and ions. Let us suppose that a gradient of potential sufficient to main-

tain a current of 1000 amperes per square centimeter through copper will decide for or against the transition of an electron in one out of every 10,000 atom-ion collisions.

With this revision of our conditions we find that the number of ions required to maintain a current of 1000 amperes per square centimeter is about 1 for every 170 atoms. This degree of ionization, at medium temperatures, does not seem improbable. At least, it cannot be objected to on the ground that it would make the specific heat of the metal too large.

2. Ohm's Law.—The mere fact that so small a portion of all the atom-ion collisions need be decisively affected by the potential gradient in order to maintain large currents raises a very strong presumption in favor of the proposition that the number of cases so affected will be proportional to the steepness of this potential gradient. Wherever we find an effect which begins with the beginning of a certain kind of stress and is tested through only a small part of its possible range, the magnitude of the effect in question is found to be proportional to the magnitude of the producing stress.

3. The Temperature Relation.—The theory of conduction which we are considering does not, at present, seem capable of giving a satisfactory account of the relation between conductivity and temperature. It would make the conductivity increase, remain constant, or decrease, according to circumstances, with rise of temperature. But, even so, it is worth examining somewhat further; for in some fashion it may at least supplement whatever main theory of conduction shall be found to hold.

In the following discussion certain assumptions are introduced which might require modification upon further study:

A free electron striking an atom is supposed to rebound or to enter the atom driving out another electron.

A free electron striking an ion is supposed always to unite with it to make an atom.

Δ, the total increase of distance from centre to centre of neighboring atoms during the rise of temperature from absolute zero to T, is supposed to be proportional to T.

L, the mean free path of a free electron between collisions, is supposed to be proportional[2] to Δ and so to T.

If c is the mean velocity of the free electrons, the mean time between collisions of an electron is proportional to $T \div c$.

Let n = the number of free electrons, or the number of ions, per unit volume of the metal.

τ, the mean life of an electron in the free state, being terminated only by collision with an ion, is proportional to $(T \div c) \div n$; that is,

$$\tau = q\,T \div n\,c, \tag{1}$$

where q is a constant.

Let P = the number of free electrons produced per unit volume per second. They are supposed to be produced by collisions,—usually by collisions of atoms with atoms, though sometimes by collisions of atoms with ions.

A little consideration shows that

$$P\,\tau = n, \text{ or } P = n \div \tau = n^2 c \div q\,T. \tag{2}$$

Let B = the total number of *ion* heat-paths begun per unit volume per second.

Let F = the total number of ion heat-paths *finished*, to collision with atom or ion, per unit volume per second.

$F < B$, because of collisions like the one here indicated (fig. 3), in which the ion captures a free electron and ceases to be an ion before it reaches the atom it is approaching.

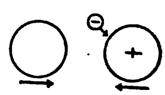


FIG. 3

Let θ = the mean duration of one heat-path of an atom or an ion. It is inversely proportional to the 'proper frequency' of atomic vibration and may therefore be taken as independent of the temperature.

If an ion could cease to be such only by collision with a free electron, the mean length of life of the ions would be the same as τ, the mean length of life of the free electrons. But an ion may cease to be such by collision with an atom, and the time required for its approach to such a collision is θ. Hence the probability[3] that an ion will survive as such till it has collision with an atom is $\epsilon^{-\theta/\tau}$. Accordingly we have

$$F = B\,\epsilon^{-\theta/\tau} \tag{3}$$

We have now to consider various types of collisions, between atoms, between atoms and ions, or between ions. Some of these may yield free electrons.

Since all the types of collision in which ions figure are here shown, we have

$$B = P + F \tag{4}$$

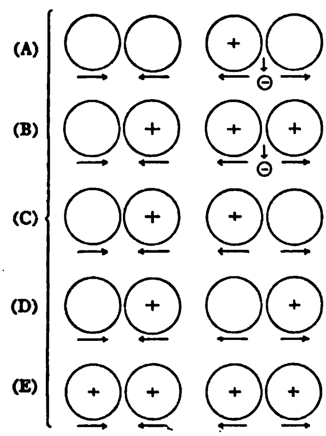
From equations (1), (2), (3) and (4) we get

$$F = \frac{n^2 c}{qT} \div (\epsilon^{(nc\theta + qT)} - 1). \tag{5}$$

As F is the number of ion heat-paths completed per unit volume per second, we assume that the conductivity is, other things being equal, proportional to F. Accordingly our question as to the dependence of conductivity on temperature becomes a question as to the dependence of F on T. This in turn, since n, c and T are the only variables in the value of F, evidently involves the question of the dependence of c and n upon T.

Types of collisions

Before After



Contribution to

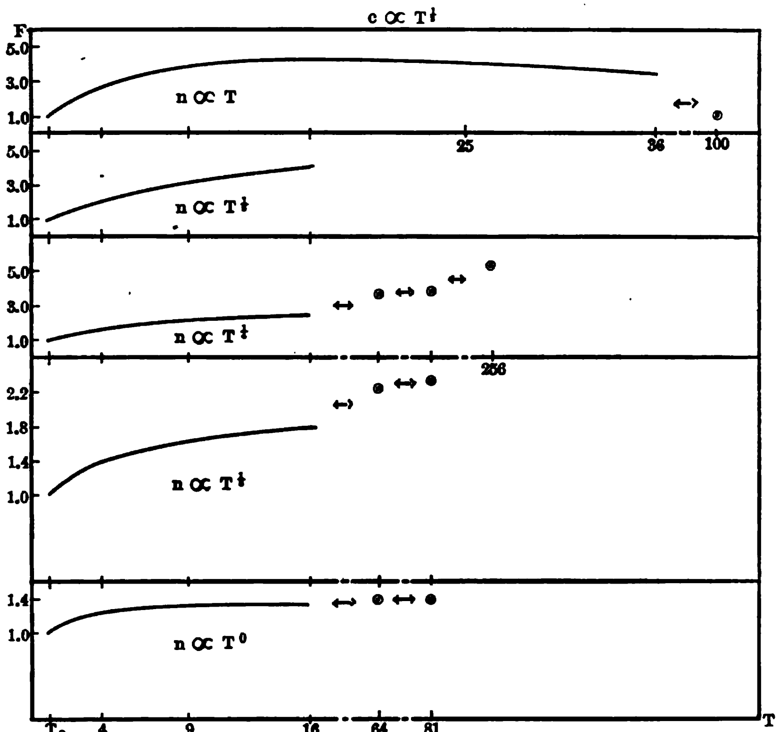
	B	P	F
(A)	1	1	0
(B)	2	1	1
(C)	1	0	1
(D)	1	0	1
(E)	2	0	2
	7	= 2	+ 5

FIG. 4

It is by no means certain that the mean kinetic energy of the free electrons is equal to that of gas molecules at the same temperature. Thus Jeans[4] says, "In this case owing to the small mass of the electron, the frequencies of the vibrations of the medium ['quasi-gas'] constituted by the electrons are very high, so that the old laws of partition of energy will not give anything even approaching a good approximation to the truth." In a paper published in 1914 I reached the conclusion[5] that, in a metal represented by a straight line on the thermo-electric diagram, the mean velocity of the free electrons may be proportional not to $T^{\frac{1}{2}}$, but to T, at least approximately.

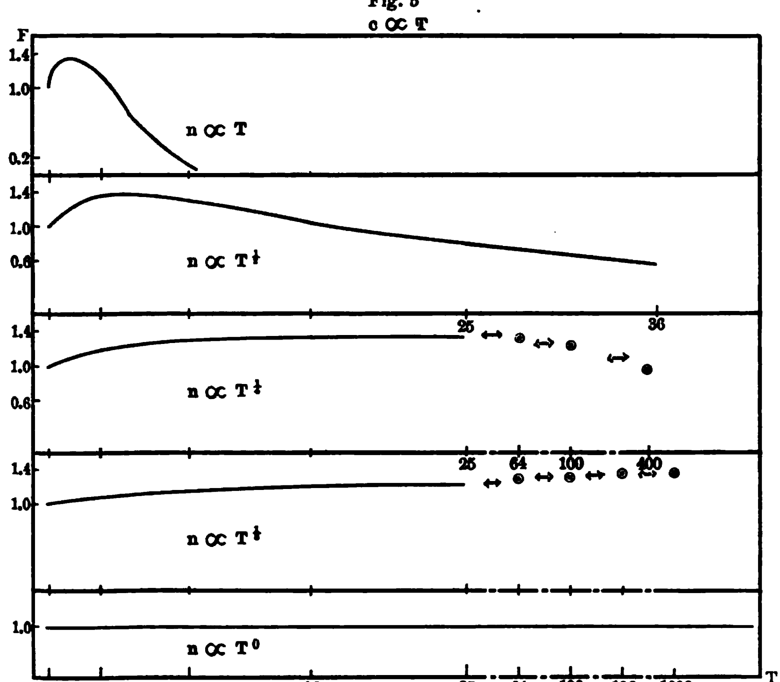
We shall here use, in turn, the two assumptions:

$$c = \gamma T^{\frac{1}{2}} \ (6) \quad \text{and} \quad c = \gamma T \ (6'), \quad \gamma \text{ being a constant.}$$



n ∝ T
n ∝ T⁰
F
T
T₀ 4 9 16 64 81
25 36 100
256

Fig. 5



c ∝ T
n ∝ T
n ∝ T⁰
F
T
T₀ 4 9 16 25 64 100 196 1000
25 36
25 64 100 400

Fig. 6

From (5) and (6) we get $F = an^2 \div T^{\frac{1}{2}} (\epsilon^{(bn + T^{\frac{1}{2}})} - 1)$, (7)

where $a = (\gamma \div q)$ and $b = (\gamma\theta \div q)$, both constant.

We next assume that the relation of n to T is of the form

$$n = kT^i \quad (8), \quad \text{where } k \text{ and } i \text{ are constants.}$$

From (7) and (8) comes $F = ak^2T^{(2i - 0.5)} \div (\epsilon^{bkT(i - 0.5)} - 1)$. (9)

where $b\ k\ T\ (i - 0.5)$, the exponent of ϵ, is merely another expression for the original $(\theta \div \tau)$.

The lowest temperature to be considered will be called T_0, and it will be defined as the temperatures at which $F = \frac{1}{2} B$, which means that only one-half of the ion heat-paths begun are completed by the ions as such.[5]

This makes $\epsilon^{\frac{\theta}{\tau}} = 2$, and so $F = ak^2\, T^{(2i - 0.5)}$. Higher temperatures will be expressed as multiples of T_0.

Giving to i the values 0, 0.125, 0.25, 0.50, 1.00 and 1.50, in turn, we get from (9) values of F which are indicated by the curves in figure 5.

If, instead of assuming $c \times T^{\frac{1}{2}}$, as in (6) we take $c \propto T$, as in (6′), we get, by making $i = 0$, 0.125, 0.25, 0.50, and 1.00, in turn, values of F which are indicated by the curves, in figure 6.

I have sought to account for the temperature relations of conductivity by combining with the considerations just presented the hypothesis of an atomic vibration consisting of a simple harmonic motion prematurely ended by collision of atom with atom or with an ion. This attempt, though not entirely successful, seemed not altogether hopeless until the experiments of Bridgman showed the atoms to be far less rigid than the kind of vibration in question required them to be.

[1] It should be, according to Conlomb's law, about 5×10^{-4} dyne, corresponding to a potential gradient of about 3×10^3 volts per centimeter.

[2] This is a doubtful proposition, and possibly some ingenious variation of it would make the ion theory more successful in dealing with the temperature relation.

[3] The question here is analogous to the inquiry what is the probability that a gas molecule will go a distance x without collision, the mean free path being l; θ corresponds to x and τ to l.

[4] Jeans, J. H., *Dynamical Theory of Gases*, 1916, §559.

[5] This does not imply a high degree of ionization. If we assume the electrons to have a mean kinetic energy equal to that of gas molecules at the same temperature, the mean velocity of the electrons will be about 350 times as great as that of the atoms. Hence, if the mean free path of an electron is equal to the mean heat path of an atom or ion, a single electron will have about 350 collisions during the time θ. If g be the degree of ionization,—that is, the ratio of n to the number of atoms per unit volume,—the total number of collisions per second of electrons with ions will be $(350 \div \theta)\ n\ g$. This is the number of ion heat-paths interrupted per second per unit volume by the free electrons. The total number

begun per second is, in the case supposed, $B = P + F = n \div \tau + \frac{1}{2} B$, and so $\frac{1}{2} B = n \div \tau$. Hence $(350 \div \theta)\, n\, g = n + \tau$, or $g = (\theta \div 350\tau)$. But when $F = \frac{1}{2} B$, $(\theta \div \tau) = \log_e 2$, and so $g = 0.00\,2$ nearly.

[6] Hall, E. H., *Boston, Proc. Amer. Acad. Arts and Sci.*, 50, 1914, (67–103).

THE GRAVIMETRIC SURVEY OF THE UNITED STATES

By William Bowie

DIVISION OF GEODESY, U. S. COAST AND GEODETIC SURVEY

Communicated by W. M. Davis, January 12, 1917

The gravimetric survey of the United States really began in 1890 with the introduction of the Mendenhall one-half second invariable pendulum. Previous to that date, 13 stations had been established but in that older work pendulums were used which gave inaccurate results, as later work showed. We shall not, therefore, consider the gravity results obtained before the use of the Mendenhall pendulum.

This pendulum consists of a bob and stem with a suitable head into which is set an agate plane which rests on a knife edge of the same material fastened to the pendulum case. The various parts of the apparatus are illustrated and described in reports of the Coast and Geodetic Survey.[1] The Mendenhall pendulums are used to determine the difference in the intensity of gravity at two stations.

The probable error of the value of gravity at Washington, determined from Potsdam by the relative method, is ± 0.001 dyne. The probable error of a station in the United States, other than that at Washington, is about ± 0.002 dyne. This is about one part in one-half million. The error of the absolute value at Potsdam enters all other values of gravity based upon the Potsdam system.

Between the years 1891 and 1907, 47 stations were established in the United States, while since January, 1909, 212 additional stations have been established, making 259 in all. The Coast and Goedetic Survey has planned to continue its gravimetric survey for an indefinite period, with a view to covering the large areas now lacking in stations, and also local areas where there are special problems to be investigated.

There are two immediate purposes to be served in carrying on this work. First, to collect data from which more accurate values may be obtained for the flattening of the earth and for the terms in the gravity formula. Second, to obtain values of the intensity of gravity at laboratories as these values are needed in certain physical and chemical work.

Another important use to which the gravimetric survey may be put is in researches into the subject of isostasy. While an old subject[2] isostasy has only recently become a vital matter to be considered in

most geophysical problems. The work done in the Survey[3,4,5] shows that, at some depth below sea level (of the order of 100 kilometers) the pressure of any unit column is very nearly equal to that of any other unit column. For instance, the pressure exerted on a square mile of the imaginary surface at a depth of say 96 kilometers[6] below sea level at the sea coast or under the plains is about the same as the pressure on an equal area at the same depth below sea level under the Rocky Mountains or under any other mountain masses.

If we assume that the equalization of pressures at the supposed depth, called the depth of compensation, is perfect, then we must conclude that the land masses are counterbalanced by deficiencies in density of the materials below sea level, under the topographic features. It is not probable that the pressures are exactly equal for small areas of the surface at the depth of compensation. It is very probable that they are practically equal for areas of the order of ten thousand square miles. It is one of the important problems of the geodesist to collect sufficient data to show the minimum area which may be in a high state of isostasy. Another problem for him to investigate is the distribution horizontally and vertically of the deficiencies in mass, which balance the material which is above sea level.

For the purposes of making the computations, the compensating deficiencies of density or mass are supposed to be uniformly distributed directly under the topographic feature from the earth's surface to the depth of compensation. And it is also assumed that the negative masses exactly equal the positive masses which are above sea level.

We cannot tell from the data now at hand just how near the truth are these assumed conditions. But we do know that they are very much nearer the truth than those conditions based upon a rigid earth, with the topographic features held up by the strength of the earth's outer material. The assumed isostatic conditions are also shown to be much nearer the truth than those based upon the theory that the topographic features should be ignored.

It will be interesting to consider briefly the results of the latest investigations by the Coast and Geodetic Survey upon the subject of gravity and isostasy.[5]

We may assume that the method of reduction which shows the smallest effect of systematic or constant errors is the nearest the truth. For instance, there may be used in the tests five classes of topography (the sea being ignored, as we have no very accurate gravity observations at sea). They are indicated in the tables below. The isostatic or Hayford method of reduction was used with two depths of compensa-

tion and with two different gravity formulas; otherwise they are identical. The Bouguer reduction postulates a highly rigid earth and the free air reduction an earth with no rigidity.

RELATION BETWEEN THE GRAVITY ANOMALIES AND THE TOPOGRAPHY

Mean anomalies

With regard to sign

	NUMBER OF STATIONS	MEAN ANOMALY			
		Hayford, 1912; depth 113.7 km.	Hayford, 1916; depth 60 km.	Bouguer	In free air
Coast stations	27	−0.009	−0.003	+0.017	+0.017
Stations near coast	46	−0.001	+0.002	+0.004	+0.017
Stations in interior, not in mountainous regions	88	−0.001	−0.001	−0.028	+0.009
Stations in mountainous regions					
Below the general level	36	−0.003	0.000	−0.107	−0.008
Above the general level	20	+0.001	+0.016	−0.110	+0.058
All stations (except the two Seattle stations)	217	−0.002	+0.001	−0.036	+0.013
Without regard to sign					
Coast stations	27	0.018	0.012	0.021	0.022
Stations near coast	46	0.021	0.020	0.025	0.023
Stations in interior, not in mountainous regions	88	0.019	0.019	0.033	0.020
Stations in mountainous regions					
Below the general level	36	0.020	0.018	0.108	0.024
Above the general level	20	0.017	0.022	0.111	0.059
All stations (except the two Seattle stations)	217	0.019	0.019	0.049	0.025

The table above shows that when the reductions are made by either of the Hayford methods the range of the mean anomalies with regard to sign is very nearly zero in most cases. The Bouguer and free air anomalies are much larger than the isostatic anomalies. By anomaly is meant the difference between the observed and the computed values of gravity at a station.

The table indicates strongly that the conditions under which the isostatic or Hayford reductions were made are very close to the truth. The evidence is that the depth 113.7 kilometers is closer to the truth than 60 kilometers, for in the former case the mean anomaly for the classes of topography indicated above varies from +0.001 to −0.009 dyne, while with the latter the range is from +0.016 to −0.003. The

Bouguer range is from $+0.017$ to -0.110 dyne. The free air means vary from $+0.058$ to -0.008.

It is important to consider whether the compensation occurs directly under a topographic feature, a mountain mass for instance, or is distributed through a column having a cross section somewhat greater in area than the base of the feature.

The compensation was distributed in horizontal extent to distances of 19, 59 and 167 kilometers in all directions from the stations. These distances correspond to the outer limits of zones K, M and O, which were used in computing the topographic and compensation corrections. It was found that the mean anomaly with and without regard to sign was approximately the same for each method of distribution horizontally and for local distribution, if all stations were treated as a single group. Consequently, no one method seemed to be more probable than any other. The stations were next considered in five groups, according to the topography, with the results shown in the following table.

RELATION OF LOCAL-COMPENSATION ANOMALIES AND REGIONAL-COMPENSATION ANOMALIES TO TOPOGRAPHY

	ANOMALY LOCAL COMPENSATION	ANOMALY. REGIONAL-COMPENSATION WITHIN OUTER LIMIT OF		
		Zone K 19 km.	Zone M 59 km.	Zone O 167 km.
At 18 Coast Stations				
Mean with regard to sign	-0.004	-0.004	-0.004	-0.006
Mean without regard to sign	0.018	0.018	0.018	0.020
At 25 stations near coast				
Mean with regard to sign	-0.002	-0.001	-0.001	-0.001
Mean without regard to sign	0.022	0.021	0.021	0.022
At 39 interior stations, not in mountains				
Mean with regard to sign	$+0.001$	$+0.002$	$+0.002$	$+0.003$
Mean without regard to sign	0.017	0.018	0.018	0.017
At 22 mountain stations below general level				
Mean with regard to sign	0.000	$+0.001$	$+0.003$	$+0.006$
Mean without regard to sign	0.017	0.017	0.018	0.019
At 18 mountain stations above general level				
Mean with regard to sign	$+0.003$	$+0.003$	0.000	-0.010
Mean without regard to sign	0.018	0.018	0.017	0.020

The mean anomaly without regard to sign is not decidedly in favor of either method. But the means with regard to sign seem to point strongly against the distribution of the compensation out to a distance of 167 kilometers from the stations, for there we have a range in the mean anomalies of 0.016 dyne between the 22 stations in mountainous

regions below the general level and the 18 mountain stations above the general level. The range for local and the other two methods is, in each case, only 0.007 dyne.

We must conclude that the regional distribution out to 59 kilometers is as likely to be true as the local distribution of the compensation. There may be some distance beyond 59 kilometers and less than 167 kilometers which would show a smaller range in the mean anomalies than any of the distributions considered above. It seems to be reasonable to expect that the distance in question is nearer 59 than 167 kilometers.

Tests made to show which is the most probable depth of compensation indicated that when the stations were taken as a single group the value is about 60 kilometers. But when the stations were arranged according to the topography the depth of 95 kilometers is the most probable depth. The depths of compensation, determined from deflections of the vertical,[3] are from the first investigation 113.7 kilometers and from the later one, 122 kilometers. When the deflection stations in mountain regions only were considered the depth is 97 kilometers. The mean of this and the depth from gravity stations is 96 kilometers. It is believed this is the best value from all geodetic data.

It is improbable that the compensation is distributed uniformly from sea level to the depth of compensation. It is not probable that what may be considered the normal distribution of densities of the material in the outer portions of the earth obtains under all places on the earth's surface.

The present data are not sufficient to enable one to compute the actual distribution of densities under any given area, but there are reasons to believe that approximations to the actual distribution may be made.

In a study of the relation between the gravity anomalies and the geological formations, as indicated by surface material, it developed that at stations on the pre-Cambrian areas gravity is nearly always in excess. It is known that the older rocks have greater densities than normal and as this material is close to the gravity stations there should result a greater intensity of gravity. The excess of gravity at a station may give some idea of the depth of this older rock.

The stations on the Cenozoic formations have a tendency to a deficiency in gravity. This seems to be logical as the rocks in this formation have densities less than normal. Here again the size of the deficiency may indicate the approximate depth of the Cenozoic formation. The Paleozoic stations tend to be negative and the Mesozoic stations

positive, but there is no such evident relation between the densities and the gravity as with the two other formations. The average densities of the Paleozoic and Mesozoic rocks are about equal.[7]

There is given below a table which shows that relations exist between the gravity anomalies and the geological formations in the United States. A positive anomaly indicates an excess in gravity and a negative anomaly a deficiency.

RELATION BETWEEN THE GRAVITY ANOMALIES AND THE GEOLOGIC FORMATION

GEOLOGIC FORMATION	NUMBER OF STATIONS			MEAN ANOMALY	
	With plus anomalies	With minus anomalies	All	With regard to sign	Without regard to sign
Pre-Cambrian	12	2	14	+0.019	0.023
Paleozoic	23	49	72	−0.011	0.021
Mesozoic	25	11	36	+0.009	0.017
Cenozoic	22	32	55	−0.007	0.019

A study was made of the stations in India, 73 in all, and these showed that gravity at Cenozoic stations is, in general, too small. There were very few stations in the other formations which made it impossible to draw any definite conclusions from them.

RELATION BETWEEN THE GRAVITY ANOMALIES AND THE GEOLOGIC FORMATION FOR STATIONS IN INDIA

GEOLOGIC FORMATION	NUMBER OF STATIONS			MEAN ANOMALY	
	With plus anomalies	With minus anomalies	All	With regard to sign	Without regard to sign
Pre-Cambrian	6	2	8	+0.002	0.025
Paleozoic	2	3	6*	0.000	0.009
Mesozoic	1	0	1	+0.022	0.022
Cenozoic	11	20	31	−0.017	0.028

* One anomaly is zero.

The larger part of most of the larger anomalies may de due to deficiencies or excesses in the densities of the materials below sea level near the stations, and these deviations from normal may be compensated in the lower strata. If this is true, isostasy is more nearly perfect than has generally been supposed.

There are 42 stations in Canada which were studied but their gravity values showed no such relations to the geological formations as were found in the United States and in India.

From the values of gravity at 358 stations in the United States, Can-

ada, India and in Europe, which had been reduced by the isostatic or Hayford method, the best gravity formula deduced is

$$\gamma_0 = 978.039\ (1 + 0.005294 \sin^2 \phi - 0.000007 \sin^2 2\ \phi)$$

in which γ_0 is the value of gravity in dynes at sea level, at the latitude, ϕ, and the first term, 978.039, is the value of gravity at the equator.

From the constant 0.005294 a reciprocal of the flattening of the earth of 297.4 was derived.

The well known formula

$$C_h = -0.0003086\ H$$

was used to correct the value of gravity for the distance above sea level. H is the elevation of the station in meters.

If we assume that the best known value of the equatorial radius of the earth is 6,378,388 meters,[8] then the polar semi-diameter is 6,356,941 meters. The difference is 21,447 meters or 13.3 miles.

The results of the investigations of the Coast and Geodetic Survey make it possible to compute the value of gravity for stations in the United States, and possibly also in any other country, with an average uncertainty in the result of about 0.020 dyne or one part in 50,000.

Further work on the gravimetric survey of the United States will enable us to obtain better values of the shape of the earth, and for the constants of the gravity formula, and will no doubt lead to important discoveries regarding the distributions of densities in the outer portions of the earth and especially within the outer ten miles.

[1] *Washington, D. C., U. S. Commerce Dept. Coast & Geod. Surv., Rep.*, 1891, Appendix No. 15; *Ibid.*, 1893, Appendix No. 12; *Ibid.*, 1910, Appendix No. 6.

[2] *London, Phil. Trans. R. Soc.*, 149, 1859, (745); Dutton, C. E., *Washington, D. C., Bull. Wash. Phil. Soc.*, 11, 1889, (51–64).

[3] Hayford, J. F., *U. S. Commerce Dept. Coast & Geod. Surv.*, The figure of the earth and isostasy from measurements in the United States, 1909; Supplementary investigation in 1909 of the figure of the earth and isostasy, 1909.

[4] Hayford, J. F., and Bowie, W., *U. S. Commerce Dept. Coast & Geod. Surv., Sp. Pub.*, No. 10, 1912; Bowie, W., *Ibid.*, No. 12, 1912.

[5] Bowie, W., *Ibid.*, No. 40, 1916.

[6] This, according to the latest investigation, is the most probable depth (see [5]).

[7] Barrell, J., *Chicago, Ill., J. Geol. Univ. Chic.*, 22, 1914, (215).

[8] See Hayford, loc. cit., Note 3, 1909, (54).

THE MAGNETIZATION OF IRON, NICKEL, AND COBALT BY ROTATION AND THE NATURE OF THE MAGNETIC MOLECULE

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Communicated by R. A. Millikan, January 15, 1917

In December, 1914, I described to the American Physical Society an extended series of experiments made in that year on the magnetization of large steel rods by mere rotation.[1]

Before these experiments were made only one method of magnetizing a body was known, viz., placing it in a magnetic field. These experiments not only revealed another and entirely new method, but they also confirmed completely the fundamental assumptions on which the results had been predicted: They proved, in a direct and conclusive way, on the basis of classical dynamics alone, without dependence upon the theory of radiation, (1) that Ampèreian currents, or molecular currents of electricity in orbital revolution, exist in iron; (2) that all or most of the electricity in orbital revolution is negative; and (3) that it has mass or inertia, so that each orbit behaves like a minute gyrostat and tends to set itself with the direction of revolution coincident with the direction of rotation of the body. It is in this way that magnetization of the body results. Furthermore, if we admit the classical theory of radiation, these experiments, together with the existence of residual or permanent magnetization, prove (4) that the arrangement of the electricity in the Ampèreian orbits is Saturnian rather than planetary.

If it is assumed that only one kind of electricity is in orbital revolution, and if the mass of a particle is denoted by m and its charge by e, theory shows that the rotation of a body with angular velocity n revolutions per second is equivalent to putting it in a magnetic field of intensity H, such that

$$H/n = 4\pi m/e. \tag{1}$$

If we assume that electrons alone are in orbital revolution, the value of the second member of this equation is -7.1×10^{-7} e. m. u. according to well known experiments on electrons in slow motion, and H/n should be equal to this quantity and identical for all substances. If positive electricity also participates, the magnitude of H/n should be smaller. The value of H/n in my 1914 experiments was -3.6×10^{-7} e.m.u.

A little later, in February and April, 1915, Einstein and de Haas[2] described to the German Physical Society successful experiments on

the effect converse to mine, viz., rotation by magnetization, which had been predicted and looked for by O. W. Richardson in 1907; and de Haas[3] has recently continued this work in a somewhat different manner. Both investigations are indirect but excellent confirmations of my own earlier work. This work has also been confirmed by further experiments of my own of somewhat increased precision described before the American Physical Society in April, 1915.[1]

In the last year, with financial aid received from the University through the interest of the dean of the graduate school, Prof. W. McPherson, and with the help of Mrs. Barnett, I have extended the investigation to other specimens of iron and to cobalt and nickel. In all the earlier work the method of electromagnetic induction was used; this later work has been done by an entirely different method, viz., that of the magnetometer.

The magnetometer was an astatic instrument, and each rod under experiment, or rotor, about 30.5 cm. in length and from 2.3 cm. to 3.2 cm. in diameter, was mounted with its axis horizontal and normal to the magnetic meridian in the equatorial position of Gauss, which offered important advantages for this work. Calibrations were made by means of solenoids wound permanently on the rotors, and subsidiary solenoids wound on wooden cores.

To avoid magnetic and mechanical disturbances as much as possible, nearly all of the observations were made after one o'clock at night. This precaution, together with the use of true, carefully adjusted, and frequently oiled bearings, heavy mountings of bronze and concrete, and a special method of driving, eliminated mechanical disturbances very largely.

Disturbances due to variations of the earth's intensity were greatly reduced by mounting a compensating rod of the same substance and nearly the same size as the rotor in approximately the same position with respect to the upper magnetometer magnet as that occupied by the rotor with respect to the lower magnet.

Possible errors due to eddy currents in the rotor and to minute shifts of the rotor's axis in altitude or azimuth were avoided by compensating accurately the earth's intensity in the region occupied by the rotor by means of a very large coil traversed by a steady electric current. Much greater variations of the compensating current on both sides of the correct value than the maximum allowed in the rotation experiments produced no appreciable effect on the results obtained with some of the rotors and not more than small effects with the others. The magnetometer magnets, control magnet, compensating rod, and a small electric coil in series with

the large coil and mounted near the upper magnetometer magnet to make the sensibility approximately independent of the compensating current, produced in the region occupied by the rotor intensities so small that their effect was negligible.

Rotation observations were made at equal intervals in sets of four as follows: The rotor was first driven (by means of an alternating current motor) at given speed in one direction and the magnetometer scale read; then the motor was reversed and the scale again read; then the readings were repeated in inverse order, all for the same speed. The double deflection obtained by subtracting the mean of the second and third readings from the mean of the first and fourth was the quantity sought. This process eliminated the difficulties due to the presence of residual magnetization of the rotor, the error due to magnetometer drift, and other possible errors. Error due to torsion of the rotor was found to be negligible by reversing some of the rotors in their bearings.

With nickel and cobalt observations were made at three speeds, and H/n was found to be independent of the speed within the limits of the experimental error, as had been found in the earlier experiments with iron.

Since mechanical disturbances were almost wholly absent and the magnetic disturbances became relatively less important with increased speed, the observations at the highest of the three speeds were more precise than the others. Table 1 contains the approximate results of the observations on four rods at the highest speeds. A few observations, consistent with the others, on a fifth rod, of soft iron, in poor condition are not included in the table.

TABLE 1.

ROTOR	MEAN SPEED	NO. OF SETS	$10^7 \times \frac{H}{n}$	AVERAGE DEPARTURE FROM MEAN
Steel (larger)	47 r. p. s.	21	5.2 e. m. u.	1.2
Steel (smaller)	44	21	5.2	0.5
Cobalt	45	79	6.1	0.8
Nickel	45	37	6.1	0.9

The mean value of H/n is in all cases less in magnitude than the standard value of $4\pi m/e = -7.1 \times 10^{-7}$ e.m.u. for electrons in slow motion, as was the case in the earlier experiments; but the experimental errors, on account of the great difficulties involved, are such that importance cannot in my opinion be attached to the discrepancy. The investigation must rather be taken as confirming equation (1) and the assumption that only electrons are in orbital revolution in all the substances investigated.

A more extended account of the investigation will be published in the near future.

[1] Barnett, S. J., *Physic. Rev., Ithaca, N. Y.*, (Ser. 2), 6, 1915, (239–270).

[2] Einstein, A., and de Haas, W. J., *Berlin, Verh. D. Physik. Ges.*, 17, 1915, (152–170, 203, 420).

[3] de Haas, W. J., *Amsterdam, Proc. Sci. K. Akad. Wet.*, 18, 1916, (1281–1299); *Sci. Abs., London*, A., 17, 1916, (351).

THE INTENSITIES OF X-RAYS OF THE L SERIES

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Communicated by E. H. Hall, January 17, 1917

The purpose of this paper is to report briefly some preliminary results of a study of the intensities of X-rays belonging to the L series of platinum considered as functions of the potential producing them and in their relations to each other and to the general radiation.

Review of Previous Work.—Many of the phenomena observed here can be predicted, though with no certainty, by analogy with corresponding phenomena of the K series. The similarity of the two series appears in Moseley's laws[1] of frequency as a function of atomic number, and especially in the fact that each series is produced as fluorescence by a substance absorbing rays of a higher frequency. It has been found by one of us[2] that the K series rays, of rhodium at least, appear only at a potential high enough to produce general radiation of a frequency as great as that of the shortest line of the series. This may be called the critical potential. Since this result is obviously connected with the law that absorbed rays will produce the K fluorescence only if their frequency is above that of this line, it is reasonable to expect a similar law for the L series.

It must be remembered, however, that the L series is more complex than the K, both in the number of lines and in their gradual shifting relative to each other from element to element, shown in Moseley's graphs[1] of square root of frequency against atomic number of the emitting element. Moreover Kossel[3] has found reason to believe that platinum and gold each show two discontinuities of absorptive power as a function of frequency near the L series, one in the middle of the series and the other near the high frequency end. An explanation of this appears in a most exhaustive study of the positions of the L series lines of the heavy elements by Siegbahn and Friman[4] who have plotted $\sqrt{\nu}$ against N for twelve lines. Four of these graphs are linear and nearly parallel, while the eight others are not linear but are nearly

parallel with a greater slope than the four. This suggests that the four, α_2, α_1, β_2, and β_5, (see fig. 1) form one series, which we shall call L_1, while the others, η, β_4, β_1, β_3, γ_1, γ_2, γ_3, and γ_4, form another series, L_2. The L_1 series is remarkably similar in appearance to the K series, unless we include in it the line *l*, reported by Siegbahn in a paper which is not yet accessible, but which is quoted by Friman.[5] From this similarity one would expect the L_1 series to have a critical potential equal to the quantum potential of its highest frequency member, β_5. The L_2

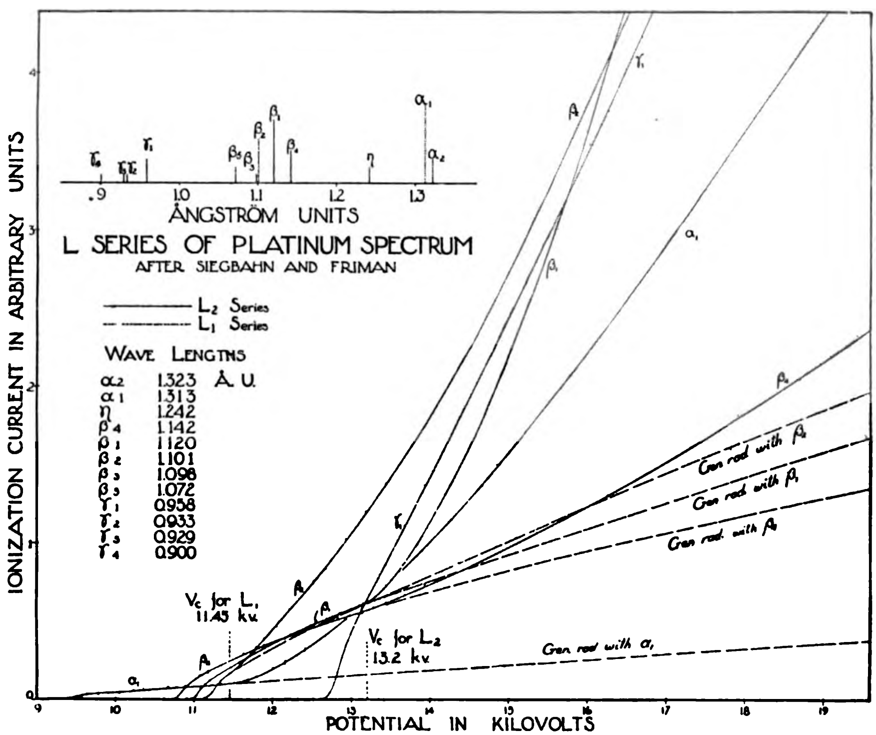

FIG. 1.

series, on the other hand, is totally different in appearance so that similar predictions can scarcely be made. The existence of two series accounts readily for that of two discontinuities of absorption.

Following farther the analogy with the K series, one might expect all lines of each of the L_1 and L_2 series to increase in intensity by the same ratio with any given increase of potential,[6] and we may expect their intensity-potential graphs to be concave upward, while that of general radiation of any one frequency starts concave downward and soon becomes linear.[7]

Results of Our Experiments.—We have partially tested these predictions for a platinum target in a Coolidge tube furnished through the kindness of Dr. Coolidge. The potential was obtained from Professor Trowbridge's 40 kv storage battery, and measured by a Chaffee electrostatic voltmeter calibrated by a manganin resistance of 0.8940 megohms. The spectrometer was similar in general plan to that used in the previous work on rhodium, but with many improvements in construction and accuracy.

The principal results of the work to date are shown in figure 1, which contains graphs of intensity as a function of potential for two lines, α_1 and β_2, of the L_1 series and three lines, β_4, β_1, and γ_1, of the L_2 series. In each case the graph leaves the axis at the potential required to give an electron an energy quantum of the proper frequency, and each graph is of the form of one for general radiation only, from the start to a definite potential at which its curvature suddenly changes. This change denotes the first appearance of the spectrum line. For both the L_1 lines, this change occurs at 11.45 kv., or the quantum potential of the β_5 line, as predicted, within the limits of error caused by the uncertainty in h. For β_4 and β_1 also the critical points are identical, but quite different from that of the L_1 lines. In none of these four cases can there be any doubt as to the existence of critical points, although the intensities are somewhat inaccurate, since even β_1 is so weak there that a fall of the electroscope over the shortest scale division requires about half a minute.

In the interpretation of the γ_1 curve, difficulty arises from the fact that the critical point is so close to the quantum potential that the general radiation itself has not acquired a uniform rate of increase. This curve has therefore been repeated in figure 2, with curves for general radiation of wave lengths just above and below γ_1. From these the general radiation with γ_1, itself may be estimated. It is represented by the dotted line, which crosses each of the other curves at about the proper distances and would lie midway between them at very high potentials. From these graphs it appears that γ_1 also has a critical potential, identical with that of β_1 and β_4, which appears to be 13.20 ± 0.05 kv. The wave length corresponding to this potential is 0.937Å, not that of γ_4, the predicted head of the series, but even longer than γ_2. This result, which was quite unexpected, raises a question as to the nature and behavior of the three very faint lines γ_2, γ_3, and γ_4, on which we hope to obtain more evidence in the course of this work.

The calculations of the critical wave lengths of the series is dependent

on the value assumed for Planck's *h*. Millikan's experiments on photo-electrons give 6.57×10^{-27} erg sec., while the value obtained in the work on rhodium was 6.53. The latter value was obtained from the potential at the middle of the sharp upward curvature at the foot of each graph and is subject to any errors that may occur in determining this potential. This is made uncertain by the width of the slit and consequent lack of homogeneity of the rays. If, for example, the graph for really homogeneous rays turns down very sharply to the axis,

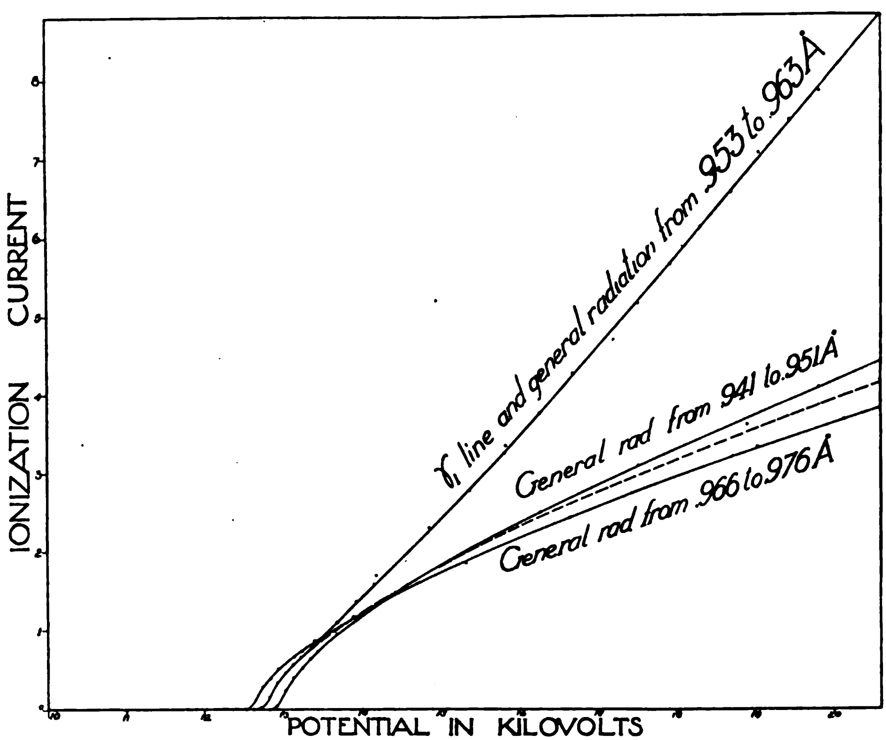

FIG. 2.

and the boundaries of the rays used are made indefinite by the size of the source, the potential to be used should be that of the steepest part of this curve. This hypothesis seems highly improbable since the maximum angle between any two rays coming through the slit is under 0.10 in this case, giving less than ½% deviation of any reflected wave length from the mean, and the steepest part of the curve occurs about 1% from the lowest potential at which any rays can be detected. Consequently we shall follow the method of calculation previously used. The values of *h* obtained are given in table I. Siegbahn and Friman's

wave lengths are used as standards. Whatever constant errors there may be in this determination of h will cancel out in the calculation of the critical wave lengths.

TABLE I

Line	λ	h (erg sec)
α_1	1.313Å	6.59×10^{-27}
β_4	1.142	6.53
β_1	1.120	6.54
β_2	1.101	6.53
γ_1	0.958	6.48
Mean		6.53

The intensities of the lines alone may be obtained by subtracting from the ordinates of their graphs the general radiation estimated as in figure 1. By this process it appears, as expected, that lines of the same series maintain the same ratio of intensities through any increase of potential up to 20 kv., the limit of our experiments to date. For potentials over 20% above the critical points, the mean deviation of these ratios from their average values is less than 3%. More accurate results should be obtained at higher potentials and with more exact determinations of the general radiation. The graphs both of lines and general radiation are also of the general forms predicted. Present results indicate that the rhodium K series and platinum L_1 and L_2 series all increase in intensity approximately in proportion to the three-halves power of the difference between the potential applied to the tube and the critical potential of the series. On all these points, however, the present statements are only preliminary, and more accurate results should be obtained from further work now being done on platinum. Since the relation between the two series is different in different elements, we hope to test these phenomena in thorium also.

[1] *Phil. Mag., London,* (Ser. 6), 26, 1913, (1024–34); 27, 1914, (703–14).

[2] These PROCEEDINGS, 2, 1916, (90–4); *Physic. Rev., Ithaca, N. Y.,* (Ser. 2), 7, 1916, (599–613).

[3] *Berlin, Verh. D. Physik. Ges.,* 16, 1914, (898–909).

[4] *Phil. Mag., London,* (Ser. 6), 32, 1916, (39–50).

[5] *Ibid.,* 32, 1916, (497–499).

[6] *Ibid.,* 26, 1913, (210–232), see especially p. 225.

[7] The instrument was made from our design by the Alvan Clark Corp., Cambridge, Mass.

THE USE OF VASECTOMIZED MALE MICE AS INDICATORS

By C. C. Little

HARVARD MEDICAL SCHOOL

Communicated by R. Pearl, January 22, 1917

In the course of certain experiments with mice it became necessary to have some way of determining the presence of the ovulation period in females which were still virgins. The small size of the external genetalia and the minuteness of the changes in them at periods of ovulation make methods of direct observation difficult if not impossible.

It was found convenient therefore, to use vasectomized males to indicate the physiological condition of the females. For this purpose, males sexually mature or nearly so, were etherized and an incision about 6 mm. long was made, a little to the right of the mid-ventral line in the right inguinal region, the hair having been previously clipped in that locality. Care was taken to avoid the larger blood vessels in the peritoneum. Through this opening it was possible with a pair of small serrated forceps, to lift out first the right and then the left testicle and their respective vasa defferentia. A piece of the vasa defferentia, about 3 mm. in length, was tied off with fine black silk, and the section between the ligatures removed. The testicles and vasa defferentia were then replaced in the peritoneal cavity and the body wall and the outer skin sewed separately. Warm salt solution was used when necessary to keep the exposed organs moist.

In a great majority of cases the operation was entirely successful and the mice recovered rapidly. In two weeks these mice were placed with females known to have had young between twenty-four and thirty-six hours previous. The vasectomized males behaved like normal males as regards their sexual instincts, and attempted successfully to copulate with the females. In so far as it was possible to observe, there was no difference in the process of mating between vasectomized and normal males.

A breeding test on a larger scale was made to determine whether in a considerable period of time the vasectomized males would be able to fertilize an occasional female. For this purpose virgin females, just becoming sexually mature, were placed with vasectomized males and were controlled by an equal number of similar females placed with normal males. The first twenty males, both vasectomized and normal, were allowed to remain with females for only ten days. During this time in a total of 106 females, the pens containing vasectomized males

had a record of no observed pregnancies, while the controls showed seven pregnancies in a total of 98 females. The second series of results showed a more definite difference. In eleven pens in which vasectomized males were allowed to remain for from eighteen to twenty days, with a total of 60 females, there were no pregnancies; while control males remaining in pens containing 55 females for from eighteen to twenty days gave a total of nineteen pregnancies. The chief reason for lengthening the time during which the females and males were together was the appearance of a communication by Long concerning the period of ovulation in mice, which states that the average length of time between the first and second ovulations following parturition in mice is from seventeen to eighteen days. It became evident therefore, that the first series of experiments in which female mice were allowed to remain with the males only ten days, would not be critical, in as much as the space between ovulation periods in virgin mice is undoubtedly approximately the same as the length of time between the first and second ovulations following parturition; and therefore probably only about one-half of the females would pass through an ovulation period while with the males. The longer period gives a far better test as the results show.

Before the experiments here recorded were made, a number of males were operated on and in each case a portion of the vasa defferentia 3 mm. long was removed. The cut ends, however, were not ligatured. Thirty-nine of these males were chloroformed, slightly over two months after operation, and were examined to see whether any possible connection between the severed vasa defferentia had been reestablished. In no case was there any sign of such a connection. In many cases there was obvious blockage and distention of the vasa defferentia, apparently by retention of substances which would have ordinarily passed through them. Certain signs of inflammation and reduction in the size of the testicles were also frequently observed. It would be interesting to see whether the sexual instincts of vasectomized males remained normal after the degenerative changes of the testes and vasa defferentia referred to had set in.

By supplementing observations on the sexual instincts of these males with a careful histological examination of the testes it might be possible to obtain an additional line of evidence as to whether it was primarily the interstitial or the sperm forming cells which influence the secondary sexual characters.

By the use of vasectomized males it will be possible to pick out the females which are in a suitable physiological condition for successful

insemination by artificial or natural means, thereby minimizing the amount of error introduced in experimental work by the use of females in unknown physiological condition. In critical experiments by using for an indicator a vasectomized male, homozygous for one color factor, and for the later insemination sperm from a male homozygous for another color factor, one will have a definite breeding test showing whether or not, by any accident, the vasectomized male was able to transfer sperm to the female.

PHOTOGRAPHIC MAGNITUDES OF STARS IN THE SELECTED AREAS OF KAPTEYN

By Frederick H. Seares

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Communicated by G. E. Hale, February 1, 1917

A casual experience with the details of astronomical investigation reveals, in the number of the stars, a serious difficulty to be overcome in undertaking any discussion of the development and structure of the stellar universe. Telescopes of even moderate size bring before the observer stars to be counted by tens of millions, while those shown by instruments of the highest power are many times more numerous. Since the individual examination of all these objects will not be seriously considered, the question arises as to a rational limitation of the program of observations. Fortunately, the problem is not as hopeless as it seems, for very important and illuminating facts are to be derived from a minute percentage of the total number of stars seen in our telescopes, provided only that the objects chosen for study be representative of the collection as a whole.

Kapteyn, in 1906, showed that by proper restriction and selection we might hope to obtain, within a comparatively few years, a fairly comprehensive notion of the salient features of the structure of the universe. His well-known 'Plan of Selected Areas,' published in that year,[1] formulates in a definite way the investigations to be undertaken in order that we may acquire an adequate knowledge of stellar positions, distances, proper motions, radial velocities, spectra, and magnitudes, both visual and photographic, which are the data essential for a consideration of the questions of structure and development.

The principle of selection adopted by him involves the detailed examination of all objects within the reach of observation, situated in certain small areas uniformly distributed over the sky. About two hundred such selected areas are included in the list; the center in each case

is definitely marked by a star of more than average brilliance, but the boundaries are elastic and may be adapted to the special requirements of any given problem. In order that the accidental irregularities of distribution may not too greatly influence the results, each region, in dealing with stars of moderate brightness, will cover one or two square degrees; but for the very numerous fainter objects, sufficiently comprehensive data can be obtained from much smaller areas, including, in some cases, only a small fraction of a degree.

Kapteyn thus avoids the task impossible of execution and sets one that is more appropriate to our limited capacities. We occupy ourselves with what in effect is a sampling of the contents of the heavens, and, because of that underlying regularity presupposed by all our science, accept with some confidence its inferences and deductions as to the totality of the universe of stars.

But with even this much restricted plan, the actual labor still is very great, beyond the resources of any single institution, and subdivision and coöperation have therefore been necessary. Various observatories and numerous individuals have devoted themselves to special programs of observation, with the result that data are now rapidly accumulating.

In accordance with this plan several investigations, for which the equipment of the Solar Observatory is peculiarly adapted, have been undertaken at Mount Wilson. One of these, with which this note is particularly concerned, relates to the determination of photographic magnitudes.

In any scheme of sidereal research, measurements of stellar brightness are an important element; for a knowledge of stellar distribution and of the concentration of stars toward the galactic plane, they are essential; for the statistical discussion of stellar distances, they furnish criteria of the greatest value; and when both photographic and visual (or photovisual) magnitudes are known, we have immediately available values of the color which, for the fainter stars at least, are of much importance, since they afford, for objects inaccessible to spectroscopic observations, a hint as to physical condition not to be obtained by any other means.

In every photometric research a precise knowledge of the scale of magnitudes is an essential feature, and for the Mount Wilson investigation this primarily was the requirement. Professor Kapteyn had previously been furnished with photographs of an hour's exposure, with the 60-inch reflector, on each of the selected areas on and north of the parallel at $-15°$ declination; and for the reduction of these photographs sequences of standard magnitudes were required. Although

standards for the faint stars were especially desired, brighter objects have also been observed in order to facilitate comparisons with other photometric systems.

A simple means of deriving a sequence of standards is to transfer to the region in question, by means of intercomparison photographs, the standards already established at the Pole. But in the present case this method could not be used; the range of brightness to be covered was too great, and the limiting magnitude to be attained too faint.

Separate determinations of the scale were accordingly undertaken for each area, and, as a means of greater precision, these were based on all the stars shown on the photographs used. We have, therefore, for each region a large number of magnitudes, instead of merely sequences of standards. The labor was thereby somewhat increased, but, besides the greater accuracy, there is the advantage of a complete utilization of the data of observation.

To establish the photographic scale of magnitudes, the relation between the brightness of a star and the size of its photographic image must be found. This can be reliably accomplished by means of successive exposures, of constant duration, made on the same plate with light of different intensities.[2] The variations in intensity, which must be known, can be produced through a change in the effective aperture of the telescope by adding diaphragms or screens of wire gauze. Individual stars will therefore show two or more images, with differing dimensions corresponding to known differences of magnitude. When once the images on any photograph have been completely measured, a simple interpolation process quickly gives the relative magnitudes of all the stars. To determine their absolute values, a zero-point correction must be added, whose value has to be derived from other data.

If the scale is to be established over a considerable range of magnitude, photographs of both long and short exposure will be required; and to reduce the influence of the various errors, which in photometric work are always numerous and troublesome, several separate determinations will be made, preferably with diaphragms and screens which change the intensity by different amounts. The program adopted for each of the Selected Areas is shown in the Table.

NO. PLATES	APERTURE IN INCHES	DURATION OF EXPOSURES
1	60, 32, 32, 60	15^m
1	60, Screen, Screen, 60	15
2	60, 32, 14, 9, 9, 14, 32, 60	2

This arrangement of exposures admits of six separate determinations of the scale, and a possible maximum of sixteen different values of the

magnitude of a given star, though the average number is only five or six; for the faintest stars there are naturally but two—those derived from the full-aperture images of the two plates of long exposure.

It was planned originally to include only the 115 Selected Areas on and north of the Equator, and the 460 plates necessary for the derivation of relative magnitudes in these regions have nearly all been taken. The measures are also well advanced, and for 80 regions the reductions, up to and including the relative magnitudes, are complete. It now seems desirable to include the 24 areas at $-15°$, and these accordingly will be added to the program.

To reduce the relative magnitudes to absolute values on the international system, it is convenient to rely upon the North Polar Standards, for which precise results are known. To determine the various zero-point corrections, two series of intercomparison photographs have been made: first, comparisons of each area with the adjacent areas of the same zone of declination, and second, duplicate comparisons of six equidistant areas in each zone with the region of the Pole. By means of the zonal comparisons, the magnitudes of each zone can be brought to a common zero-point, which, through the polar comparisons, can finally be referred to the international standard. Since the intercomparison exposures are of only two minutes duration, the photographs are rapidly accumulated. All but 50 of the 302 plates required for the original program have been obtained.

Since these photographs have not yet been reduced, the lower limit for the magnitudes is not accurately known, but probably it is not far from 17.5 on the photographic scale. The range of brightness is always such as to include the central star of the area and usually is eight magnitudes or more. The field is that of good definition, with the full aperture of the 60-inch mirror; its diameter is 23′ and its area about one-ninth of a square degree. The number of stars shown in such a field on the plates of fifteen minutes exposure varies from 40 or 50 in high galactic latitudes to 2000 or 3000 in the star clouds of the Milky Way. The total for the Areas on and north of the Equator should be 55,000 or 60,000.

[1] Published by the Astronomical Laboratory at Groningen.

[2] Seares, F. H., *Mt. Wilson Contrib.*, No. 80; *Astroph. J., Chicago*, 39, 1914, (307–340).

ARCHAEOLOGY OF MAMMOTH CAVE AND VICINITY: A PRELIMINARY REPORT

By N. C. Nelson

AMERICAN MUSEUM OF NATURAL HISTORY, NEW YORK

Communicated by H. F. Osborn, February 2, 1917

American anthropology appears to have arrived at something like a turning point in its history. The continent has now been covered more or less intensively and on the basis of the collected data both the ethnologists and the archaeologists are beginning to publish maps outlining in a tentative way the boundaries respectively of the historic and the prehistoric culture centers. (See W. H. Holmes, also Clark Wissler, *Amer. Anthrop., New York*, **16**, No. 3, 1914; and Wissler in the Holmes Anniversary Volume, 1916.) These two maps, as would be expected, while not identical show a noteworthy correspondence.

The question at once arises: how came these centers of cultural intensification to be? Thus far we have no scientific answer. We may say either that they were originated and developed in place or that they were transmitted from without, or finally—and what is most likely—that they are the products partly of transmission and partly of local origination. In any event the given culture complex did not drop out of the heavens in its perfected form: it has a history and what that history has been is for the archaeologist to determine.

That the archaeologist can do this service need not be questioned. He has done it—or at least so it appears—in Europe. To some of us his performance is not entirely satisfactory as yet and when he insists that by searching in the New World we shall find the identical state of things which he himself has found in the Old World we openly rebel.

But having said this we must still allow the European to point with pride to his chronological series and at the same time admit the justice of his criticism to the effect, viz., that our own investigations are almost totally deficient in this respect. Our museums abound in choice collections from this and that type locality but we possess little knowledge of how the diversified cultural traits of which these collections bear testimony came about or what are their antecedent relationships. In so far as we mentally arrange these cultures in order of their complexity beginning, let us say, with the shellmound peoples and ending with the Mayas or the Incas it is a purely selective procedure, perhaps essentially true but nevertheless devoid of real scientific merit.

It is true that some sporadic efforts at chronological determinations of different sorts have been made, as, for example, by Dr. Uhle in Peru,

by Dr. Spinden in the Maya area, by the writer and others in the California shellmounds and by a number of men from different institutions—including the American Museum—in the Trenton gravels, as well as in several of the rockshelters of New Jersey and finally by the writer again in the Southwest. All these attempts are of very limited significance, however, each embracing but an infinitely small segment of the entire cultural curve. Without elaborating on the subject any further it must be tolerably clear from the foregoing how urgent is the need for problem work in American archaeology.

Being convinced of this necessity, the American Museum last summer made a preliminary investigation of some of the Kentucky caverns. Caves and rockshelters, in view of the wonderful returns they have yielded in Europe, are difficult to resist even though they have been tried over and over again in America with practically negative results. The general locality was deliberately chosen as being well south of the limits of glaciation, and in fact in some respects quite comparable to the Lower Pyrenees. The quest was not precisely to find evidence of Paleolithic man; it was merely to ascertain whether in the middle Mississippi region there was any trace of a relatively primitive stage of development that might have given rise to the Moundbuilder culture as we know it at its best. In this the writer, who conducted the investigation, is at least morally certain that he succeeded.

In the spring of the year, after consulting with Professor Arthur M. Miller of the Geological Department of the State University at Lexington, two series of caves and shelters were inspected: one along the Kentucky River, south of Lexington, and the other along the Green River in the vicinity of Mammoth Cave. Later in the season some trial excavations were carried out in several of the Green River sites and positive results were obtained in two places, viz., in Mammoth Cave and in a small unnamed rockshelter about six miles lower down the river.

The discovery in Mammoth Cave consisted of a stratified relic-bearing deposit ranging from a few inches to about 4 feet in depth and forming part of the floor debris of the large entrance vestibule. In some places the refuse reached the surface of the cave floor while in others it was buried under as much as 4 feet of sterile cave earth and rock laid down unquestionably by modern man. The body of the refuse was composed largely of ashes, being presumably the slow accumulation of aboriginal hearth fires. In this matrix was found a considerable quantity of crushed animal bones among which have been distinguished the deer, beaver, opossum (?), turtle, bat and several birds; as well as numerous shells representing two or three species of fresh-water bivalves,—all of it

together being nothing more nor less than 'kitchen' refuse. Isolated portions of the human frame, such as, for example, the distal end of an adult femur, a small fragment of a pelvic girdle and several teeth, were met with at different levels in the deposit; but the remains are not, as might be supposed, in such a state as to suggest cannibalism. Of artifacts in a stricter sense of the term there were obtained a small series of awls and other pointed implements made from bone and antler, many of them being simply improvised from splinters while a few were as perfect as anything of the sort found in America. The flint specimens consisted of numerous reject flakes (some of them apparently used) and five or six chipped blades, mostly fragmentary. Of workmanship in shell there came to light two perforated pendants and several half-shells that may have served as spoons or scrapers. Finally, there were uncovered three more or less crude pestle-like objects made of limestone. And that was all. Several test trenches were opened at different places and quite a number of cubic yards of the refuse was most carefully worked over, but without avail. Not a trace could be found either of maize or of pottery or of any of the finer forms of polished stone work otherwise so characteristic of the general region. The absence of these things in Mammoth Cave may be an accident but if so it is most extraordinary.

The second discovery, as previously stated, was made in a rock-shelter about six miles down the Green River, in the bluff opposite the lower end of Boardcut Island. The place is about three miles in an airline from the Mammoth Cave. Here in the natural floor earth was found a thin stratum of ashes containing bits of flint and also fragments of pottery. A little deeper down was laid bare a stone-grave burial of the type so often met with in that section of the Mississippi basin and generally accepted as of relatively late date. Unfortunately nothing whatever accompanied the interment, and as conditions did not permit digging more than the one test pit no complete artifacts were obtained from the shelter.

The significance of these two isolated horizons of culture, briefly stated, seems to be that we have evidence in the one instance of an agricultural people and in the other of a people who lived mostly if not entirely by hunting. The rockshelter horizon being clearly associable with the later developments in the Moundbuilder area, we must assume the cultural segment in the Mammoth Cave to antedate it. At the same time it should be emphasized, in conclusion, that the Mammoth Cave horizon, in view of the faunistic remains accompanying it, cannot in any absolute sense be regarded as ancient.

A detailed discussion of these investigations will appear in the Anthropological series of the American Museum of Natural History, New York City.

THE PRODUCTION IN DOGS OF A PATHOLOGICAL CONDITION WHICH CLOSELY RESEMBLES HUMAN PELLAGRA

By Russell H. Chittenden and Frank P. Underhill

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Communicated, February 2, 1917

The nutritional diseases designated as beri beri, scurvy and pellagra are currently believed to be induced by a deficiency in the diet of some undefined but essential constituent or constituents. From the experimental standpoint the pigeon is peculiarly sensitive when the diet fails to provide a sufficiency of these unknown but essential food substances, and polyneuritis develops. The symptoms of polyneuritis in birds closely resemble those of beri beri in man. The guinea pig has proved to be especially susceptible to scurvy. On the other hand, pellagra in the ordinary laboratory animals is rarely mentioned.

We have accomplished the experimental production in dogs of a diseased condition which closely resembles human pellagra. The characteristic phenomena are readily evoked by feeding these animals a diet consisting of boiled (dried) peas, cracker meal and cotton seed oil, or lard. The ingestion of suitable quantities of meat causes the symptoms of disturbed nutrition to disappear. On the other hand, if the amount of meat contained in a selected mixed diet is insufficient, the same evidences of abnormality may be exhibited. The symptoms appear in varying periods of time which may be altered by changes in the character of the diet.

The onset of the symptoms is generally very sudden. Usually the first abnormal manifestation in dogs is a refusal to eat, and cursory examination reveals nothing to account for the loss of appetite. The animal lies quietly in its pen and is apathetic. After continued refusal to eat for a day or two, the mouth of the dog presents a peculiar and characteristic appearance, in that the inner surface of the cheeks and lips, and the edges of the tongue, are so covered with pustules as to give the impression of a mass of rotten flesh. The odor from the tissues is foul. The mucous lining of the mouth comes away in shreds when stroked with absorbent cotton. Intense salivation exists. The teeth remain normal. A bloody diarrhoea is present, attempts at defecation being very frequent, and resulting in the passage of little more than a

bloody fluid of foul odor. In some cases the thorax and upper part of the abdomen may contain many pustules one half an inch in diameter and filled with pus organisms. No other skin lesions are prominent. Death usually results without any particularly striking features.

In some instances convulsions constitute a distinctive symptom which may or may not be manifested simultaneously with the abnormal symptoms already described.

At autopsy two types of conditions are recognizable. In the animals presenting foul mouth and bloody diarrhoea the chief interest centres in the lower bowel and in the rectum which exhibit an intense hemorrhagic appearance. With those animals dying rapidly from convulsions the only visible abnormality of the alimentary tract is the presence in the duodenum of one or more large ulcers.

The detailed data of the investigation which will be published elsewhere justify the following summary and conclusions:

Dogs fed upon a diet consisting of boiled (dried) peas, cracker meal and cotton seed oil, or lard, rapidly develop symptoms indicating abnormal nutrition. This condition eventually terminates in death. Previous to the development of the pathological manifestations the dogs are usually in nitrogen balance and exhibit excellent food utilization. The nitrogen partition of the urine is normal when compared with that of animals maintained upon the same level of nitrogen intake.

The pathological symptoms at times can be made to disappear and the normal condition of nutritional rhythm can be re-established by the addition of meat to the dietary.

In the production of the symptoms it is immaterial whether the transition from a diet containing meat to one of vegetable origin is sudden or gradual. The final outcome is the same in both cases.

The intake of a large quantity of peas is less detrimental than smaller amounts.

In the development of the pathological condition the level of nitrogen intake as such plays little or no rôle.

The typical symptoms may be induced in dogs, but with much greater difficulty, when a diet containing meat, cracker meal and lard is fed in appropriate quantities. For the production of the diseased condition the meat intake must be reduced to a certain undefined minimum. Under these circumstances less than fifty per cent of dogs exhibit pathological symptoms and these may appear in periods of two to eight months.

From the facts enumerated the conclusion seems tenable that the abnormal state may be referred to a deficiency of some essential dietary

constituent or constituents presumably belonging to the group of hitherto unrecognized but essential components of an adequate diet.

In the essential features the pathological manifestations described in this investigation closely resemble those which may be observed in human pellagra.

THE COMPLETE ENUMERATION OF TRIAD SYSTEMS IN 15 ELEMENTS

By F. N. Cole, Louise D. Cummings, and H. S. White

Read before the Academy, November 14, 1916

If any set of 15 points are joined by all the possible 105 connecting lines, then these may be combined in sets of three to form 35 triangles. The marks or 'elements' designating the three vertices are a sufficient description of any one triangle, and a list of all 35 triangles constitutes a 'triad system on 15 elements.' Two such sets of triangles are essentially alike if a renaming of its points turns the one list into the other; if that cannot be done, the two lists or systems are essentially different. How many essentially different triad systems can be formed of a given number of elements, is a question of much difficulty, never before answered when the number of elements is 15 or more. For 13 elements there are but 2 different systems, for 9 or 7, only one. The present paper shows that for 15 elements, there are exactly 80 different systems. This conclusive result is established by Mr. Cole.

Three years ago the dissertation of Dr. Cummings increased the number of known triad systems on 15 elements from 10 to 24, and furnished a definite method for comparing systems and verifying their difference or equivalence. All the new systems found by Miss Cummings contained a 'head' —a triad system on 7 of the 15 elements, while one exceptional system among the 10 previously known was 'headless,' a system constructed by Heffter. But all of them admitted groups of transformation into themselves, and it was suspected that the possession of a group might be a necessary property of triad systems. Mr. White takes the group for a starting-point; finds seven types of substitutions, one or more of which must occur in the group; and constructs all the district triad systems for each of those seven typical substitutions. This gives as a gross result 83 systems. By two methods, that of Miss Cummings (by sequences) and one introduced by Mr. White (by trains) these are tested, duplicates are eliminated, and the net result is found to be 44 systems. Of these, exactly 23 exhibit heads and are equivalent to those in Miss Cummings' list, while 21 are headless, including the one such (Heffter's) previously

known and twenty new ones. The orders of the groups are 2, 3, 4, 5, 6, 8, 12, 21, 24, 32, 36, 60, 168, 192, 288, and 20160.

Groupless systems on 31 elements were next found (see these PROCEEDINGS, 1, 1915, 4), and the question was raised whether possibly groupless systems could exist in 15 elements. This question is answered by the actual construction of several such systems by Mr. White and Miss Cummings. But while their empirical method is productive, it is not deductive and so could not be shown to be exhaustive. A new starting-point and a new method are requisite, to insure a complete survey of groupless systems as well as the better known kinds. This method was furnished, and its tedious and difficult execution undertaken, by Mr. Cole.

Starting out with the four possible openings:

I............	123	145	167	189	11011	11213	11415
		246	278	2910	21112	21314	2515
II............	123	145	167	189	11011	11213	11415
		246	258	279	21012	21114	21315
III............	123	145	167	189	11011	11213	11415
		246	257	2810	2912	21114	21315
IV............	123	145	167	189	11011	11213	11415
		246	257	2810	2911	21214	21315

which, from the mode of interlacing of the triads containing 1 with those containing 2, may be called dodekad, hexad, single tetrad, and triple tetrad types, respectively, he began by proving that no system could be built up with interlacings of type I alone. Then it was found that with types I and II alone only one system was possible: that already found by Heffter. With this one exception every triad system in 15 letters has an interlacing of type III or IV.

The census was next continued by working out all the systems containing type IV. These included most of the systems with a 7-head (triad system in 7 of the 15 letters); a brief excursus covered the remainder of the 7-head systems. There were in all 23 systems with 7-head and 38 without 7-head.

There remained the systems with type III but not type IV. These were divided into two classes: (1) those with a 'semi-head:' 123 145 167; 246 257; 347; and (2) those without semi-head. Of the former there were 3, of the latter 15.

The total number of types of triad systems in 15 letters therefore proves to be 80.

Proof that the 44 systems with groups are different is based on the set of trains belonging invariantively to each system. There are over

200 kinds of trains altogether, represented in their connection by graphs, so that a glance furnishes intuitive evidence of their essential difference. For the groupless systems Miss Cummings gives a table of the indices which represent the sequences of each system, and comparison is not difficult. She exhibits also a table of the four varieties of interlacing of pairs, as distinguished by Mr. Cole, showing how many of each kind are found in each system. These latter data alone are found, in eight cases, to fail to discriminate two systems actually different. Perfect discrimination would almost certainly be possible by the use of a double entry table, 15 by 15, showing the exact distribution of tetrads, hexads, oktads, and dodekads.

NEW DATA ON THE PHOSPHORESCENCE OF CERTAIN SULPHIDES

(DISCUSSING MEASUREMENTS BY DRS. H. E. HOWE, H. L. HOWES AND PERCY HODGE)

By Edward L. Nichols

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Read before the Academy, November 14, 1916

Ph. Lenard to whom we owe extended studies of the class of highly phosphorescent substances known as the Lenard and Klatt[2] sulphides, describes[3] the spectrum of the emitted light as consisting of a single broad band in the visible spectrum. This band which appears single in most cases, as viewed with the spectroscope, does not however conform to the recognized criteria. The marked difference between the color of fluorescence and that of phosphorescence and the changes of color during decay, suggest over-lapping bands. As shown by E. Becquerel[4] the color of the emitted light varies with the wave length of the exciting rays. His observations apply, it is true to sulphides of barium, calcium and strontium not identical with the preparations of Lenard and Klatt, but belonging to the same class. In a recent paper[5] I gave more direct evidence of the existence of more than one band in the spectra of these substances.

In their original paper[2] Lenard and Klatt depicted these spectra as complex instead of single; but in both the earlier and the later papers attention is given rather to the mode of excitation than to the character of the phosphorescent light itself and the regions of excitation in the violet and ultra violet are carefully mapped.

Significance of the Bands of Excitation.—It seemed probable that these regions of maximum excitation, the positions and appearance of which

had long since been depicted by Becquerel, were due to the presence of absorption bands. Dr. H. E. Howe, who was employed last summer in the study of ultra violet absorption spectra, was kind enough to test this hypothesis.

In his experiments the phosphorescent substance was exposed to the continuous ultra violet spectrum of the powerful submerged aluminum spark described by Henri[6] and subsequently employed by Howe[7] in his study of absorption spectra. The bands of excitation (Erregungsbände of Lenard) were thus located and with these absorption bands of the phosphoresent substance, obtained by reflection, were shown to coincide in position and extent. In the case of a barium sulphide with lead as an active metal, the crests of the bands of excitation were at 0.380μ and 0.335μ. Lenard gives for a sulphide of similar composition, 0.377μ and 0.332μ.

The crests of the absorption bands are at 375μ and 332μ. Similar coincidences were established in the case of Sr, Zn and Sr, Pb sulphides and the relation is therefore probably a general one corresponding to that already established in the case of the selective activity of infra red rays upon the phosphorescence of zinc sulphide,[8] where the maximum effect was found in regions of maximum absorption.

Spectrophotometric Measurements.—A detailed spectrophotometric study reveals widely varying degrees of complexity in the spectra of these sulphides.

Dr. H. L. Howes kindly made for the writer very careful measurements of three characteristic compounds. Settings were made at intervals of 50 Angstrom units throughout the spectrum.

The curves thus obtained show the existence of many overlapping bands so nearly merged that to the eye the appearance is that of a single simple band. There is moreover a distinct suggestion of a systematic relation.

Taking the relative frequencies, i.e., reciprocals of the approximate wave lengths of the crests ($1/\mu \times 10^3$) it is found that the over-lapping bands are members of one or more series of constant frequency interval; which interval varies for the different sulphides.

Whether the spectra under consideration are to be regarded as consisting of a single band or of more than one band is not a question of complexity of structure.

Any system however complex which behaves as a unit under varying conditions of temperature, mode of excitation, etc., all the components being affected in like manner, may be considered as a single band in the sense in which that term has been used by Lenard. We have a striking

example indeed of such bands or systems of great complexity of structure in the case of the uranyl salts.

The evidence that, in general, the spectra of the phosphorescent sulphides contain more than one band or complex has already been mentioned, e.g., the marked changes of the color of phosphorescence with temperature and during the process of decay.

The Decay of Phosphorescence in Different Parts of the Spectrum.—To obtain the curve of decay for a restricted region of the spectrum Drs. Howes and Hodge used a spectrophotometer in combination with the synchrono-phosphoroscope. In this way a set of curves corresponding to several nearly equidistant regions within the phosphorescent spectrum were obtained for each of the three sulphides under consideration.

A notable feature of all these curves is the existence of two so-called linear processes the first of steeper slope and therefore indicative of a more rapid decay of phosphorescence than the second. This form of curve, as is well known, is characteristic of phosphorescent substances in general, the only well established exceptions being those occurring in the case of the uranyl salts.[9] As regards the relation of the two processes recorded in these diagrams to what appear as the first and second processes in the usual study of the long time phosphorescence of such sulphides, it is clear that the second process in our curves is not identical with the first process as observed by the usual long time methods. Assuming the second process to continue, the intensity after one second would be about 1/1000 of that at 0.01 second or roughly 1/20,000 of its initial brightness whereas as is well known these substances retain an easily visible phosphorescence after many seconds.

Owing to the over-lapping of the components in the spectra under consideration it is difficult to determine whether the group of equidistant bands are to be regarded as a unit, as is in the case of the uranyl salts or indeed whether they constitute the whole of the phosphorescence spectrum. To that end some method permitting of more complete resolution must be devised.

The pronounced changes in the color of the phosphorescent light would make it seem probable that we have to do in these observations chiefly with components of the phosphorescence that are of rapid decay and that, after a few hundredths of a second, these disappear leaving behind other components which constitute the phosphorescence of long duration. These, which are probably of relatively insignificant initial brightness, doubtless overlap the phosphorescence of short duration but occupy, as a whole a somewhat different portion of the spectrum.

In that case since one has to do with a different group of bands in

observing the initial and the later phases of phosphorescence there would be an actual discontinuity between the processes referred to above and those in the curves for the phosphorescence of slow decay.

Summary.—(1) The regions of selective excitation (the bands of excitation) for the Lenard and Klatt sulphides are shown to coincide in position and extent with absorption bands in the transmission spectrum of the substances.

(2) The spectrum of the phosphorescent light during the first few thousandths of a second after the close of excitation, contains one or more groups of over-lapping bands the crests of each group forming a spectral series having a constant frequency interval.

(3) The decay of phosphorescence during the first three hundredths of a second after the close of excitation may be described as consisting of two processes each showing a linear relation between $I^{-\frac{1}{2}}$ and time. The first and more rapid process lasts for less than 0.01 second for the three sulphides studied under the intensity of excitation employed. The second process probably persists for 0.06 seconds or more.

(4) The phosphorescence of long duration of the sulphides under consideration is probably due to another group of bands of comparatively feeble initial brightness which come under observation only after the phosphorescence of short duration has vanished.

[1] The investigation was carried out in part with apparatus purchased by aid of a grant from the Carnegie Institution of Washington.

[2] Lenard and Klatt, *Ann. Physik, Leipzig*, (Ser. 4), **15**, 1904, (225).

[3] Lenard, *Ibid.*, **31**, 1910, (641).

[4] Becquerel, E., *La Lumière*, Vol. 1, 1861.

[5] Nichols, *Philadelphia, Proc. Amer. Phil. Soc.*, **55**, 1916, (494).

[6] Henri, V., *Physik. Zs., Leipzig*, **14**, 1913, (516).

[7] Howe, *Physical Rev., Ithaca*, (Ser. 2), **8**, 1916, (637).

[8] Nichols and Merritt, *Washington, Carnegie Inst., Pub.*, No. 152, (84).

[9] Nichols, these PROCEEDINGS, **2**, 1916, (328).

THE REACTIONS OF THE MELANOPHORES OF THE HORNED TOAD

By Alfred C. Redfield

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Communicated by G. H. Parker, February 3, 1917

The reactions of the melanophores of the horned toad Phrynosoma cornutum are of three distinct types: (1) Those which manifest themselves in a daily rhythm of reactions, correlated with definitely changing

* Contributions from the Zoological Laboratory of the Museum of Comparative Zoology at Harvard College, No. 292.

conditions of illumination and temperature; (2) those which result in an approximation of the color of the skin to that of the substratum on which the lizards live; and (3) those occurring during nervous excitement.

The daily rhythm of melanophore reactions consists in an expansion of the melanphore pigment, and a consequent darkening of the skin, in the morning and afternoon, and a contraction of the melanophore pigment and a paling of the skin at mid-day and at night. These reactions are due to the interaction of illumination and temperature upon the pigment cells. At mean temperatures (20°C. to 30°C.) the melanophore pigment is expanded in the light and contracted in the dark. In this way the coloration of the skin in the morning, afternoon, and night is explained. At higher temperatures the melanophore pigment is contracted irrespective of illumination; the pale coloration of the skin at mid-day is thus explained. At lower temperatures the melanophore pigment is expanded irrespective of illumination.

The responses of the melanophores to illumination and temperature are due to the direct action of these stimuli upon the pigment cells or some closely associated tissue, for: (1) a local illumination, a local shadow, or a local heating of the skin produces a local reaction of the melanophores; (2) the reactions to illumination and temperature take their normal course in regions of the skin which have been isolated from the nervous system.

The adaptive reactions of the melanophores are initiated by stimuli received through the eyes. If horned toads are blindfolded, no adaptive reactions take place. This inhibition of the reaction is not due to the mechanical effects of blindfolding. Upon the adaptive reactions are superimposed the daily rhythm of color changes, with the result that lizards adapted to a dark substratum become paler at night and at mid-day, while lizards adapted to a light-colored substratum become darker in the morning and afternoon.

During states of nervous excitement the melanophore pigment of the horned toad is so contracted that the color of the skin becomes pale. This reaction is brought about by any noxious stimulus, such as prolonged mechanical or faradic stimulation, holding an animal on its back, or prying open its mouth. The reaction occurs irrespective of illumination, temperature, or the adaptive condition of the skin. The coördinative mechanism, by which a local noxious stimulus brings about a reaction of the melanophores of the entire body is described in the following paper. Probably this mechanism also carries out the adaptive reactions of the melanophores.

THE COÖRDINATION OF THE MELANOPHORE REACTIONS OF THE HORNED TOAD

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Communicated by G. H. Parker, February 3, 1917

In a foregoing paper (Redfield, 1916)[2] it was pointed out that the melanophores of the horned toad Phrynosoma cornutum are coördinated by a hormone, produced during nervous excitement, which causes its pigment to contract. The following considerations indicate that this hormone is adrenin, the secretion of the adrenal glands. (1) Adrenin in very minute subcutaneous doses causes a contraction of the melanophore pigment. (2) The adrenal glands exhibit the chromaffin reaction and their extract not only yields a characteristic physiological test for adrenin, but causes the melanophore pigment to contract when injected subcutaneously into a horned toad. (3) Faradic stimulation of the adrenal glands causes a contraction of the melanophore pigment of the skin. (4) The occurrence of "emotional" hyperglucemia in the horned toad indicates that adrenin is secreted during nervous excitement. (5) The melanophore pigment is contracted under conditions known to produce adrenal secretion in mammals, i.e., asphyxia, ether anaesthesia, morphia and nicotine poisoning. (6) Removal of the adrenal glands blocks the reaction of the melanophores, so that no contraction of the pigment accompanies nervous excitement. In the majority of individuals it is necessary to destroy the nervous system supplying a part of the skin (an operation which of itself does not block the melanophore reaction) before this effect of adrenalectomy manifests itself.

The melanophores are also under the direct control of the nervous system. Stimulation of the sciatic nerve causes a contraction of the melanophore pigment of the leg. Transection of the spinal cord prevents the melanophores posterior to that region from being affected, when, as a result of nervous excitement, the melanophore pigment of the remainder of the skin contracts. It is necessary that the adrenal glands be removed before this procedure; otherwise the secretion of adrenin will cause a contraction of the melanophore pigment of the entire body, and thus mask the effect of the operation upon the nervous system.

The nervous system and the adrenal glands act in a supplementary way in coördinating the reactions of the melanophores, both being called into action during nervous excitement. The fact that the melan-

ophore pigment is contracted by adrenin may be taken as a strong indication that the melanophores are under the control of the sympathetic division of the autonomic nervous system (Elliott, 1905).[3] A resemblance therefore appears between the mechanism coördinating the melanophores of the horned toad and that coördinating the smooth muscles of the mammalian body.

Smooth muscles commonly possess a double innervation, each contractile element being influenced by fibers from the sympathetic and from the cranial or sacral division of the autonomic nervous system. These pairs of fibers act in an opposite sense upon the muscle. Heretofore no analog of the cranial-sacral division has been demonstrated to affect melanophores. The melanophores of the Florida chameleon, Anolis carolinensis, are controlled by nerves belonging to the autonomic nervous system, impulses from which cause their pigment to *expand* (Carlton, 1903).[4] If these fibers belong to the sympathetic division of the autonomic nervous system, adrenin should have a similar effect upon the pigment cells. Adrenin, however, *contracts* the melanophore pigment of Anolis. It is suggested, consequently, that the melanophores of Anolis are controlled by fibers analogous to the cranial-sacral autonomic nervous system of mammals. It is not improbable that both divisions of the autonomic nervous system are in control of the melanophores of many vertebrates, but only that division manifests itself which dominates when the nerve trunks are stimulated.

The resemblance between the coördinative mechanism of melanophores and smooth muscles supports the contention of Spaeth (1916)[5] that these pigment cells are functionally modified smooth muscle cells. The fact that this mechanism is brought into action during nervous excitement indicates that the physiological basis of emotional manifestations is similar in reptiles and mammals.

[1] Contributions from the Zoological Laboratory of the Museum of Comparative Zoology at Harvard College. No. 293.

[2] Redfield, A. C., The coördination of chromatophores by hormones, *Science, New York*, N. S., **43**, 1916, (580–581).

[3] Elliott, T. R., The action of adrenalin, *J. Physiol., London*, **32**, 1905, (401–467, 2 figs.).

[4] Carlton, F. C., The color changes in the skin of the so-called Florida chameleon, Anolis carolinensis Cuv, *Proc. Amer. Acad. Arts Sci., Boston*, **39**, 1903, (257–276, 1 pl.).

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PETRIFIED COALS AND THEIR BEARING ON THE PROBLEM OF THE ORIGIN OF COALS

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Communicated by J. M. Clarke, February 3, 1917

In recent years I have published two preliminary articles on the conditions of formation of coal based upon a study of the internal organization of a large number of coals from various geographical regions and different geological horizons.[1,2] The success of these investigations has been based on improved methods, which are recorded in an article recently published.[3] The conclusion reached, contrary to the usually accepted opinion supposed to be based on reliable stratigraphic evidence is that coal is not in general a deposit laid down *in situ* but is composed of drift materials deposited in open water. The arguments from structure for this opinion seem to be overwhelming, for the typical organization of the numerous coals investigated is entirely canneloid. Since cannel coal is universally conceded to be a deposit accumulated in open water, it follows that the great mass of coals must have been accumulated under similar conditions to those admitted for cannels and oil shales as a consequence of their structure. The present writer accordingly on the basis of new facts arrived at by new and improved methods, must announce his adhesion to the doctrine of coal formation long held in France, namely that this invaluable mineral is formed of drift material laid down in open water and does not correspond at all to the peat deposits formed on land in the present epoch and in temperate climates.

The strongest argument supplied from the structural standpoint for the origin in place of coal is that provided by petrified coals, or as they are often designated 'coal balls.' A very interesting summary account of these structures, with special reference to those occurring in the British Isles, was published some years ago.[4] The conclusion is here reached that the Upper Foot Seam as well as other English and Continental seams of higher and lower geological level, which contain the masses of petrified vegetation known as 'coal balls' consist of vegetable materials accumulated in sea water or at least water that was saline. The preservation of the remains during the years of accumulation is attributed to the antiseptic properties of sea water. It is apparently an unnecessary assumption since very delicate vegetable structures are found at the present day at depths of from three to ten meters in post glacial lacustrine accumulations. It must further be pointed out that the laying

down of vegetable materials in saline estuarial basins, even if this mode of accumulation be conceded, is very far from complying with the conditions of *in situ* formation.

An interesting feature of the organization of 'coal balls' which apparently has not previously been emphasized vouches strongly for the accumulation of their constituents under open water conditions. Very frequently masses of charcoal are found distributed irregularly and without any principle of stratification through the petrified substance of the ball. This burned material irregularly disposed can only be explained naturally as the result of the washings of the relics of forest fires into open water, as often occurs in the case of our actual lakes. In figure 1 is shown part of a 'coal ball' showing on one side Cordaitean wood which had not been burned previous to mineralization and on the other side black woody material which shows all the evidence of transformation into charcoal.

It is sometimes assumed that the occurrence of a peat-like organization in the coal balls vouches for their accumulation *in situ*. Nothing could be further from the truth. The use of the peat prober designed by the late Dr. C. A. Davis of the U. S. Bureau of Mines, in the case of lacustrine accumulations which are still covered by open water, has convinced the author that material, which superficially resembles ordinary peat is formed often at considerable depths, from the sinking of the water-logged twigs, leaves, cones, etc., of land plants to the bottom. Such an accumulation is shown in sections in figure 2, which represents material probed at about the depth of two meters under the open waters of a small lake in Eastern Quebec. It strikingly resembles in appearance the organization of the 'coal balls' shown in figure 1. An organization like that of land peat is consequently clearly not convincing evidence of the terrestrial origin in the case of masses of petrified coal.

It will be convenient next to consider the organization of the coals which surround the petrifactions known as 'coal balls'. Figure 3 illustrates the structure of a coal derived from the well known Upper Foot Seam of Lancashire, England, from which so many of the English 'coal balls' have been secured. The cutting of successful sections of coals from seams producing 'coal balls' has proved to be a matter of considerable technical difficulty on account of the lack of bituminous binding material in such coals. The coals in question have to be held together with wrappings during the process of softening and must receive just the right degree of treatment with hydrofluouric acid and nascent chlorine to furnish utilizable sections. The general results here recorded depend on the examination of the structure of the three different samples of

English coals containing 'coal balls,' which the author owes to the kindness of Dr. Marie Stopes of the University of London whose investigations on the origin of these petrifactions are known to all (4). Through the kindness of Dr. M. Zalessky of the Comité Geologique of Petrograd similar coals have been secured from the great Donetz coal fields of Russia. To the administration of the Geologische Landesanstalt of Prussia, the author owes material of the dolomitic so-called 'Torfkohle' of Westphalia. It will be seen that there has been no dearth of appropriate materials for investigation. The organization in every case has been proved to be the same and is that shown in figure 3. This represents a substance which is composed of darker and lighter bands, more or less interrupted by cracks in the matrix of the coal. The lighter zones correspond to modified woody material present in the original accumulation and the darker stripes to more perishable matter, to judge from the situation in the bottom deposits of existing lakes, often consisting of the excrements of aquatic animals, such as molluscs, fish and amphibia, as well as the more delicate and destructible parts of plants. Where the woody material is massive the resulting coal is frequently quite homogeneous in its character and corresponds to the lighter substance shown in figure 3. The type of organization shown in the figure under discussion is frequent in the higher grade coking coals of the United States, namely those of Pennsylvania and Virginia. Practically no petrified coals however have yet been described for the North American continent.

Figure 4 exemplifies the organization of a typical coal from Lancashire, England. This illustration shows the presence of a large number of light bodies, which are flattened spores. A very large spore (a so-called megaspore) is seen in the lower region, while innumerable smaller ones are scattered throughout the substance of the coal. In addition to the spores are seen bands of darker hue corresponding to much modified wood and black zones which represent the more perishable parts of the original materials from which the combustible has been derived. The bands of the coal under discussion have their counterpart in similar structures shown in figure 3. In the coal ball however the innumerable spores of figure 4 are conspicuous by their absence. It will at once be apparent to the reader that ordinary bituminous coals are much more like cannels in their organization than like that of the seams in which the petrifactions known as 'coal balls' occur. It is thus clear that ordinary bituminous coal must, if structure is a reliable criterion of origin, have been produced under similar conditions to cannels. It is universally admitted that cannel coals are of lacustrine origin and owe

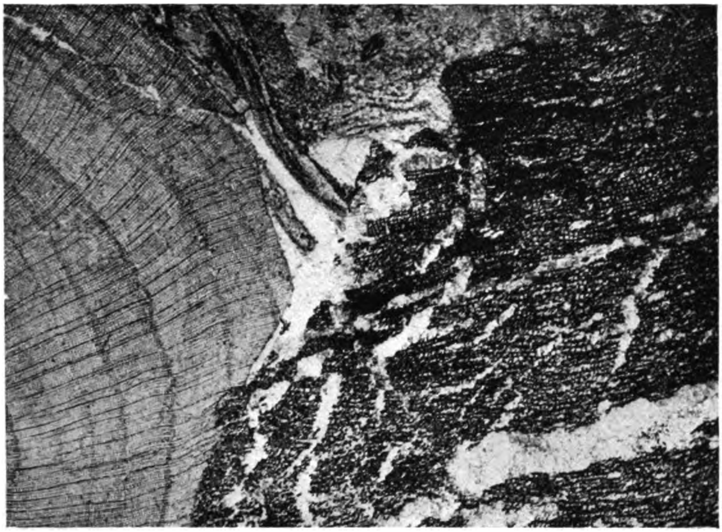

FIG. 1

FIG. 2

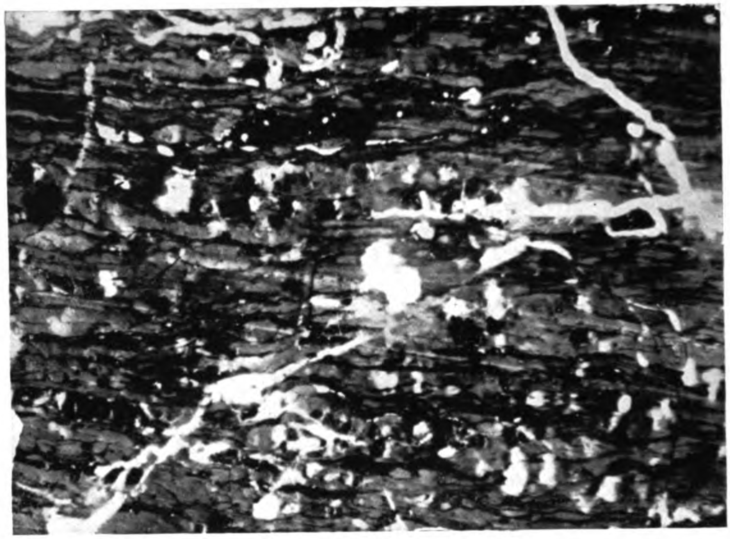

FIG. 3

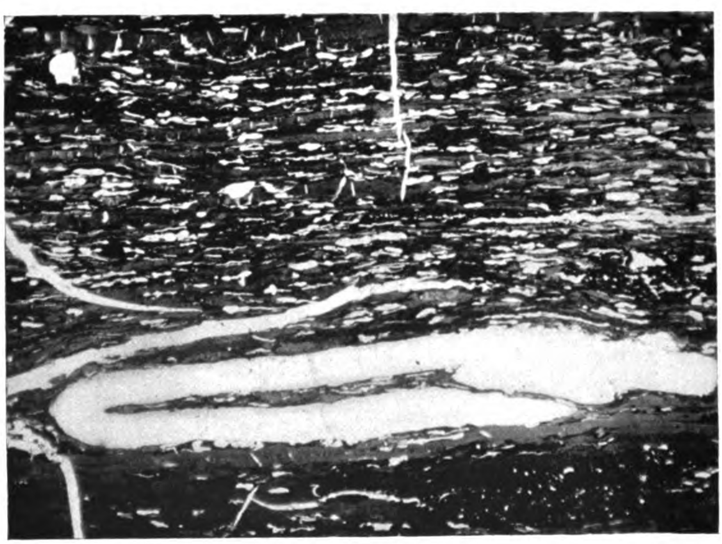

FIG. 4

their accumulation to the bringing of vegetable material from elsewhere by air and by water transport. It follows that the commoner coals have had a similar derivation since they have practically an identical organization.

It may be summarily stated in conclusion, that there is no good evidence that 'coal balls' are organized from material accumulated *in situ*. The facts that they often include isolated masses of charred vegetable matter and that identical material to that composing their substance is often accumulated under modern conditions, by transport and sedimentation in open water, furnish very strong evidence of their formation from transported material. Further the coals in which 'coal balls' have been found are abnormal coals singular by the absence of spore material which is a striking feature of the organization of typical coals of every geological age and all geographical regions. The structure of the coals containing the 'coal balls' cannot consequently be used as an argument in favor of the *in situ* origin of coals in general, even if it were proved that they themselves had been accumulated in this manner, which, as has been shown above on the basis of the organization of 'coal balls,' is very far from being established. The great mass of coals by their close resemblance in organization to cannels show that they have been laid down under the open water and transport conditions, which are universally conceded for the coals of the canneloid category. A fuller account with evidence in greater detail will appear at a later date.

[1] Jeffrey, E. C., Economic Geology, 9, 1914, (730–742).
[2] Jeffrey, E. C., *Chicago J. Geol. Univ. Chic.*, 23, 1915, (218–230).
[3] Jeffrey, E. C., *Science Conspectus, Boston*, 6, 1916, (71–76).
[4] Stopes and Watson, *London Phil. Trans. R. Soc.*, B. 200, 1907, (167–218).

THE EFFECT OF DEGREE OF INJURY, LEVEL OF CUT AND TIME WITHIN THE REGENERATIVE CYCLE UPON THE RATE OF REGENERATION

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Communicated by E. G. Conklin, January 22, 1917

1. *Degree of Injury*.—In a former series of papers the writer gave the results of experiments on the effect of degree of injury upon the rate of regeneration. A number of different species of animals and various combinations of injuries were involved. The results obtained tend to show that on the whole, within certain limits, the rate of regeneration

from an injured surface is not retarded by simultaneons regeneration in other parts of the body. Where a difference exists between the rates with and without additional injury there is usually an advantage in favor of the part with additional injury. The differences are however slight and in some cases come within the limits of probable error. It is only when the data are taken as a whole that it is possible to judge of the correctness of the general conclusion that within fairly wide limits of additional injury there is certainly no decrease in rate of regeneration but rather a tendency toward an increase.

Additional data on these points have been obtained and a further analysis of the problem was made with a view to the determination of the effect of additional injury to a like organ as compared with additional injury to an unlike organ.

Two of the six experiments will be mentioned here. In the first the experiment consisted in the determination of the length of the regenerating right fore-leg of the salamander, Amblystoma punctatum, under three degrees of injury; (1) when the right fore-leg alone is removed, (2) when its mate is also removed, and (3) when its mate and one-half of the tail are removed. The second degree involves the removal of additional material of the same kind and the third a further removal of material of a different kind. In every case it is the regeneration of the fore-leg that is used as the basis of comparison.

At two days the average regenerated lengths of the fore-leg are respectively 0.13, 0.16 and 0.15 mm. for the three degrees of additional injury; at four days the corresponding values are 0.22, 0.36 and 0.29; at six days 0.42, 0.53 and 0.55; at eight days 0.66, 0.83 and 0.73; at ten days 0.91, 1.34 and 1.24; at twelve days 1.48, 1.60 and 1.61; at fourteen days 1.98, 2.19 and 2.29; at sixteen days 3.02, 3.01 and 3.08; and at nineteen days 3.84, 3.64 and 3.90. In no case does the removed fore-leg with no additional injury to the animal give the highest value for regeneration. In all but two of the cases it has the lowest value.

The comparisons show that the regeneration of a fore-leg is not as rapid as when the individual is regenerating no other part of the body as it is when the other fore-leg is being regenerated at the same time. There is, however, no essential difference between the effect of additional injury of a fore-leg and an additional injury of a fore-leg plus one-half of the tail. It may be that the effect of additional removal is confined to removal of a similar part. On the other hand, the accelerating effect may be found only within certain degrees of injury, the limit being exceeded by the highest of the three degrees.

In order to test further the view that the accelerating effect may be confined to additional removal of a similar organ a comparison was made of the regenerating tail lengths following removal of one-half of the tail under the two degrees of no additional injury and of removal of the two fore-legs. In such a case there is no difference in rate of regeneration of the tail.

These experiments, together with others that have been made, when taken as a whole show that a part regenerates slightly more rapidly when additional material of the same kind is removed than when the part alone is removed. Simultaneous removal of tail material, however, may not accelerate the regeneration of a leg and simultaneous removal of a leg may not accelerate the regeneration of the tail. The rate in these cases is, however, not decreased by the additional injury. The statement may therefore be made that within limits the regeneration of a part is not retarded by simultaneous removal and regeneration of material in other parts of the body. When this additional material is of the same kind as that whose rate is being studied there may even be an acceleration of regeneration.

2. *Level of Cut.*—That the level of the cut has an important influence upon the rate of regeneration has been made out by a number of investigators. Their work indicates that regenerations from deeper levels are on the whole more rapid than from more superficial ones. The present data confirm this conclusion and make possible a further analysis of the relation. They show that within wide limits the length regenerated in the tail of an Amphibian larva is directly proportional to the length removed. Within these limits therefore the length regenerated per unit of removed length is a constant.

First and second regenerations of the tail in the tadpoles of Rana clamitans and first regenerations in the salamander, Amblystoma punctatum, give essentially the same results. Second regenerations of the frog will be taken as an example. The removed tail lengths were 1.5, 2.8, 4.9, 8.4, 13.1 and 18.1 mm. or respectively 6, 10, 18, 31, 49 and 67% of the original tail lengths. The regenerated lengths for these six levels ten days after the operation were respectively 1.0, 1.3, 1.4, 2.3, 3.7, and 5.1 mm., an increase for each increase in depth of the level of injury.

The specific lengths or lengths regenerated per unit of removed length as calculated from these values are respectively 0.67, 0.46, 0.29, 0.28, 0.28 and 0.28, a close approach to constancy for removed lengths of 4.9 to 18.1 mm. Figures 1 and 2 give graphic representations of these results.

An analysis of the progress of regeneration from the time of the operation to the completion of the process shows that the proportional increase in regenerated length with the increase in removed length and resultant constancy in specific regenerated length applies only to the material produced by active cell division. During the first four days in frog tadpoles, when the regenerating part is made up almost entirely of cells that have migrated from the old tissues without division, there is no such relation. The length of new material at this time is not strikingly different for the different levels and the process seems to be a local response of the cells to the injury.

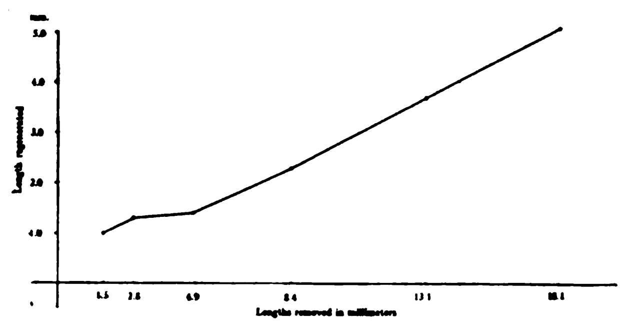

FIG. 1. RANA CLAMITANS. SECOND REGENERATIONS. TEN DAYS

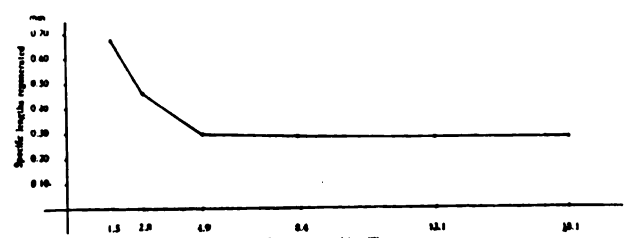

FIG. 2. RANA CLAMITANS. SPECIFIC REGENERATED LENGTHS. TEN DAYS

A complication in the relation between regeneration and level of the cut is introduced by the fact that the regenerated tail is not as long as the original one. A certain per cent only of the removed length is replaced, 40% or less for all except the levels near the tip. Also, the end of the process is reached sooner for the shorter than for the longer removals. From the deepest levels regeneration is still proceeding when it has stopped from the medium and shallowest ones. When all regenerations are completed the specific lengths are therefore slightly greater for the longest removals than for the medium ones.

As to the cause of the greater rates at the deeper levels little more can be said than that it does not seem to be due to inherent differences

in the cells at the different levels. If differentiation in the tail proceeded from the tip toward the base, the more rapid rates from the more basal levels might be explained by the more embryonic character of the cells at these levels. As the tip is approached the material would become more and more highly differentiated and therefore less and less capable of readjustment. There is however no evidence that differentiation proceeds in this way in this case.

The progressive increase in rate with depth of level of the cut is undoubtedly due to reactions which involve a more central control, a coördination of the functional activity as a whole. The period of cell migration probably is only slightly subject to such control. It is a period in which the response is largely local in character. The rate of cell division which is the important factor during the period of rapid increase in length is however undoubtedly under central control.

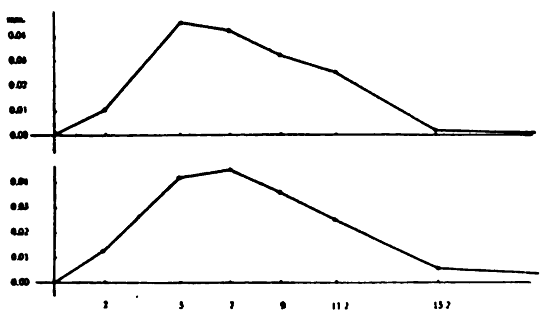

FIG. 3. SPECIFIC RATES OF FIRST AND SECOND REGENERATIONS AT DIFFERENT TIMES AFTER THE OPERATION. RANA CLAMITANS TAIL REGENERATION. UPPER FIGURE = SECOND, LOWER FIGURE = FIRST REGENERATION.

3. *Time within the Regenerative Cycle.*—The present analysis of change in rate of regeneration during the regenerative cycle was made in extension of previous studies which showed that the increase in amount of material during regeneration follows the general rule of increase during an ordinary life cycle. The rate is slow at first, increases very rapidly to a maximum, then declines rapidly at first and then more and more slowly as zero is approached.

The present study deals with tail regenerations in frog and salamander larvae. Large tadpoles of Rana clamitans which remained fairly constant in size during the course of the experiment were found to be the most satisfactory. They yielded results which were uniform enough for an analysis of the change in rate. The second regenerations are taken up here. The levels of removal averaged 1.5, 2.8, 4.9, 8.4,

13.1 and 18.1 mm. from the tip or respectively 6, 10, 18, 31, 49 and 67% of the tail length.

Since the specific regenerated length or length regenerated per unit of removed length is a fair constant for all levels between 4.9 and 18.1 mm. the most reliable data on change in rate are obtained by averaging the specific rates for all the individuals with these levels of injury. The average specific rates per day obtained in this manner are 0.010 mm. for the 0–4 day period, 0.045 for the 4–6 day period, 0.042 for 6–8

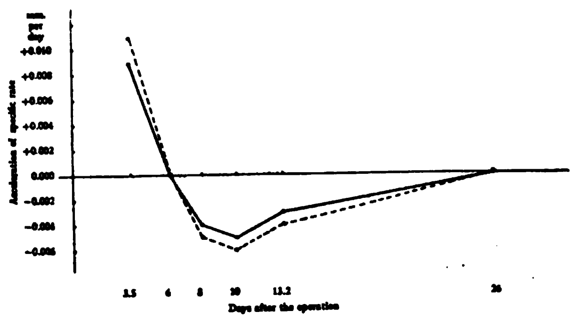

FIG. 4. ACCELERATION OF SPECIFIC RATE. FIRST AND SECOND REGENERATIONS OF THE TAIL IN RANA CLAMITANS. UNBROKEN LINE = FIRST REGENERATION. BROKEN LINE = SECOND REGENERATION.

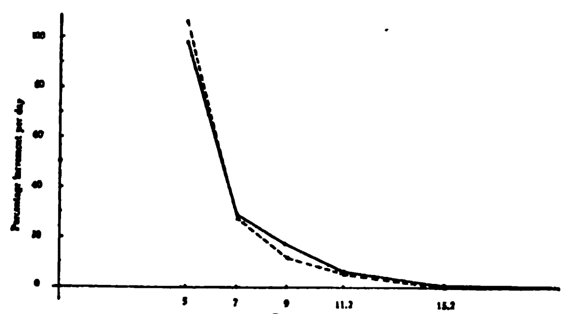

FIG. 5. PERCENTAGE INCREMENT PER DAY AT DIFFERENT PERIODS AFTER THE OPERATION. FIRST AND SECOND REGENERATIONS OF THE TAIL OF RANA CLAMITANS. UNBROKEN LINE = FIRST REGENERATION. BROKEN LINE = SECOND REGENERATION.

days, 0.032 for 8–10 days, 0.025 for 10–12½ days, 0.002 for 12½–18 days and 0.000 for 18–56 days (fig. 3). The specific rate reaches its maximum just before the end of the sixth day.

The values for acceleration of specific rate are +0.011 mm. from the 0–4 to the 4–6 day period, 0.000 from the 4–6 to the 6–8 day period, −0.005 from the 6–8 to the 8–10 day period, −0.006 from the 8–10 to the 10–12½ day period, −0.004 from the 10–12½ to the 12½–18 day period and 0.000 from the 12½–18 to the 18–56 day period (fig. 4).

The only plus value comes between the first two periods and the lowest values are between the 8–10 and the 10–12½ day periods.

An examination of these values and a comparison with the facts of histogenesis shows that acceleration of rate is a plus quantity only during the period before active differentiation of the cells has begun. The retarding effect is evident with the beginning of apparent tissue differentiation and by the ninth to eleventh days the negative acceleration is at its height.

The percentage increments for the six periods represented are respectively 106, 28, 12, 5, 0 and 0 (fig. 5). There is a very rapid decrease at first and then a slower and slower one as zero is approached. The data agree with those of ordinary growth.

First regenerations of frog tadpoles gave results which were essentially similar to those for second regenerations.

The data will appear in full in the *University of Illinois Biological Monographs*.

PRELIMINARY NOTE ON THE DISTRIBUTION OF STARS WITH RESPECT TO THE GALACTIC PLANE

By Frederick H. Seares

MOUNT WILSON SOLAR OBSERVATORY, CARNEGIE INSTITUTION OF WASHINGTON

Communicated by G. S. Hale, February 9, 1917

A significant feature of the distribution of stars over the face of the sky is their concentration toward the plane of the Galaxy. Approaching the Milky Way from either side, we find that objects of all degrees of brightness become more and more numerous; with decreasing galactic latitude, the star-density regularly increases and attains a maximum in the star clouds of the Milky Way itself, a fact long known and, as early as 1750, the basis of cosmological speculation by Thomas Wright of Durham.[1] The phenomenon was studied by both the Herschels, and more recently Seeliger, Celoria, Pickering, Kapteyn,[2] and Chapman and Melotte,[3] among others, have given values of the stellar density; and yet, from a numerical standpoint, the matter remains even now more or less an open question.

Our knowledge of stellar distribution must include the total number of stars per unit area at each galactic latitude, and, as well, of the number for each interval of magnitude from the brightest to the faintest; moreover, the magnitudes themselves must be homogeneous and in accordance with a uniform scale. The Herschel counts, giving only totals to a certain limit near the 14th magnitude, do not satisfy these

conditions. Seeliger's results, based mainly on the great *Bonner Durchmusterung* catalogues of nearly half a million stars, though affording results for graduated intervals of brightness, do not go below the 10th magnitude. Celoria's counts include fainter objects, but, like those of the Herschels, are only totals for a certain range of magnitude.

To revise these results and extend them to the stars within reach of modern telescopes, Kapteyn, in 1908, discussed all the reliable data then available. His magnitudes for the fainter stars are on the visual scale of Parkhurst,[4] and to the 15th magnitude depend on photometric standards. Beyond this his tables of distribution are extrapolated, but the changes with increasing magnitude are so regular that his values should be reliable to a somewhat fainter limit, provided the photometric standards are not in error.

The latest study of stellar distribution, depending largely on the excellent photographs secured on the initiative of Franklin-Adams, is also of special interest. Transferring the scale of the Harvard Polar Sequence to thirty of the Franklin-Adams regions by means of intercomparison photographs, Chapman and Melotte derived values of the star-density for magnitudes 12 to 17. Other plates gave results for the brighter stars, which also are referred to the Harvard photographic scale.

A comparison with Kapteyn's results reveals two important facts:

(*a*) Kapteyn's total of stars in the whole sky to specified limits of magnitude is systematically the larger. Approximately 50% greater from the 4th to the 10th magnitudes inclusive, it increases rapidly for fainter limits, and at the 17th magnitude is 7 times that of Chapman and Melotte.

(*b*) With increasing magnitude, the galactic condensation (ratio of star-density at galactic latitude 5° to that at 80°) increases:

Limiting Magnitude.....	5	7	9	11	13	15	17
Kapteyn...............	2.13	2.25	2.82	4.3	7.9	17.5	44.8
Chapman and Melotte...	2.06	2.26	2.69	3.2	3.7	4.1	4.3

To the 9th magnitude there is agreement, but beyond, Kapteyn's values are greatly in excess.

The difference noted in (*a*) is, in part, to be expected, for in one case the magnitudes are visual, in the other, photographic. The number of stars to a specified visual limit is necessarily greater than that to the same limit on the photographic scale, the excess depending on their color. To the 10th magnitude the two results thus agree well enough;

for but lower limits the differences suggest a divergence of the scales, or, perhaps, an increase in the average color of the stars. The faint stars are really redder, at least in certain regions; but, by itself, increased color is inadequate as an explanation. On the other hand, there is evidence that below the 10th magnitude Parkhurst's photometric standards, upon which Kapteyn's scale is based, are increasingly too bright. Kapteyn's tabulated densities therefore properly apply to magnitudes which are fainter, by gradually increasing amounts, than those used as arguments for his tables. The probable amount of the scale divergence is sufficient to account for the relatively rapid increase of Kapteyn's totals, which, in any case, must be associated mainly with the characteristics of the photometric standards; for the supposition that Parkhurst's scale is normal would imply that the fainter stars are very red, and Kapteyn's large totals would then be referable to the high color thus presupposed.

The differences in galactic condensation shown under (*b*) are more difficult to explain. Scale differences between galactic and non-galactic regions, and systematic errors in the counts depending on galactic latitude are factors which obviously might enter. Some information is given by the photographic magnitudes of stars in the Selected Areas now being determined at Mount Wilson.[5] Ultimately these will yield results for graduated intervals of magnitude, but at present only total densities to the limiting magnitude of the plates are available.

ZONE	TOTAL NUMBER OF STARS	LOG MEAN NUMBER STARS	MEAN GAL. LAT.	GAL. LAT.	LOG. N_m MW	MW *minus* C and M	GRONINGEN MAGNITUDE	LOG. N_m KAPTEYN	MW *minus* KAPTEYN
I	13,702	3.10	2°	5°	4.12	+0.74	16.2	4.17	−0.05
II	12,322	3.05	11	15	3.86	+0.57	16.3	3.86	0.00
III	6,010	2.74	19	25	3.53	+0.37	16.2	3.58	−0.05
IV	2,751	2.40	29	35	3.26	+0.29	16.1	3.33	−0.07
V	1,743	2.20	39	45	3.10	+0.28	16.2	3.14	−0.04
VI	1,350	2.09	48	55	2.98	+0.13	16.3[6]	2.97	+0.01
VII	987	1.95	59	65	2.89	+0.08	16.6	2.82	+0.07
VIII	767	1.84	73	80	2.79	+0.04	16.9	2.66[6]	+0.12[6]

The counts, now complete for 88 areas, include nearly 40,000 stars. These have been divided into eight groups according to galactic latitude, as shown in the first four columns of the table. Variations in the limiting magnitude from plate to plate cause some uncertainty; but, since

each group includes eleven areas and the number in each area has been derived from the better of two photographs, the means per field cannot be seriously affected. The fields are small (one ninth of a square degree), but local variations of density have been well eliminated and the mean number of stars increases uniformly with decreasing galactic latitude. With the exception of Zones I and II which include the irregular cloud-forms of the Milky-Way, the deviations from the smooth curve of the plotted data are only 2 or 3%.

For comparison with other results the logarithms of the numbers of stars have been reduced to the latitudes in the fifth column and referred to the square degree as unit. The resulting logarithms of the star-density (number of stars per square degree brighter than the limiting magnitude) are in the sixth column.

The zero point corrections of the Mount Wilson results have not yet been found, but since Pickering has determined the visual intensity of the central star of each Selected Area, the approximate limiting magnitude is known. His data fix the zero point and indicate a mean apparent limit of 17.1, but, to obtain the real limit on the photographic scale, this must be increased by the average color index of the central stars. Adopting 0.6 as the average color, we have 17.7; the counts are not complete, however, for the last few tenths of a magnitude, and we accept provisionally 17.5 photographic as the limit of the Mount Wilson densities.

Since the results of Chapman and Melotte do not extend below the 17th magnitude, their data used for the comparison in the seventh column of the table are for this limit. The Mount Wilson densities, referring to 17.5, should be the larger by approximately 50%. At 80° the excess is only 10%, but increases rapidly toward the lower latitudes, and at 5° the Mount Wilson value is 5.5 times that of Chapman and Melotte. This divergence is remarkable and, in its implications, serious.

In comparing Mount Wilson and Groningen results, Professor Kapteyn has called my attention to the importance of using the same limiting magnitude for both series of counts, in order to avoid the disturbing effect of the rapid change in galactic condensation with magnitude previously alluded to. The magnitude on the Groningen scale corresponding to the Mount Wilson limit can be interpolated from Kapteyn's tables with the Mount Wilson values of log N_m as argument. The results for each zone of latitude are in the eighth column of the above table. The agreement, excepting for the last two values is close and indicates a very satisfactory homogeneity in the counts. The mean

of 16.3 is accordingly the Groningen visual magnitude corresponding to the Mount Wilson limit, provisionally placed at 17.5 photographic. Of the difference between these quantities, perhaps half a magnitude is to be attributed to the color of the stars; the remaining 0.7 or 0.8 mag. is the indicated error of the Groningen scale, and agrees with what was to have been anticipated from evidence relating to the photometric standards of Parkhurst.

Interpolating now from Kapteyn's tables for magnitude 16.3, we obtain the densities in the ninth column, and, finally the differences in the last column. These are in nowise affected by the outstanding errors in the scales, and, though systematic, are very small. The values of the galactic condensation for the limiting magnitude here considered are, respectively, Kapteyn, 32; Mount Wilson, 21.4.

The relation of the Groningen and Mount Wilson scales has been derived on the assumption that equal densities correspond to the same limit of brightness. Were the scales of the same kind—both photographic or both visual—the difference in the limits for equal densities would be their relative error; as it is, the numerical difference in the limits must be apportioned between scale error and the influence of the color of the stars. With the distribution suggested above, the influence of relative scale error would cause Kapteyn's total at the 16th or 17th magnitude for all the stars to exceed by two or three times that indicated by the Mount Wilson counts. His values for the totals are probably to be decreased, though not by the amount suggested by the figures of Chapman and Melotte.

In estimating the significance of the small divergence shown by the last column of the table, which tests the relative homogeneity of the Groningen and Mount Wilson counts, the following should be noted: Kapteyn's values are extrapolated a magnitude or more beyond the limit for which he had photometric standards; and, for many regions, he experienced much difficulty in determining the necessary magnitudes. Further, the Mount Wilson counts are determined by the limiting magnitudes of the photographs, and although well distributed in latitude, variations from zone to zone may affect the totals. The agreement in distribution—even in the values of the galactic condensation, which are unduly influenced by the large deviation near the pole—is therefore very good.

A comparison of Mount Wilson with Chapman and Melotte for corresponding magnitudes might also have been made, but the conclusions which would have followed were obvious, for the large divergence

shown in the seventh column of the table is little affected by differences in the limiting brightness. This divergence indicates that, as compared with both Kapteyn and Mount Wilson, their results are not homogeneous; and noting, further, that the numerous and careful guages of the Herschels which extend to the 14th magnitude give a galactic condensation agreeing closely with Kapteyn (Herschel 12.9, Kapteyn 11, Chapman and Melotte 3.9), one gains the impression that, through some unknown cause, their counts in the richer fields fail to include many of the fainter stars.

[1] Wright, T., *Theory of the Universe*, London, 1750.

[2] Kapteyn, J. C., *Pub. Astr. Lab. at Groningen, Groningen*, No. 18, 1908, (1–54). This memoir includes bibliographical references to earlier investigations.

[3] Chapman, B. A. and Melotte, P. J., *Mem. R. Astr. Soc., London*, 60, 1914 (145–173).

[4] Parkhurst, J. A., Researches in Stellar Photometry, *Washington*, *Carnegie Inst., Pub.*, No. 33, 1906, (1–192). Similar results for the fields of several variable stars have appeared from time to time in the *Astrophysical Journal*.

[5] Seares, F. H., These PROCEEDINGS, 3, 1917, (188–191).

A CORRECTION

Professor Alexander McAdie has kindly drawn my attention to an error in dates occurring in my paper, Inferences Concerning Auroras, published in the Proceedings for January, 1917, pages 1–7. In reference to the "Aurora of April, 1883", he has convinced me from his data that the particular event occurred on November 17, 1882. My mistake was due to faulty references, and in no way affects the general argument.

ELIHU THOMSON.

NATIONAL RESEARCH COUNCIL

RESEARCH COMMITTEES IN EDUCATIONAL INSTITUTIONS

A very large proportion of the scientific research of the United States is conducted in the laboratories of educational institutions. It is now widely appreciated that contact with knowledge *in the making* is the most effective means of seizing and holding the student's attention. And it is also recognized that no greater injury can be done to the cause of science than to compel a promising investigator, fresh from the researches of his graduate years, to relinquish all hopes of further studies because of the complete absorption of his time and energy by other duties.

It is with the fullest appreciation of the difficulties which financial limitations involve, and with a sincere desire not to interfere with the just demands of the teacher's profession, that the National Research Council invites the coöperation of educational institutions in the promotion of research at this critical period in our national progress. We believe it to be feasible, without decreasing the efficiency of the university, the college, or the professional school as teaching institutions, to increase greatly their contributions to knowledge through research. Indeed, we do not hesitate to say that if a portion of the time now given to teaching were devoted to investigation, and if the courses of instruction were so altered as to take full advantage of this change, the educational efficiency of the institutions in question would be materially enhanced. In extending a request for the formation of Research Committees in educational institutions of high standards, which accord serious support to scientific research undertaken by the faculty and advanced students, we beg to call attention to some of the possibilities which lie open to committees of this character.

In view of the importance of encouraging research on the part of members of the faculties of colleges which do not undertake graduate instruction, the invitation of the Council is not limited to universities and other institutions now giving specific recognition to research. It is highly important to encourage competent men to continue the work of research begun in their university career, and a sympathetic Research Committee could help greatly in this respect. Even the existence of such a committee should serve as a valuable stimulus to men who properly look for some measure of encouragement. In small institutions, as an illustration cited below will indicate, powerful support can be given to research by a body of men who genuinely appreciate its significance.

Each Research Committee will doubtless discover its own best method of procedure, adapted to the circumstances of the case. The following suggestions, which embody the results of the discussions of the Council's Com-

mittee on Research in Educational Institutions, may nevertheless be of service in organizing the work of the committees.

(1) It will probably be advantageous to begin by preparing a survey of the research already in progress in the institution in question. This should serve to indicate the possibilities of extending existing work, and point out favorable opportunities for initiating new lines of investigation.

(2) The Research Council will shortly undertake the preparation of a National Census of Research, indicating the equipment for research, the men engaged in it, and the lines of investigation pursued in government bureaus, educational institutions, research foundations, and industrial research laboratories. The purpose of the Census is to provide data for the effective development of research in pure science and in the industries, as well as for strengthening the national defense. The various Research Committees in educational institutions can aid the Council materially in securing data for the Census, and in supplying information for annual surveys of the progress of scientific research in the United States.

(3) One of the great problems of research laboratories is to find suitably trained men to carry on their work. Nearly all of these men come from educational institutions, where every available means should be used to increase the supply. If research is encouraged on the part of faculty members, and if its national importance is frequently impressed upon the students, more of them will be impelled to follow the career of investigators. The tendency toward narrow specialization, so common at present, should be counteracted by developing more interest in science as a whole. Lectures on the history of science, and broad courses on evolution, covering its various aspects, from the constitution of matter and the evolution of stars and the earth, to the rise of man and the development of civilization, should be widely encouraged. From the purely educational viewpoint such courses may be expected to produce a more favorable influence and leave a more lasting impression than routine discussions of the minutiae of the various branches of science, though the latter are obviously essential in the training of the investigator.

(4) The Council wishes to develop a wider appreciation of the part which men of science may play in researches bearing both on industrial progress and national defense, including those of ship design, aeronautics, the fixation of nitrogen, and many other subjects. Various committees of the Council will soon be prepared to furnish information regarding such research problems.

(5) The development of more general coöperation and coördination in research, within each educational institution and in alliance with other workers outside, is another important subject for consideration. It is essential to remember, however, the necessity of safeguarding the personal freedom and the individual initiative of all investigators.

(6) The interchange of research workers, especially to secure for the smaller

institutions the stimulus given by leaders of research, should be strongly encouraged.

(7) The establishment of a large number of research fellowships, each yielding one thousand dollars or more annually, is very desirable. If students showing special aptitude in their work for the doctor's degree could thus be enabled to devote themselves to research for a year or more, their future career as investigators might be assured. Research fellowships may be conferred by colleges on graduates who have taken their doctor's degree elsewhere, or used to secure the services of non-graduates in research laboratories.

(8) The time is also opportune to secure the establishment of research professorships and research endowments. The present appreciation of the national importance of research, and the increasing sense of personal obligation to the state, will cause men of means to contribute more freely than ever before.

(9) Most important of all is the encouragement of the *spirit* of research, and the development of a sympathetic atmosphere in which the investigator can work to the best possible advantage.

Large institutions should easily be able to extend their research activities, but smaller ones may encounter greater difficulties. As a practical example of what can be done by small institutions in the promotion of the objects of the National Research Council, some results accomplished since June by Throop College of Technology, at the direct instigation of the Council, may be cited. The steps it has taken in connection with the work of the Council are as follows:

Passage by the board of trustees of a resolution endorsing the objects of the Research Council and promising coöperation and of a second resolution providing that in the event of war with a first-class power all available research men and facilities required for the solution of problems of national defense or public need may be counted upon by the Research Council.

Provision of a new fund of two hundred thousand dollars as an endowment for research in physics.

Appointment of Dr. Robert A. Millikan as Director of the Physical Laboratory (under an arrangement with the University of Chicago by which he is to spend a part of each year in Pasadena).

Organization of a coöperative attack on electron problems from the physical, chemical, and astronomical standpoints, in which the physical and chemical laboratories of Throop College and the Mount Wilson Solar Observatory will take part.

Provision of three research fellowships, yielding one thousand dollars each annually, to be awarded to men who have shown exceptional ability in their research work for the doctor's degree. (Beloit College has also established, for a period of five years, a research fellowship yielding one thousand dollars annually.)

Provision of a wind tunnel and well equipped aerodynamical laboratory for researches on the structure of aeroplanes.

Participation in a coöperative arrangement permitting the repetition at Throop College of Professor Michelson's experiment on the tides within the body of the earth, to determine the possible influence of oceanic tides, and to serve as a part of the general study of Pacific Ocean problems undertaken by a committee of the National Academy of Sciences.

CENTRAL COMMITTEES ON RESEARCH

The National Research Council, with the coöperation of the American Association for the Advancement of Science, the American Chemical Society, the American Physical Society, the American Mathematical Society, and other national scientific societies, has established a series of central committees to organize research in the various branches of science.

The purpose of these committees may be outlined as follows:

(1) To join in the preparation of the National Census of Research. This will be taken by the Census Committee of the Research Council, of which the Chairmen of the various central committees are members.

(2) To prepare reports embodying comprehensive surveys of the larger possibilities of research in the various departments of pure science, suggesting important problems and favorable opportunities for investigation.

(3) To survey the economic and industrial problems of the United States, and report on possible means of aiding in their solution by the promotion of research in the fields represented by the various committees. (In coöperation with the Council's Committee on the Promotion of Industrial Research.)

(4) To indicate how investigators in each committee's field can aid in the solution of research problems involved in strengthening the national defense. (In coöperation with the Military Committee of the National Research Council.)

(5) To point out opportunities, national and international, for coöperation in research, and to assist in the coördination of the various agencies already established for this purpose.

(6) To keep in touch with the Research Committees of educational institutions, and to supply research problems, suggestions, or thesis subjects when requested to do so.

(7) To serve as a national clearing house of information regarding research problems in each committee's field which arise from scientific, industrial, and other sources, and are communicated to the Council by local Research Committees or other agencies.

(8) To promote research by such other methods as may prove advisable, including the encouragement of such courses of instruction in educational institutions as are best adapted to develop greater breadth of view, a wider understanding of the methods of research, and a more general perception of the national importance of all forms of research, both in pure and applied

science; the more effective use of existing research funds; the establishment of research fellowships, research professorships, and research endowments.

All reports of the National Research Council and of its committees are published in full in these PROCEEDINGS, through which members of the separate committees may keep in touch with the work in progress in all its various fields.

GEORGE ELLERY HALE, *Chairman*.

NATIONAL RESEARCH COUNCIL

REPORTS OF MEETINGS OF THE EXECUTIVE COMMITTEE

The seventh meeting of the Executive Committee of the Research Council was held at the office of the Council, New York City, on December 18, 1916. Messrs. Carty, Chittenden, Conklin, Dunn, Noyes, Pupin, Stratton, Vaughan, Welch, and the Secretary were present; also Mr. M. T. Bogert, Chairman of the Chemistry Committee.

The Secretary reported that the President of the Academy had appointed Howard E. Coffin, William M. Davis, and Franklin H. Martin, members of the Council; and that the Chairman of the Council had appointed R. H. Chittenden and R. A. Millikan members of the Executive Committee.

A communication from the Chairman of the Council was presented stating that the Massachusetts Institute of Technology, Yale University, the University of Chicago, Northwestern University, and Throop College of Technology, had appointed Research Committees, constituted as follows:

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

Richard C. Maclaurin, *president;* John R. Freeman, Francis R. Hart, Everett Morss, A. D. Little, C. T. Main and Jasper Whiting, *from the Corporation;* Charles R. Cross, Harry M. Goodwin, Arthur E. Kennelly, Warren K. Lewis, Waldemar Lindgren, Arthur A. Noyes, Joseph C. Riley, George C. Whipple, and Edwin B. Wilson, *from the Faculty;* and George E. Hale and Willis R. Whitney, *from the Alumni.*

YALE UNIVERSITY

Arthur T. Hadley, *president;* Harry G. Day and John V. Farwell, *from the Corporation;* Ernest W. Brown, Russell H. Chittenden, Treat B. Johnson, James F. McClelland, Ernest F. Nichols, and C. E. A. Winslow, *from the Faculties;* and Edwin M. Herr and William W. Nichols, *from the Alumni.*

UNIVERSITY OF CHICAGO

Harry Pratt Judson, *president;* Harold H. Swift, Julius Rosenwald, and Martin A. Ryerson, *from the Board of Trustees;* R. R. Bensley, Thomas C. Chamberlain, John M. Coulter, Albert A. Michelson, Robert A. Millikan, Eliakim H. Moore, and Julius Stieglitz, *from the Faculty;* and Raymond Bacon and Frank B. Jewett, *from the Alumni.*

NORTHWESTERN UNIVERSITY

James A. Patten, William S. Mason, Irwin Rew, and Theodore W. Robinson, *from the Board of Trustees;* Henry Crew (chairman), D. R. Curtiss, Phillip Fox, U. S. Grant, William A. Locy, A. B. Kanavel, A. I. Kendall, John H. Long, S. W. Ranson, John H. Wigmore, O. H. Basquin, Thomas L. Gilmer, and Arthur D. Black, *from the Faculties;* and Charles H. Mayo and W. C. Danforth, *from the Alumni.*

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James A. B. Scherer, *president;* Arthur H. Fleming, George E. Hale, and Henry M. Robinson, *from the Board of Trustees;* Walter H. Adams, George A. Damon, Robert A. Millikan, Arthur A. Noyes, R. W. Sorensen, Franklin Thomas, and Harry C. Van Buskirk, *from the Faculty;* and Joseph Grinnell and Frank B. Jewett, *from the Alumni.*

It was voted to approve the organization of subcommittees in Chemistry as recommended by the Chemistry Committee, and to empower that committee and the other science committees to establish and appoint subcommittees, at their discretion.

The eighth meeting of the Executive Committee was held in New York City on December 27, 1916. Messrs. Carty, Conklin, Dunn, Noyes, Pearl, Vaughan, and Welch, and the Secretary were present.

It was reported by Mr. Noyes that the Committee of One Hundred on Scientific Research of the American Association for the Advancement of Science had adopted on December 26 the following resolutions:

VOTED that the Committee of One Hundred on Scientific Research of the American Association for the Advancement of Science coöperate with the National Research Council in those research movements in which both organizations are interested; and, especially, in order to avoid the duplication of effort that might arise from individual action of the two groups of committees which have already been appointed in the different sciences, that the Committee of One Hundred of the Association coöperate with the National Academy of Sciences and the national scientific societies in the formation of single committees in the various branches of science.

VOTED that, in order to effect the coöperation provided for by the previous vote, the Committee of One Hundred on Research designate, through its chairman, members of the Association to serve as members of the research committees of the National Research Council devoted to the various branches of science, with the understanding that these committees will in general consist of members designated in equal or approximately equal number by the Association, the National Academy of Sciences, and the national scientific society representing the branch of science involved.

It was noted that, in response to the request of the National Canners Association, the names of scientific men be suggested to the Association, who might be appointed by it as an advisory committee with reference to the investigations to be carried out on the toxic relations of canned foods.

It was voted that the Agriculture Committee be constituted as recommended by the Chairman of the Committee, subject to the approval of the Secretary of Agriculture in the case of those who are connected with the Department of Agriculture.

It was reported that the Alumni Council of the Massachusetts Institute of Technology had appointed a Committee for the Mobilization of Technology Resources, for the purposes of "stimulating research at the Institute, tabulating the various researches that have been or are being conducted by alumni, and suggesting, both to the alumni for their consideration and to the faculty as subjects for graduating theses, lines for experiment and research which should be of the greatest immediate value." In addition to these research functions, this committee is to promote the participation of the alumni in movements relating to the national defense and the industrial development of the country. The committee has twenty-seven members, including an executive committee of five, consisting of I. W. Litchfield, Merton L. Emerson, James P. Munroe, Raymond B. Price, and Charles A. Stone. The Executive Committee of the Research Council had adopted at a previous meeting the following resolution expressing appreciation and willingness to coöperate with the Alumni Council:

VOTED that the National Research Council express its appreciation of the proposed plan of the Alumni Council of the Massachusetts Institute of Technology for stimulating interest in research among its alumni and promoting the further introduction of scientific investigation in the industries with which they may be connected, and its willingness to coöperate with the Alumni Council in any way that may prove desirable and practicable.

The ninth meeting of the Executive Committee was held at the Smithsonian Institution in Washington on January 6, 1917. Messrs. Carty, Dunn, Noyes, Stratton, Walcott, Welch, and the Secretary were present; also, by invitation during the latter part of the meeting, Mr. H. E. Coffin and all the military members of the Research Council, namely, Messrs. William Crozier, J. D. Gatewood, W. C. Gorgas, R. S. Griffin, G. O. Squier, and D. W. Taylor.

The Secretary presented a resolution adopted by the Associated Business Papers, Inc., endorsing the work of the Research Council and promising assistance in giving to its work proper publicity in the trade journals of the country.

It was voted to appoint J. M. Clarke chairman of the Geology Committee, W. H. Holmes chairman of the Anthropology Committee, and E. H. Moore chairman of the Mathematics Committee.

It was voted that the Military Section of the Council be hereafter known as the Military Committee its functions being further defined, when appropriate, by adding the clause "For Governmental Research Requirements," and that the committee consist of the six official representatives of the Army and Navy, of the other government appointees residing in Washington, of Mr. H. E. Coffin member of the Research Council and of the Advisory Commission of the Council on National Defense, and of such other persons as may be added by the Military Committee or the Executive Committee. C. D. Walcott, Secretary of the Smithsonian Institution, was elected chairman of the Military Committee; and (at a subsequent meeting of that committee)

S. W. Stratton, Director of the Bureau of Standards, was elected its secretary. It was voted that an appropriation of twelve hundred dollars, payable in monthly installments, be made for the use of the Military Committee for clerical work and other expenses, including the salary of a clerical secretary.

CARY T. HUTCHINSON, *Secretary.*

VOLUME 3 APRIL, 1917 NUMBER 4

PROCEEDINGS

OF THE

NATIONAL ACADEMY OF SCIENCES

OF THE

UNITED STATES OF AMERICA

Publication Office: Williams & Wilkins Company, Baltimore
Editorial Office: Massachusetts Institute of Technology, Cambridge
Home Office of the Academy: Washington, D. C.

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PROCEEDINGS

OF THE

NATIONAL ACADEMY OF SCIENCES

Volume 3 APRIL 15, 1917 Number 4

A RE-DETERMINATION OF THE VALUE OF THE ELECTRON AND OF RELATED CONSTANTS

By R. A. Millikan

RYERSON PHYSICAL LABORATORY, UNIVERSITY OF CHICAGO

Communicated, February 5, 1917

This re-determination of the most fundamental of physical constants was entered upon three years ago for three reasons.

First, in 1913 results began to be published from Vienna,[1] which though obtained by a modification of my method,[2] were wholly irreconcilable with those which I had found; and I accordingly wished to see whether I could find conditions under which the method failed.

Second, there developed a tendency, especially among British physicists, to adopt a value of e about 2% lower than that which I had obtained, and as this difference was much greater than the necessary error in my method I was anxious to see, by entirely new work, whether a numerical error could have crept into the former determination.

Third, the electron has recently taken on added importance because it has been found to carry with it not merely all molecular and atomic magnitudes, as heretofore, but also all of the most significant of the radiation constants, such as Planck's h, the Stefan-Boltzmann constant σ, the Wien constant C_2, all X-ray constants, i.e., the wave lengths of characteristic X-rays, etc.[3] It seemed worth while therefore to drive my method, which is certainly exceedingly exact if its validity is granted, to the utmost limit of its possible precision.

The method is the same as that used in the preceding determination,[4] but the apparatus is new throughout and every constant entering into the value of e has been redetermined with increased care and precision. The condenser plates MN (fig. 1) consist of two optically flat brass surfaces 22 cm. in diameter, held apart by three small pieces of echelon plates about 1 cm. square and 14.9174 mm. thick placed at points 60°

apart about the circumference. The dimensions of the condenser therefore now introduce an error of no more than 1 part in 10,000. The oil droplets from the atomizer *A* blown by a puff of air through *r* entered the condenser *MN* through 5 minute holes 0.25 mm. in diameter in the middle of the upper plate and were observed by means of light from the arc *a*, filtered through a trough of water *w* and one of cupric chloride *d* for the removal of heat rays. The temperature was held constant to within one or two hundredths of a degree by the oil-bath *G*. The charge on the drop *p* was changed by X-rays from the bulb *X* passing through the window *g*. The pressure was varied from 13 cm. to 76

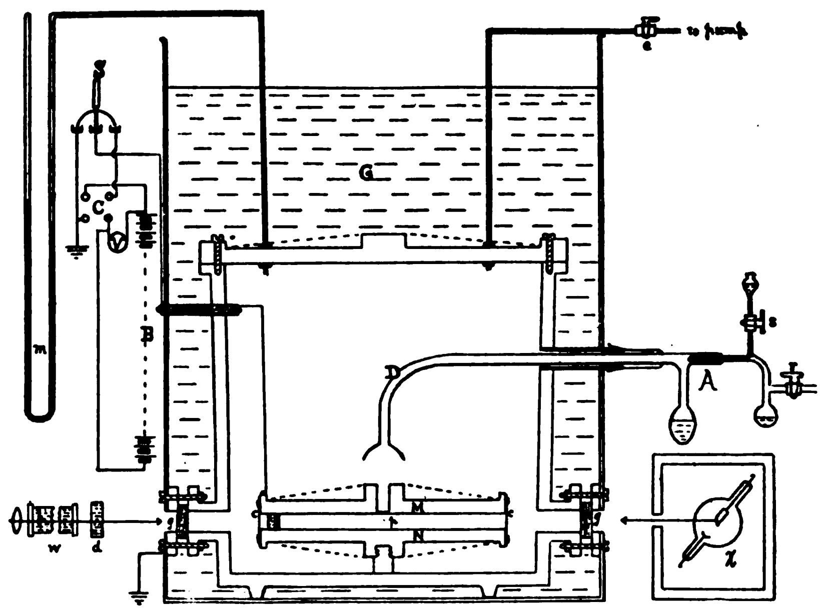


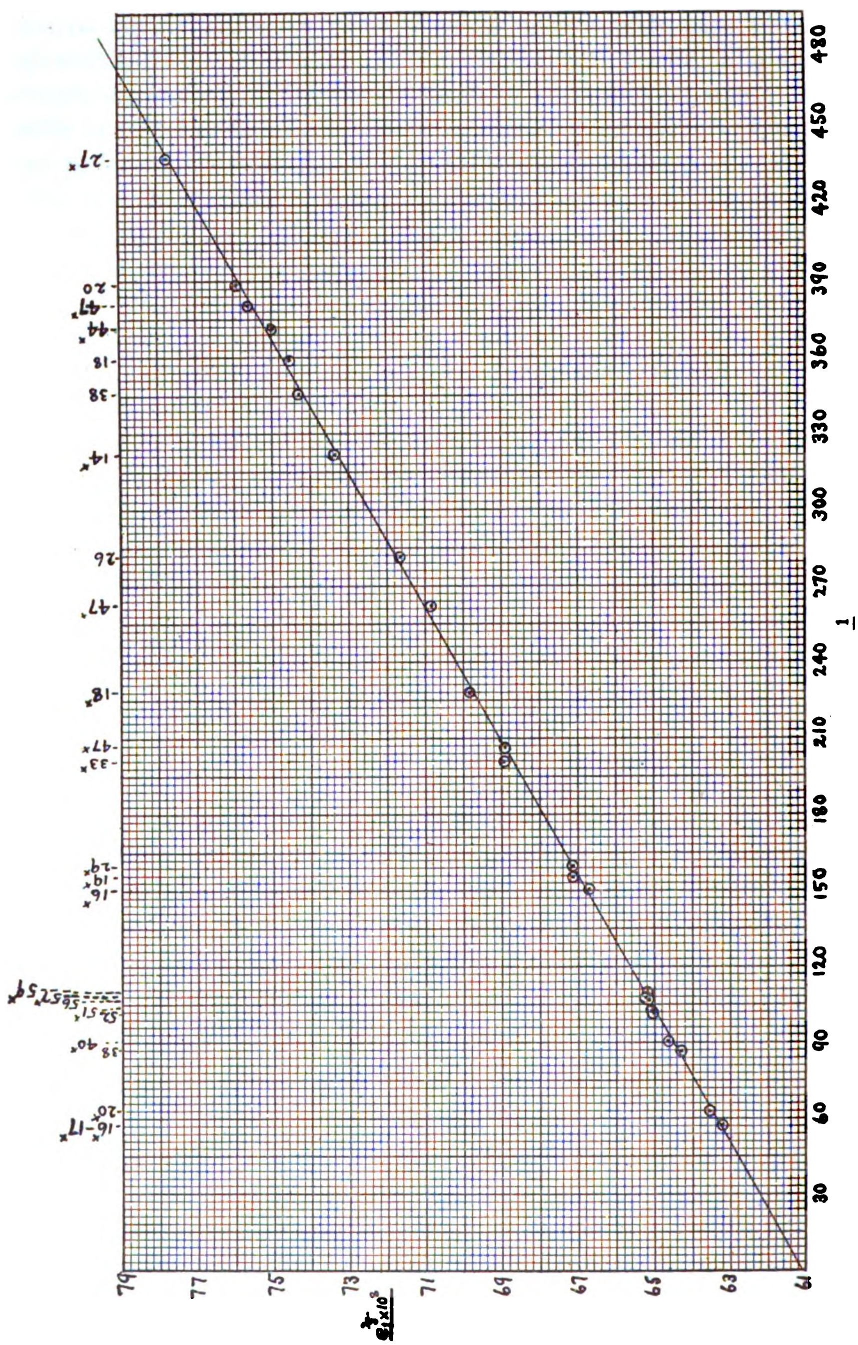
FIG. 1.

cm. and was measured to 0.1 mm. by the manometer *m*. The atomizer *A*, fed from *s* with the highest grade of watch-oil, density at 23°C. redetermined as 0.9199, was blown with carefully dried and cleaned air let in through the cock *r*, the bulbs below *A* being to catch excess oil. The observing optical system was a specially constructed telescope of 30 mm. objective and a magnification of 25 diameters. The distance through which the drops were timed (cross hair distance) was 1.0220 cm. correct to 1 part in 2000. The velocities of the drops were measured with a most convenient and reliable printing chronograph made by Wm. Gaertner & Company of 5545 Lake Avenue, Chicago, and kindly loaned to the laboratory for this determination. It prints

directly the time of pushing a key, in terms of the indications of a standard clock, with an error which is never more than 1/100 second. The times of fall and rise were from 14 to 60 seconds. This makes the error in the mean of a series of time determinations a wholly negligible quantity. The electric field strengths were measured to 1 part in 3000, by a 750 volt Weston standard laboratory voltmeter calibrated at frequent intervals during the experiment against 3 standard Weston cells. The coefficient of viscosity of air was redetermined with extraordinary precision by Dr. E. L. Harrington who, using the constant deflection apparatus designed by Dr. Gilchrist and myself, succeeded in introducing such improvements in conditions and perfections in detail as to make his final value altogether unique in its reliability and precision.[5] In view of this and other work now in progress with the same apparatus it is hardly possible that the correct value of η for dry air at 23°C. can be more than 1 part in 2000 removed from the value 0.00018227. This is within less than 0.1% of my former value, viz., 0.0001824.[6] All the other elements of the problem have been looked to with a care which is the outgrowth of six years of experience with measurements of this kind.

That portion of the investigation which has had to do with the testing of the general validity of the method has been reported in detail elsewhere.[7] Suffice it to say here that I find no indications whatever that, when properly used, it ever fails, or that it ever even remotely suggests the existence of a subelectron.

The precision of the method is sufficiently attested by the consistency of the results on different drops, provided no constant error inheres in the measurement of the dimensions of the condenser, the volts, the time, or the viscosity of air. The extent of this consistency is shown in the figure and the table which present the observations on 25 consecutive drops taken with all possible precautions during a period of several months. The data on these drops are treated precisely in the manner adopted in the 1913 article. A more detailed presentation of this work will be published elsewhere. It will be seen from the table that the final mean value of $e^{\frac{2}{3}}$ is 61.126×10^{-8}. *There is but one drop in the table which yields a value of $e^{\frac{2}{3}}$ differing from this by as much as one-third of one per cent and the probable error of the mean computed by least squares is one part in 4000.* This value is 0.07% higher than the value 61.086, which I published in 1913. Both values however are computed in terms of 0.0001824 as the coefficient of viscosity of air. The new value 0.00018227 is more reliable than the old and is 0.07% lower *so that the new value of e computed solely from the new data obtained in this re-*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$\frac{1}{pa}$

FIG. 2.

determination is exactly the same as the value published in 1913. The uncertainty in this value should now be no more than 1 part in 1000 for it now contains but two factors which are uncertain by as much as 1 part in 2000, namely the coefficient of viscosity of air and the distance of fall of the drops (cross hair distance). The result of this final work on e may then be stated thus:

$$e = 4.774 \times 10^{-10} \pm 0.005.$$

The values of the most important radiation constants may be found from the value of e as follows:

TABLE

NO.	$\frac{1}{pa}$	$e_1^{2/3} \times 10^8$	$e^{2/3} \times 10^8$	NO.	$\frac{1}{pa}$	$e_1^{2/3} \times 10^8$	$e^{2/3} \times 10^8$
1	57.45	63.21	61.03	14	206.4	68.90	61.11
2	57.5	63.204	61.03	15	200.7	68.97	61.39
3	63.0	63.54	61.16	16	227.8	69.88	61.27
4	86.7	64.27	60.97	17	262.4	70.85	60.94
5	90.6	64.63	61.21	18	281.4	71.60	60.98
6	101.3	65.02	61.19	19	321.4	73.34	61.20
7	102.4	65.07	61.20	20	345.4	74.27	61.22
8	106.3	65.13	61.11	21	359.1	74.54	60.97
9	109.7	65.19	61.05	22	371.5	75.00	60.97
10	107.3	65.21	61.16	23	380.6	75.62	61.24
11	150.6	66.70	61.01	24	388.5	75.92	61.24
12	160.1	67.12	61.07	25	438.3	77.74	61.18
13	155.6	67.14	61.26				

Mean—61.126.

Bohr's theory[8] gives the constant of the Balmer series in hydrogen as

$$\frac{2\pi^2 e^4 m}{h^3} \text{ or } \frac{2\pi^2 e^5}{h^3 \frac{e}{m}}. \tag{1}$$

I have directly determined h photoelectrically[9] with an error of no more than 0.5%, the value from my sodium curve coming out 6.56×10^{-27}. Webster's[10] value found by the method discovered by Duane and Hunt[11] is 6.53×10^{-27}. From the mean of these two recent determinations of h, the foregoing value of e, and $e/m = 1.767 \times 10^7$, the Rydberg constant is found by (1) to be 3.294×10^{15}. Its experimental value known with the great precision attained in all wave length measurements is 3.290×10^{15}. This agreement constitutes most extraordinary justification of Bohr's equation and warrants the use of this spectroscopic data, combined with the foregoing data on e for a most exact evaluation of h. The value thus obtained is

$$h = 6.547 \times 10^{27} \pm 0.011.$$

This value should involve an uncertainty of but 1 part in 600. It will be seen to agree within 1 part in 500 with the value obtained directly from my sodium curve—a value which I estimated correct to only 1 part in 200.

Again the Wien constant C_2 of spectral radiation is given by [12]

$$C_2 = \frac{hc}{k} = \frac{6.547 \times 10^{-27} \times 2.999 \times 10^{10}}{1.372 \times 10^{-16}} = 1.4312 \pm .0030 \text{ cm. degrees.}$$

The estimated uncertainty is obtained from an uncertainty of 1 part in 600 for h and 1 part in 1000 or k. The latest Reichsanstalt value[13] is $C_2 = 1.4300$, while Coblentz[14] obtains from two different modes of approach $C_2 = 1.4322$ and 1.4369. Again from Planck's equations $C_2 = (48\,\pi\sigma k/a)^{\frac{1}{3}}$ and $\sigma = ac/4$ and the foregoing values of k and C_2 computed from e we obtain

$$\sigma = 5.72 \times 10^{-12} \pm 0.034 \text{ watt cm.}^{-2} \text{ deg.}^{-4},$$

which is exactly the result recently found by Coblentz.[14]

A summary of the most important constants the values of which are fixed by the foregoing determination of e is given below with the uncertainty attaching to each.

The Electron	e	$= 4.774 \pm 0.005 \times 10^{-10}$
The Avogadro Constant	N	$= 6.062 \pm 0.006 \times 10^{23}$
Number of gas molecules per cc. at 0°C 76 cm	n	$= 2.705 \pm 0.003 \times 10^{19}$
Kinetic energy of translation of a molecule at 0°C	E_0	$= 5.621 \pm 0.006 \times 10^{-14}$
Change of translational molecular energy per °C	ϵ	$= 2.058 \pm 0.002 \times 10^{-16}$
Mass of an atom of hydrogen	m	$= 1.662 \pm 0.002 \times 10^{-24}$
Planck's element of action	h	$= 6.547 \pm 0.013 \times 10^{-27}$
Wien constant of spectral radiation	C_2	$= 1.4312 \pm 0.0030$
Stefan-Boltzmann constant of total radiation	σ	$= 5.72 \pm 0.034 \times 10^{-12}$
Grating spacing in calcite[15]	d	$= 3.030 \pm 0.001$ A

[1] Ehrenhaft, F., *Ann. Physik, Leipzig*, **44**, 1914, (657); **46**, 1915, (261); also *Physik. Zs. Leipzig*, **16**, 1915, (10).

[2] Millikan, R. A., *Physic. Rev., Ithaca*, **32**, 1911, (349–397); (Ser. 2), **2**, 1913, (109–143).

[3] Millikan, R. A., *Ibid.*, (Ser. 2), **7**, 1916, (353–388); and Webster, D. B., (607).

[4] Millikan, R. A., *Ibid.*, (Ser. 2), **2**, 1913, (109–143).

[5] Harrington, E. L., *Ibid.*, (Ser. 2), December, 1916.

[6] Millikan, R. A., *Ann. Physik., Leipzig*, **41**, 1913, (759).

[7] Millikan, R. A., *Physic. Rev., Ithaca*, December, 1916.

[8] Millikan, R. A., *Ibid.*, (Ser. 2), **7**, 1916, (374).

[9] Bohr, N., *Phil. Mag., London*, **26**, 1913, (1).

[10] Webster, *Physic. Rev., Ithaca*, (Ser. 2), 1916, (599).

[11] Duane, and Hunt, *Ibid.*, **6**, 1915, (166).

[12] Millikan, R. A., *Ibid.*, **2**, 1913, (142).

[13] Mueller, Warburg, et al., *Ann. Physik., Leipzig*, **48**, 1915, (430).

[14] Coblentz, *Physic. Rev., Ithaca*, (Ser. 2), **7**, 1916, (694).

[15] Webster, D. B., *Ibid.*, **7**, 1916, (607).

BODY PIGMENTATION AND EGG PRODUCTION IN THE FOWL

By J. Arthur Harris, A. F. Blakeslee, and D. E. Warner

STATION FOR EXPERIMENTAL EVOLUTION, COLD SPRING HARBOR, NEW YORK

Communicated by C. B. Davenport, January 10, 1917

The recognition by both plant and animal breeders of the great economic value of any character by which superior individuals might be indirectly selected has been father to a widespread conviction that such characters actually exist.

In the main, beliefs concerning distinctive structural peculiarities of the heavier yielding cereals, the bodily dimensions or proportions of the best dairy animals, and the criteria by which the most prolific layers may be selected from the flock without recourse to trap nesting, rest solely on personal experience and judgment, instead of upon measurements and correlations. They have, therefore, little scientific value.

The quantitative work which has been done on both animals and plants indicates that for the most part there is little prospect of the selection of the economically important characters of yield in plants, milk production in cattle or fecundity in poultry on the basis of structural characters unimportant as such but correlated with economically important physiological characters.

In certain cases, the relationship between two physiological characters, one with and the other without economic value, may be much more intimate. The measurements of the correlations may then be not merely of theoretical interest to the physiologist but of very real economic significance.

The relationship between the intensity of yellow somatic pigmentation and egg production in antecedent periods of time may serve as an illustration of such characters.

Working on two years data covering trap nesting records for 309 and 375 White Leghorn birds respectively, we find the following results for the correlation between the percentage of yellow pigment in the ear lobe[1] and total egg production for the preceding year[2]:

$$\begin{array}{ll} \text{For 1913–1914, } r_{yE} = & -0.5816 \pm 0.0253 \\ \text{For 1914–1915, } r_{yE} = & -0.5271 \pm 0.0252 \\ \text{Difference} & 0.0545 \pm 0.0358 \end{array}$$

Within the limits of the probable errors of random sampling, the results for the two years may be considered identical.

Expressing the relationship in terms of straight line prediction equations, represented graphically in Figure 1, we find:—

$$\text{For 1913–1914, } E = 204.754 - 1.459\,y,$$
$$\text{For 1914–1915, } E = 212.058 - 1.416\,y,$$

where E = total eggs laid per year and y = per cent yellow in ear lobes. Since yellow has been recorded in units of 5% range, the actual difference in egg production associated with a difference of one working unit in pigmentation is about seven eggs.

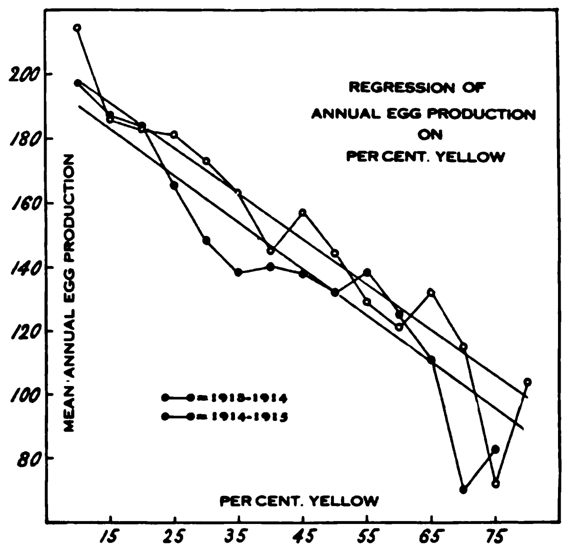

FIG. 1.

The practical significance of the differences in egg production indicated by this equation may be most readily seen from a little table showing the actual mean annual egg production of the birds grouped in classes of 15% range in ear lobe pigmentation.

PER CENT YELLOW IN EAR LOBES	1913–1914		1914–1915	
	Number of birds	Mean number of eggs	Number of birds	Mean number of eggs
10–20	83	187.0	81	185.1
25–35	67	148.2	72	172.2
40–50	111	136.7	119	148.4
55–65	46	132.1	90	126.9
70–80	2	76.5	13	111.5

It is clear that by selecting, in October, birds with 10 to 20% yellow it is possible to obtain a group which have averaged over 30 eggs more than the flock as a whole and over 50 eggs above the average of the class with 55 to 65% yellow. These differences are not merely very

great indeed, but the number of birds included in the heavy laying class is sufficiently large for practical selection operations.

The correlations for October pigmentation and egg production for the individual months, represented in Figure 2, are without exception negative in sign. The agreement between the results for the two years is very close indeed.[3]

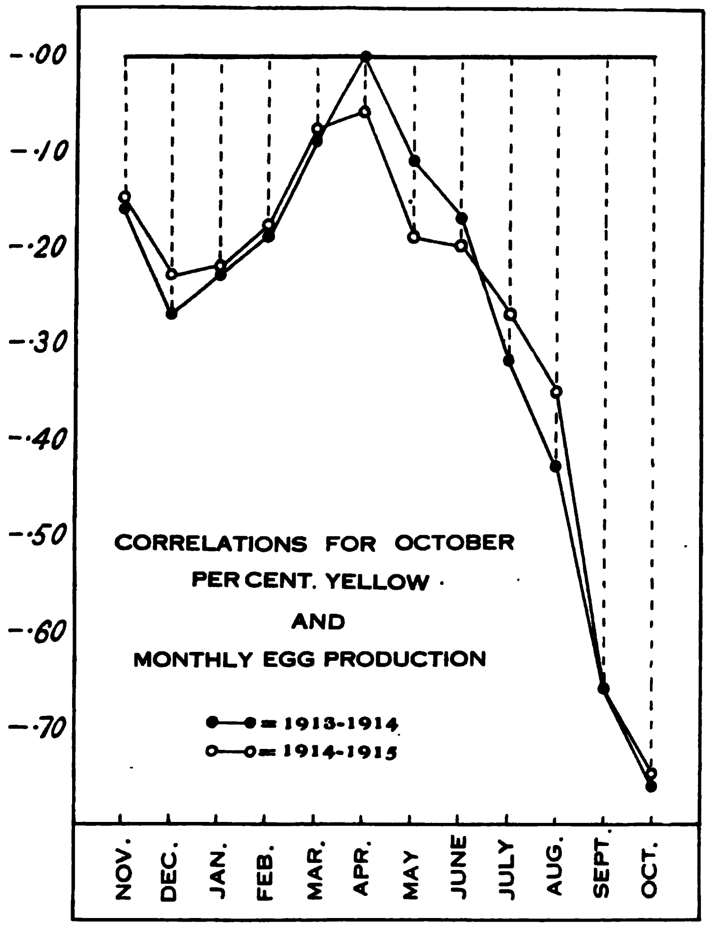

FIG. 2.

Thus there is not merely a strong negative correlation between the October ear lobe pigmentation and the egg production of the year as a whole, but there is a negative correlation between October ear lobe pigmentation and the egg records of each of the preceding twelve months.

The simplest physiological hypothesis to explain the demonstrated negative correlation between egg production and intensity of pigmen-

tation is that the egg yolk in its growth abstracts fat-soluble pigment from the food, thus precluding its localization in the body tissues, or that it actually withdraws the pigment from the tissues.

This view is supported by a number of lines of evidence. First, the correlation between October pigmentation and the egg production of the preceding months decreases (but, as is shown in Figure 2, not regularly) as the time at which egg production is measured becomes more distant from that at which the measurement of pigmentation was taken.

Again, in comparing pigmentation as measured in October of the second year with egg production in November of the pullet year, it is found that pigmentation decreases but slightly and irregularly, though apparently in a linear manner, with increasing egg production. In comparing October pigmentation with October egg production of the same (second) year, one notes that pigmentation decreases very rapidly as one passes from birds which have laid no eggs to those which have laid 1, 2, 3 or more eggs, but that this decrease soon falls off so that birds which have laid over 6 or 7 eggs are apparently sensibly alike in the amount of yellow which they exhibit.[4]

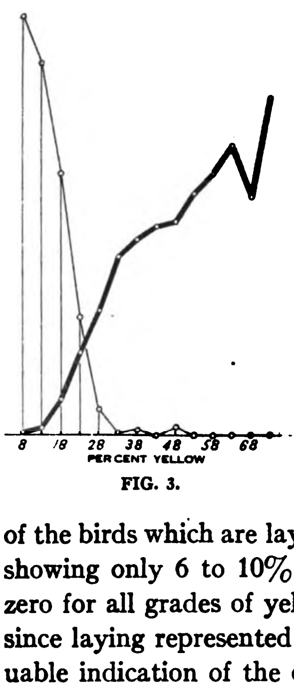

FIG. 3.

The same point may be brought out if a series of measurements be arranged to show the percentage of birds of various pigmentation classes which are 'laying' or 'not laying' at the time the color determinations were made.[5] As shown in Figure 3 the percentage of the birds which are laying falls precipitously from 87.8% among those showing only 6 to 10% yellow (centered at 8% yellow) to practically zero for all grades of yellow above 30%. The average number of days since laying represented by the heavy line in Figure 3, is also a valuable indication of the direct relationship between egg production and pigmentation. Beginning with an average of only 0.4 day since laying in the 6 to 10% color class, the average length of time since laying increases rapidly from birds with smaller to those with larger amounts of yellow pigment.

Finally, the problem may be approached statistically as follows:

If the relationship between percentage of yellow and egg production be chiefly of a physiological nature, it is quite conceivable that the correlations between the October percentage of yellow and egg produc-

tion during the earlier months of the experiment may be largely the resultant of other interrelationships. Consider this possibility in detail.

Let r_{ye_1}, r_{ye_2}, r_{ye_3}, . . . $r_{ye_{12}}$ be the correlations between percent yellow in the twelfth month of the contest and egg production in the first, second, third . . . twelfth months respectively. Further let $r_{e_{12}e_1}$, $r_{e_{12}e_2}$, $r_{e_{12}e_3}$ . . . $r_{e_{12}e_{11}}$ be the correlations between October and November, October and December, October and January . . . October and September egg productions. These constants have been shown to be positive throughout, indicating that birds excelling in egg production in October gave on an average higher productions in every other month of the year.

The application of the well known partial correlation formula for one variable, e_{12}, constant results in very material reductions in the values of r_{ye_1}, r_{ye_2}, r_{ye_3}, . . . $r_{ye_{11}}$. Thus the values of r_{ye_1}, r_{ye_2}, r_{ye_3}, . . . $r_{ye_{11}}$ must be in large part the resultants of $r_{e_{12}e_1}$ and $r_{ye_{12}}$, $r_{e_{12}e_2}$ and $r_{ye_{12}}$, $r_{e_{12}e_3}$ and $r_{ye_{12}}$ . . . $r_{e_{12}e_{11}}$ and $r_{ye_{12}}$.

A discussion of the biological and biochemical literature, and a detailed statistical treatment of the data are appearing in *Genetics*, 1917.

[1] Ear lobe color has been measured in units of 5% range by means of the color top. Only yellow and white sectors were used.

[2] All birds entered the international Egg Laying Competition at Storrs, Connecticut, in November of their pullet year, and remained until the end of October of the following year. Pigment determinations were made near the end of October.

[3] When the constants for the comparable months in the two years are considered in comparison with their probable errors, there is not a single difference which can be considered significant.

[4] The biological inference to be drawn from this result would seem to be that the egg production of a recent period influences very profoundly the concentration of yellow pigment, so that there is a very rapid decrease in yellow pigment for each additional egg laid up to a certain point, beyond which the body pigment is relatively little reduced by extra egg production. Thus for October, the change in pigmentation is to be described by a curve, not by the slope of a straight line. The change in pigmentation is not proportional to egg production, but at first is very rapid and then falls off.

[5] In collecting these data a bird which laid on the day the pigment determination was made or on a later day within the month was considered to be laying, and was recorded in the zero class, *i. e.*, no days since laying. If she laid on the day before the record was taken but not later she is recorded as one day since laying, and so on.

VARIABILITY OF GERM CELLS OF SEA URCHINS

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Communicated by A. G. Mayer, February 6, 1917

As a basis for an understanding of the changes in aging germ cells, it was necessary first to ascertain the normal variability, i.e., the varia-

bility of eggs and sperm removed immediately after receiving the freshly collected sea urchins. For this purpose three different species of sea urchin were studied, namely, *Toxopneustes* and *Hipponoë*, of the shallow tropical waters of the Dry Tortugas, Florida, and *Arbacia* of the deeper colder waters of Woods Hole, Mass.

To obtain optimum conditions as nearly uniform as possible, preliminary experiments were made with each species. These experiments established the optimum concentration of fresh eggs and of fresh sperm for a given surface and volume of filtered sea water. For example about 800 eggs in 10 cc. of sea water, in Syracuse dishes, with 0.05% sperm, with care exercised to avoid evaporation, gave optimum results with *Toxopneustes* and *Arbacia*. For *Hipponoë* the same conditions sufficed except that much greater concentration of sperm was needed.

Having ascertained the optimum and constant conditions it was found that the remaining variability was in large measure a function of the particular male and female used. If cleavage be used as a measure of variability and if the eggs of a female be fertilized by different males, widely varying percentages of cleavage occurred. The female whose eggs gave the highest percent of cleavage by one male, usually gave the highest percent by other males, though the absolute percentage differed considerably. And *vice versa* a low cleavage female usually gave low cleavage by other males. A few experiments are cited to show some of the details (table 1).

With these facts in mind a detailed study of certain variations were made as follows: (a) variations in size and shape of the eggs; (b) variation in the jelly layer of eggs; (c) variation in membrane formation; (d) variation in cleavage.

(*a*) Eggs of a considerable number of females of each species were carefully measured. The eggs of some females varied little from the norm, others varied much, usually by enlargement. One set of readings of two females collected and prepared at the same time is given below.

Ocular readings of diameter of eggs

♀	21	20.5	20	19.5	19	18.5
1	0	0	0	0	33%	67%
2	8%	24%	48%	24%	0	0

Female number 1, varied but little from the norm (19); female number 2 showed considerable enlargement, and variability. Eggs normal or nearly normal in size tended to be all or nearly all globular, eggs showing considerable variation tended to be elliptical.

(*b*) The number of eggs with intact jelly layer also varied in the different females. Some females contained eggs, nearly 100% of which possessed the jelly layer, others as low as 40%, etc. Such reduction was correlated with an increase in size, and an increase in the number of elliptical eggs.

TABLE 1

THE VARIATION IN CLEAVAGE WITH DIFFERENT MALES OR FEMALES

NUMBER OF		PER CENT CLEAVAGE	NUMBER OF		PER CENT CLEAVAGE
Females	Males		Females	Males	
Toxopneustes					
1	1	34	1	1	79
2	1	71	2	1	83
3	1	16	3	1	98
			4	1	99
1	2	24			
2	2	24	1	2	65
3	2	9	2	2	60
			3	2	98
1	3	20	4	2	100
2	3	95			
3	3	33	1	3	61
			2	3	48
			3	3	85
			4	3	81
Arbacia					
1	1	34	1	1	99
2	1	71	2	1	25
3	1	16	3	1	57
1	2	24	1	2	83
2	2	24	2	2	64
3	2	9	3	2	30
1	3	20	1	3	98
2	3	95	2	3	93
3	3	33	3	3	50

TABLE 2

TO ILLUSTRATE THE RANGE OF VARIABILITY IN RATE OF MEMBRANE FORMATION IN FRESH GERM CELLS

NUMBER OF		MINUTES REQUIRED FOR FIRST MEMBRANE TO APPEAR
Females	Males	
1	1	6
2	1	4
3	1	4
1	1	2
2	1	$1\frac{1}{2}$
3	1	$1\frac{1}{2}$
4	1	$1\frac{1}{2}$
1	2	×
2	1	$2\frac{1}{2}$
3	1	$1\frac{3}{4}$
4	1	1
1	1	×
2	1	×
3	1	$2\frac{1}{2}$
4	1	$2\frac{1}{2}$
5	1	×
6	1	×
7	1	$2\frac{1}{2}$

× Signifies that no membrane formed in ten minutes.

(*c*) The rate of membrane formation also varied in different females. In some, the eggs formed membranes within two minutes, in others three to ten minutes, in others not at all. Rapid membrane formation was correlated with slight variability in size, globular shape of eggs, and high percent of jelly layers; slow membrane formation with the reverse conditions. A few examples are cited to illustrate this type of variation, in the eggs of different females fertilized by the same males (table 2).

(*d*) The rate of early cleavage (first and second) and the total cleavage showed even greater variations, from complete or almost complete sterility to 100%. High percent cleavage was correlated with little variation in size, globular shape, high percent jelly layer, and rapid membrane formation. The reverse conditions were associated with low cleavage. In these ways one may separate the so-called 'good' from 'bad' eggs, which morphologically are indistinguishable. A single example of 'good' and 'bad' females are given below:

SPECIES	NUMBER OF MALE	NUMBER OF FEMALE							MAXIMUM DIFFERENCE	TIME CONSTANT
		1	2	3	4	5	6	7		
Toxopneustes.....	1	99	83	98	99				16%	2 hrs.
	2	81	76	18	14	40			72	
Hipponoë........	1	92	97	83					14	
	2	90	54	59	47	16			74	
	1	100	100						0	
Arbacia..........	2	99	98	97	97	98	99	99	7	
	3	81	58	1	77	91			90	

The amazingly large variability in these fresh germ cells finds its explanation in the following facts: (1) There is a primary small variation in fresh eggs of any female. (2) Eggs do not all ripen at the same time but at different intervals, within the body of the female. (3) Injurious changes occur in the eggs from the moment they ripen. (4) The time of elapsing between maturation and removal from the body may be different for different females, and therefore the intensity of the changes will be different for different females. (5) Sea water is also slightly injurious to ripe eggs, and the already different physiologic condition of the eggs is further heightened by the differential effect of the sea water.

Eggs removed from different females at the same time though of the same chronologic age are nevertheless rarely in the same physiologic condition. In order to determine their physiologic state and therefore, in order to separate the eggs according to their physiologic condition, it becomes necessary to ascertain in the manner just described the degree of variation of size, of jelly, and better still of membrane formation, and still better of cleavage, as well as by other means.

It becomes highly probable in view of these facts that the varying behavior of the eggs in the experiments of J. Loeb, F. Lillie, R. Lillie, Wasteneys and others was due in large part to a variation in the physi-

ologic conditions of the eggs which these investigators used. These variations have now been adequately described and measured, and correlated with physiologic condition of the eggs. For experimental work it is absolutely necessary to determine in advance the exact physiologic condition of the eggs, and to use only such, as are nearly in the same condition. We may then hope to obtain more constant and predictable results.

A full account of these experiments will appear in the forthcoming volume of the Researches of the Marine Biological Laboratory of the Carnegie Institution of Washington.

TRANSPLANTATION OF LIMBS

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Read before the Academy, November 14, 1916

The specificity of the tissue of the limb bud in the amphibian embryo has been clearly shown by Braus,[1] who found that when transplanted to any part of the body it would develop into a normal appendage. Since then evidence has accumulated[2] to show that the limb rudiment, and more especially its mesoderm, constitutes an equipotential system.[3] Legs are, nevertheless, rights or lefts and, having no plane or axis of symmetry, the leg of one side of the body cannot be superimposed upon that of the other. In the early embryonic condition, however, there is no visible evidence of laterality, and the question arises when and how this property is determined. Experiments made during the past year have rendered it possible to state more simply than before[4] the rules that govern its determination.

All of the experiments here considered were made with the fore limb of Amblystoma punctatum under precautions necessary to prevent regeneration from the host.[2] In grafting the limb buds three different circumstances relating to their position in the embryo were taken into account—location, laterality and orientation (fig. 1). First, the limb buds were placed either in their natural location in another embryo after removal of the normal bud (orthotopic transplantation) or else in some other region of the body, preferably on the flank of the embryo between the normal fore and hind limbs (heterotopic transplantation). Secondly, some were grafted on the same side of the body as that from which they were taken (homopleural) and others on the opposite side (heteropleural). Thirdly, they were placed either in upright position, with the dorsal border of the transplanted disc corre-

sponding to the dorsal border of the wound (dorso-dorsal), or in the inverted position with the ventral border of the disc at the dorsal border of the wound (dorso-ventral). There were thus eight different possible combinations, all of which were tried in numerous individual cases. Intermediate positions have not yet been experimented with. When the limb bud of one side is implanted on the other, it is obviously impossible to place it in a normal posture, for when the dorsal and

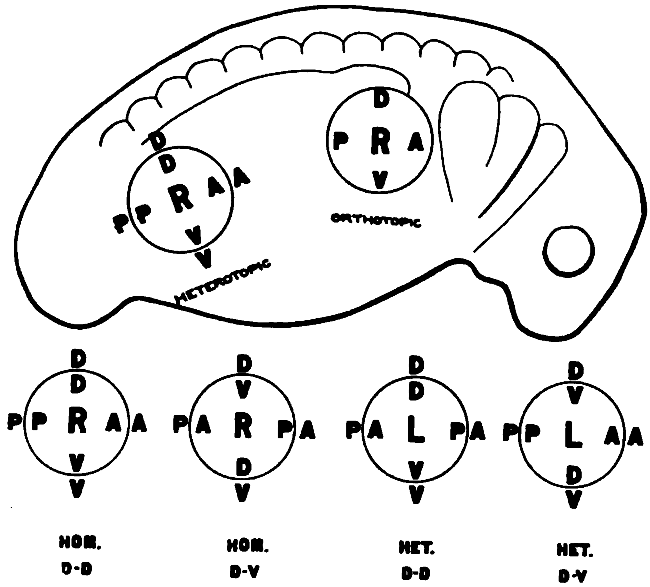

FIG. 1.

Diagram showing the eight different operations. The outline of an Amblystoma embryo in the operating stage is shown above. The circles within it represent the limb bud, in the normal (orthotopic) and the abnormal (heterotopic) location. The four circles below represent the four different ways in which limb buds may be oriented with reference to the cardinal points of the embryo; the letters (*d*, dorsal; *v*, ventral; *a*, anterior; and *p*, posterior) within the circles designate the original cardinal points of the transplanted limb, those outside the corresponding points of the embryo. The operations are represented to be on the right side. *R*, right limb bud; *L*, left limb bud; *hom.*, homopleural; *het.*, heteropleural.

ventral borders correspond respectively the anterior and posterior borders are interchanged and vice versa. Eight further combinations are formally possible by interchange of internal (medial) and external (lateral) surfaces, though they are impracticable, because the mesoderm would thereby be placed on the outside.

The experiments confirm previous ones in showing that the limb bud is a self differentiating body, in so far as it may produce a fore limb

wherever or in whatever position implanted, but they also show that its laterality may be affected by its new surroundings. There are many instances o imperfect development, and of total absorption of the transplanted tissue, especially in the case of the heterotopic transplantations. Also, as has been observed before (Braus,[5] Harrison,[6]) supernumerary appendages often arise after transplantation, just as in regenerating limbs under certain conditions (Tornier,[7] Della Valle.)[8]

In the orthotopic operations the transplanted limb, whatever its form, usually becomes functional, while in the heterotopic this is rarely the case and the function is never perfect. Functional activity, together with the superior potency of the norma surroundings in influencing the development of the limb, renders the results of orthotopic transplantation, in so far as they bear on the problem of laterality, more complicated and difficult to interpret than the heterotopic. Nevertheless the following fundamenta rules underlie the determination of laterality in both cases.

Rule 1. A bud that is not inverted (dorso-dorsal) retains its original laterality whether implanted on the same or on the opposite side of the body.

Rule 2. An inverted bud (dorso-ventral) has its laterality reversed whether implanted on the same or on the opposite side.

Rule 3. When double or twin limbs arise the original one (the one first to begin its development) has its laterality fixed in accordance with the above rules, while the other is the mirror image of the first.

In the heterotopic transplantations the tissue is very often resorbed or the appendages which arise are rudimentary or imperfect (56%). Probably vascularization is not so good and innervation is incomplete. Reduplications were produced in all of the four combinations.

In the orthotopic transplantations only about 18% of the limbs were rudimentary or imperfect, the rest being either fully developed reduplications or normal limbs. Double limbs were obtained frequently in two of the combinations (heteropleural dorso-dorsal and homopleural dorso-ventral) and were very rare in the other two. Other modifications also occurred. The results were as follows:

1. Homopleural dorso-dorsal grafts developed normally though at first very slightly retarded. Rule 1; 6 cases (100%).[9]

2. Homopleural dorso-ventral grafts resulted in (*a*) A single limb of reversed laterality (a structurally and functionally perfect right limb on the left side). Rule 2; one case only (4.3%). (*b*) Reduplicated limbs. Rule 3; 16 cases (69.6%). (c) Typical non reversed limbs which began their development by growing in an abnormal direction,

but ultimately assumed normal posture by rotation. 6 cases (26%). The latter, which constitute the only exception to the rules, require further investigation.

3. Heteropleural dorso-dorsal transplantations yielded: (*a*) Single non reversed limbs. Rule 1; one case only, and that not perfect (3%).

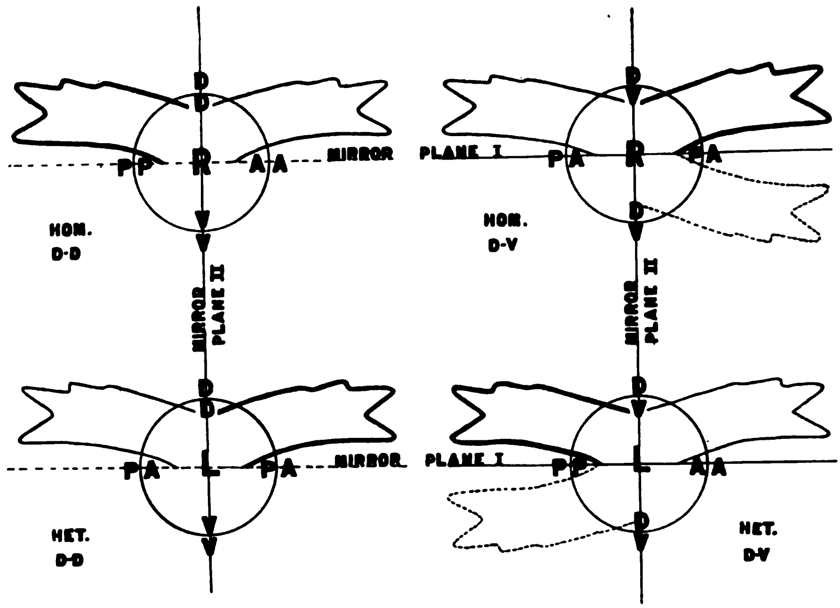

FIG. 2.

Diagram showing the results of the four operations, heterotopic or orthotopic, represented as on the right side of the embryo. The circles indicate the transplanted limb buds, the letters having the same significance as in figure 1. Thus the two upper figures in the diagram represent homopleural, and the two lower ones heteropleural transplantations. The two on the left show the transplanted bud in upright (dorso-dorsal) orientation while the two on the left are inverted (dorso-ventral). The limbs which develop are shown in profile, the ulnar border being uppermost (dorsal) in all which actually develop. A heavy outline indicates the primary member, a light outline the reduplicating one. It is to be noted, however, that the latter develop in by no means all cases while the former may be resorbed in the heteropleural dorso-ventral combination leaving only the reduplicating member present. The broken outlines show the posture that the limb would have assumed, had it developed as a self differentiating member totally independent of the influence of its surroundings.

(*b*) Reduplicated limbs, in which the secondary member, being reversed, has the laterality of its new surroundings. Rule 3; 26 cases (78.8%). (*c*) Cases similar to the above (*b*) in their early development but differing later in that the reduplicating bud gained the upper hand and developed into a normal functioning limb of reversed laterality

(corresponding to its new surroundings); at the same time the original bud became reduced to a small spur or appendage upon the other. Rule 3 modified; 6 cases (18.2%).

4. Heteropleural dorso-ventral transplantations developed into: (*a*) Single limbs of reversed laterality somewhat retarded in their development. Rule 2; 17 cases (94.4%). (*b*) Duplicate limbs. Rule 3; a single case only (5.5%).

The results of the experiments are summed up in diagrammatic form in figure 2. The ulnar border of the limb is at first always dorsal slightly inclined to medial, as in the normal appendage, while the palmar surface is medial slightly inclined to ventral, but the limb may point in either an anterior or a posterior direction. Bending of the elbow or rotation from the shoulder joint may of course modify the positions later in development. The reversal of laterality which takes place is as if the limbs were mirrored in either one of two planes, one of which is horizontal and the other vertical with respect to the main axis of the embryo. When the reversal takes place across the former plane it is complete and no reduplication occurs. It is accomplished immediately or at least very early in development, so that there is little or no outward sign of disturbance and development is but slightly retarded. This plane may be called the primary one. It comes into play when the dorso-ventral axis of the transplanted bud is inverted. When mirroring takes place in the vertical plane, which may be called the secondary one, actual reduplications are found. This occurs in those combinations in which the dorso-ventral axis of the disc is not inverted and it may also occur secondarily following reversal across the primary plane as just described. The twinning of the appendages may, however, be masked, at least in orthotopic transplantations, through the absorption of the original (primary) limb bud (3 c).

The relations of the duplicate limbs change considerably during development, and their definitive position is subject to considerable variation. There may be further reduplication, so that more or less complete triple limbs may result. The three limbs then have approximately the same relations as found by Bateson,[10] especially in arthropods.

In two of the combinations the limbs that develop are in normal orientation with respect to the cardinal points of the embryo. One of these (hom. dd.) is when the graft is placed in the original normal posture, the other (het. dv.) when it is placed upside down on the opposite side of the body. These combinations may be termed harmonic. The other two combinations, the simply inverted bud (hom. dv.) and the bud planted on the opposite side of the body in upright position (het.

dd.), result primarily in limbs that are not normally placed in relation to the cardinal points of the embryo, and may be termed disharmonic.

In comparing the distribution of single and reduplicated appendages in the various groups a remarkable result becomes apparent. In the heterotopic group, where function is excluded, the harmonic combinations yield a relatively large proportion of reduplications (about 54%), while the disharmonic combinations yield a far greater proportion of single limbs (about 87%). In the orthotopic group, on the other hand, the harmonic combinations yield in overwhelming proportion (96%) single limbs which are functional and perfectly normal with respect to their surroundings, while the disharmonic combinations yield about 96% of reduplications.

Experiments with superimposed limb buds and with transplanted half-buds confirm the above results. The harmonic combinations yield single limbs while the disharmonic combinations result in reduplications.

All double formations do not remain as such, however, for it is possible to achieve a normal result from at least one of the disharmonic combinations (het. dd.) by the reduction of the original limb of the pair and the preponderance of the other, which then becomes a normal single limb of reversed laterality (3c) corresponding, therefore, to the laterality of its new surroundings. On the other hand, in the only case in which a single normal limb of opposite laterality (2a) developed in the orthotopic position, it functioned perfectly. This shows that when innervation and vascularization are sufficient, a functional condition may arise which is independent of the harmony of the combination.

It would be premature to discuss the bearing of the experiments upon the question of adaptation. It may be pointed out, however, that the fundamental rules of laterality, as stated above, lead as often to disharmonic as to harmonic results. On the other hand the secondary factors, especially those which determine whether a reduplicated or a single appendage shall arise and those which lead to the resorption of one of the two members of a pair, show a tendency to produce a preponderance of adaptive results. To determine the exact nature of these factors will require further investigation, pending which it seems unwise to appeal to mysticism for the explanation.

[1] Braus, H., *Münchener med. Wochenschr.*, 1903.

[2] Harrison, R. G., these PROCEEDINGS, 1, 1915.

[3] The shoulder girdle rudiment is itself such a system according to Braus, *Experimentelle Beiträge zur Morphologie*, 1, 1909 (400). Also *Morph. Jahrb.*, *Leipzig*, 39, (271).

[4] Harrison, R. G., *Anat. Rec.*, *Philadelphia*, 10, 1916.

[5] Braus, H., *Anat. Anz., Jena*, 26, 1905, (461).
[6] Harrison, R. G., *J. Exp. Zool., Philadelphia*, 4, 1907, (254).
[7] Tornier, G., *Arch. Ent.-Mech., Leipzig*, 20, 1905.
[8] Valle, P. della, *Napoli, Boll. Soc. Nat.*, 25, (1911–12), 1913.
[9] In calculating percentages only those cases which yielded positive results were taken into account.
[10] Bateson, W., *Materials for the Study of Variation*, London, 1894, (479).

THE SHAPES OF GROUP MOLECULES FORMING THE SURFACES OF LIQUIDS

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Communicated by A. A. Noyes, February 2, 1917

In a recent paper[1] I have developed a theory according to which all the forces involved in the structure of solids and liquids, are similar in nature to the forces causing chemical combination. Thus condensation, evaporation, adsorption, cohesion, crystallization, liquefaction, viscosity, surface tension, etc., are manifestations of the forces already known to the chemist. In all these cases the range of the forces is limited to atomic dimensions except in so far as their effects may be transmitted from atom to atom. According to this theory, every atom in a solid or liquid is chemically combined to every adjacent atom. This chemical union may be strong or weak and may be characterized either by primary or secondary valence (Werner).

In most inorganic solid or liquid substances of the strongly polar type, the identity of the molecule is wholly lost, but in organic compounds the groups of atoms constituting the chemical molecule usually have a real existence even in the liquid or solid state. These group molecules are held together by primary valence forces while the forces acting between the group molecules, although no less chemical than the others, are to be characterized as secondary valence forces.

From this viewpoint the forces involved in adsorption and surface tension do not originate from the group molecule as a whole, but rather from certain atoms in the molecule.

This theory leads inevitably to the conclusion that adsorbed films on plane surfaces of solids or liquids should, in general, be one atom or group molecule in thickness. Considerable experimental evidence has already been presented that this is the case with films of gases adsorbed on solids.

Miss A. Pockels[2] showed, in 1891, that very small amounts of oil on the surface of water have no appreciable effect on the surface tension,

but that the surface tension begins to *decrease suddenly* when the amount of oil per unit area is increased beyond a certain sharp limit.

Lord Rayleigh,[3] Devaux,[4] and Marcelin[5] have made similar experiments and have concluded that the amount of oil needed to produce an appreciable effect on the surface tension corresponds to a layer one molecule deep. Devaux finds with triolein that a film $11. \times 10^{-8}$ cm. thick just begins to lower the surface tension of water. From the density and molecular weight of the oil and from Perrin's value of the Avogadro constant, he calculates that the diameter of the molecule would be 11.3×10^{-8} cm. if this be assumed spherical in shape.

According to my theory, however, molecules should not be regarded as spheres, since such a supposition would not be consistent with the chemical nature of the forces. The fundamental question immediately arises: What causes the spreading of an oil upon the surface of water? If we regard molecules as spheres, any attraction between the water and the oil should cause the oil to dissolve in the water instead of spreading on the surface. From the chemical viewpoint, however, the force causing the spreading should be a force between atoms, not between molecules. Evidently, then, some atom or atoms in the oil must have an affinity for the water. Now it is known that the presence of the $-COOH$, $-CO$, or $-OH$ groups in an organic molecule tends to render the substance soluble in water, while the hydrocarbon chain decreases the solubility. On the other hand, hydrocarbons are soluble in each other. Therefore the $-COOH$, $-CO$, and $-OH$ groups have more affinity for water than for hydrocarbons, while hydrocarbons have more affinity for each other than for water.

Thus, when an oil is placed on water, the $-COO-$ groups combine with the water, while the hydrocarbon chains remain combined (secondary valence, of course) with each other. This process leads directly to the spreading of the oil on the surface. If only a limited amount of oil is placed on a large surface, the spreading ceases as soon as all the available $-COO-$ groups have come into contact with the water, for any further spreading would separate the hydrocarbon chains from each other.

According to this conception, pure hydrocarbon oils would not spread on water. Experiment shows that this is actually the case.[6]

The theory furthermore indicates that the hydrocarbon chains in the oils must be placed above the $-COO-$ groups on the surface. Thus, in a series of homologous compounds, as the length of the chain increases the thickness of the oil film should increase in proportion, while the area occupied by each molecule should remain constant.

Now the area occupied by each molecule, which we will designate by a, is readily calculated. If w is the weight of oil placed on the surface and A is the area to which the film must be compressed before the surface tension changes, then the area a covered by each molecule is evidently $a = AM/wN$, where M is the molecular weight of the oil (oxygen = 16), and N is the Avogadro constant (6.06×10^{23} molecules per gram molecule). Furthermore, we may calculate τ the length of the molecule (measured vertically), as follows: the volume of each molecule is $M/\rho N$, where ρ is the density of the oil. By dividing this volume by a, the cross-section of the molecule, we obtain the length of the molecule, thus $\tau = M/a\rho N = W/\rho A$.

To measure A, a new method has been developed. A strip of paper, floating on the water in a long tray, is fastened to a horizontal balance. Oil is placed on the water on one side of the strip only, and leakage of oil past the ends of the strip is prevented by localized blasts of air from two small tubes. Thus the force tending to cause spreading of the oil is measured for different values of A. By plotting these results it is easy to extrapolate to zero force and thus obtain the maximum area completely covered by the oil film.

TABLE 1

CROSS SECTIONS AND LENGTHS OF MOLECULES

SUBSTANCE	M	CROSS SECTION a	$\sqrt{a}$	LENGTH τ
		sq. cm. $\times 10^{-16}$	cm. $\times 10^{-8}$	cm. $\times 10^{-8}$
Palmitic acid $C_{15}H_{31}COOH$	256	24	4.9	19.6
Stearic acid $C_{17}H_{35}COOH$	284	24	4.9	21.8
Cerotic acid $C_{25}H_{51}COOH$	396	25	5.0	29.0
Cetyl alcohol $C_{16}H_{33}OH$	242	21	4.6	21.9
Myricyl alcohol $C_{30}H_{61}OH$	536	29	5.4	35.2
Tristearin $(C_{18}H_{35}O_2)_3C_3H_5$	891	69	8.3	23.7
Cetyl palmitate $C_{15}H_{31}COOC_{16}H_{33}$	482	21	4.6	44.0
Oleic acid $C_{17}H_{33}COOH$	282	48	6.9	10.8
Triolein $(C_{18}H_{33}O_2)_3C_3H_5$	885	145	12.0	11.2
Trielaidin $(C_{18}H_{33}O_2)_3C_3H_5$	885	137	11.7	11.9
Ricinoleic acid $C_{17}H_{32}(OH)COOH$	297	90	9.5	5.8
Linoleic acid $C_{17}H_{31}COOH$	280	47	6.9	10.7
Linolenic acid $C_{17}H_{29}COOH$	278	66	8.1	7.6
Castor oil $(C_{17}H_{32}(OH)COO)_3C_3H_5$	929	280	16.8	5.7
Linseed oil $(C_{17}H_{31}COO)_3C_3H_5$	875	143	11.9	11.0

Table 1 gives the values of a and τ found for a number of different solid and liquid films on distilled water at 16°C. Palmitic, stearic, and cerotic acids all occupy the same area ($24. \times 10^{-16}$ sq. cm.) per molecule. The length of the molecule in the vertical direction (τ) in-

creases about in proportion to the number of carbon atoms in the chain. This is a direct proof that the molecules are oriented on the surface with the COOH groups in contact with the water, and the hydrocarbon groups placed vertically above them. Dividing the value of τ by the number of carbon atoms in these molecules, we obtain an average of 1.19×10^{-8} cm. It is not likely that the carbon atoms in a hydrocarbon molecule are closer to each other than in a diamond, so that we must conclude that the carbon atoms in the chain are not arranged along a straight line, but in a zigzag fashion. The cross-section a is ample for such an arrangement.

The data for tristearin show that the three $-COO-$ groups are all located on the surface of the water and that each occupies the same area as it does in the stearic acid molecule. The three chains are placed side by side. Cetyl palmitate occupies actually less space than palmitic acid. The two chains lie side by side, each having less than one-half the cross-section, but twice the length, that it has in palmitic acid or cetyl alcohol.

These results prove that the areas are determined by the space on the water required by the active groups $-COO-$ or $-OH$ and not by the cross-section of the hydrocarbon chain itself.

The unsaturated acids and esters occupy much larger areas than the saturated ones, and have correspondingly shorter lengths. This is clearly due to the double bond (and the $-OH$ in ricinoleic acid) itself occupying a space on the water. Marcelin[6] has shown that when a globule of oleic acid is placed on a water surface, the film obtained is twice as thick as one which just alters the surface tension of water. Marcelin concludes that such a film is two molecules thick. The above data show that when the thin film of oleic acid is doubled in thickness, each molecule then occupies the same area as does one of stearic acid. Clearly, when an excess of oleic acid is added, the $-COOH$ groups displace the double bonds and the chains assume an erect position on the surface.

These facts prove that the hydrocarbon chain is extremely flexible and has no definite shape of its own. The term 'chain' thus describes its properties admirably.

The above method makes it possible to determine the lengths and cross-section of molecules of non-volatile, insoluble substances. Devaux has shown that oils spread on clean mercury surfaces as well as on water, so that it may be possible to study many substances soluble in water by this method.

For volatile or soluble substances, however, there is another method by which the cross-sections and lengths of molecules in the surfaces of liquids may be determined.

According to Gibbs' equation

$$q = -\frac{c}{RT}\frac{d\gamma}{dc} \tag{1}$$

we may calculate q the amount of material adsorbed per square centimeter in the surface of a liquid, by determining the rate at which the surface tension, γ, changes as the concentration of a dissolved substance, or that of a vapor above the liquid, is altered. It has been pointed out by Milner[7] that when substances strongly depressing the surface tension are added to water, the surface tension varies linearly with the logarithm of the concentration for all except extremely dilute solutions. If we write equation (1) as follows:

$$q = -\frac{1}{RT}\frac{d\gamma}{d\ln c} \tag{2}$$

it is evident that under these conditions q is independent of the concentration. Milner thus calculates from Whatmough's data for acetic acid solutions, that q is 3.8×10^{-10} grams mol. per square centimeter over a rather wide range of concentrations. This should correspond to a monomolecular film. Multiplying the above result by N, we find that it corresponds to $23. \times 10^{13}$ molecules per square centimeter. The area occupied by each molecule is the reciprocal of this, or 43×10^{-16} sq. cm. per molecule. If the whole of this area were covered by a single molecule of acetic acid, the value of τ would be only 2.2×10^{-8} cm. It is therefore probable that the group molecule forming the surface layer contains water adsorbed around the acetic acid group. The polar character of the $-COOH$ group should exert its influence on the CH_3 radical, causing it to pack into the surface layer surrounded by a definite number of water molecules. This hypothesis is in accord with the fact that acetic acid mixes in all proportions with water.

Still more conclusive evidence in support of the new theory is furnished by a paper by Szyszkowski,[8] in which surface tension data for water solutions of propionic, butyric, valeric, and caproic acids are given. The results are found to be given quite accurately by the purely empirical relation

$$1 - \frac{\gamma}{\gamma_0} = b \log_{10}\frac{c+a}{a} \tag{3}$$

where γ is the surface tension of the solution and γ_0 is that of water; a and b are constants. Szyszkowski found that b had the same value, namely 0.411, for all the fatty acids investigated, while the constant a had a different value for each substance. Over a large part of the range of concentrations investigated, a was negligible compared to c so that the empirical equation 3 is equivalent to the relation found by Milner. That is, if we neglect a in (3) and differentiate, we obtain

$$\frac{d\gamma}{d\ln c} = -0.434\,\gamma_0\,b. \tag{4}$$

Comparing this with (2),

$$q = \frac{0.434\,\gamma_0\,b}{RT}. \tag{5}$$

Since b was found to have the same value for all the fatty acids, this equation indicates that the same number of molecules of acid are adsorbed in the surface layer in each case. Placing $\gamma_0 = 73$ dynes per centimeter; $b = 0.411$, $R = 83.2 \times 10^6$ ergs. per degree and $T = 290°K$ we find $q = 5.4 \times 10^{-10}$ gram mols. per square centimeter. This corresponds to a value of a of 30.6×10^{-16} sq. cm. per molecule. This agrees even better with our value of $24. \times 10^{-16}$ sq cm. per molecule for palmitic acid.

Donnon and Barker[9] have measured the amount of nonylic acid adsorbed in the surface of its water solutions and have compared the result with that calculated from surface tension measurements by Gibbs' equation. Both methods gave approximately the same results, averaging about 1.1×10^{-7} grams per square centimeter (independent of the concentration over a wide range). This corresponds to a value of a of 23.7×10^{-16} sq. cm. per molecule, practically identical with the values for the fatty acids given in table 1.

We may therefore conclude that in moderately concentrated solutions of all substances which strongly depress the surface tension, the surface consists of a layer, *one molecule deep*, of the dissolved substance, and that there is never a transition layer in which the concentration varies progressively as we go further from the surface into the solution. The amount of solute required to form the monomolecular layer can be calculated from Gibbs' equation. The length and cross-sections of the molecules forming the surface layer may then be found.

In the case of solutions of inorganic salts, such as NaCl, the surface tension is greater than that of pure water and increases linearly with the concentration. Milner[7] pointed out, according to Gibbs' equation, that there is a deficiency of solute in the surface proportional to the

concentration. Now this is exactly what we should expect if the surface layer of these solutions consists of a single layer of group molecules of water. Milner shows that the deficiency of salt is 4.1×10^{-11} gram molecules of salt per square centimeter for a normal solution ($c = 0.001$). Hence we may place $q/c = -4.1 \times 10^{-8}$.

This result can be explained if we assume that there is a layer 4.1×10^{-8} cm. thick on the surface which contains no salt. This then should represent the length of the water group molecule. It is interesting to note that practically this same value is obtained with solutions of all the different inorganic salts.

Taking the molecular weight of water in the surface to be 18, we find the cross-section of the water molecule to be 7.3×10^{-16} sq. cm.

It is believed that this method of studying the structure of liquid surfaces will prove to be of very general application. The writer is undertaking experiments to measure the cross-sections of molecules adsorbed on water surfaces from vapors. Thus, benzol vapor greatly lowers the surface tension of water, and in this way some knowledge may be obtained as to the shape of the benzol molecules under different conditions. Of course other liquids than water may also be used.

There is a great deal of available data on surface tension of solutions in the literature which can serve this same purpose. I hope to refer to as many of these cases as possible, in a paper on the constitution of liquids which will soon be submitted for publication in the *Journal of the American Chemical Society*.

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THE IMPORTANCE OF THE WATER CONTAINED IN THE DEEPER PORTIONS OF THE SUBSOIL

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Communicated by R. Pearl, February 10, 1917

The most extreme views are entertained as to the importance of the water contained in the deeper portions of the subsoil—that below the depth penetrated by the roots of crop plants. Thus while McGee[1]

considered it to be largely responsible for the crops of extensive areas which would be unproductive if entirely dependent upon the rain and snow which fall upon their surface, and Cameron[2] sees in it the means of indefinitely maintaining the mineral nutrients of the surface soil, Rotmistrov[3] regards it of no importance to those plants whose roots do not reach down to it. Hall[4] in a recent analysis of the situation has stated: "The evidence on either side is far from being conclusive and more experiments are very desirable."

The present differences in views appears to be due to the failure in laboratory experiments and field studies to take into consideration some physical constant that is directly related to both the lower limit of available moisture and the *water-retaining-capacity* of the soil, if we define the latter as the maximum amount which a soil will carry after it has been saturated and then, protected from both direct evaporation and the indirect effects of this as well as the action of plant roots, allowed to come into approximate moisture equilibrium by the downward movement of the excess of water into the subsoil mass. The lower limit of available moisture as determined by plant-house experiments, in which crop plants were grown in 6-foot cylinders and left unwatered until they matured or died of lack of water, appears to be practically coincident with the hygroscopic coefficient. Up to the present a method of estimating the water retentiveness in the field from one of the physical constants of the soil has not been developed. The laboratory experiments and field studies of the authors make it appear that in the case of soils with hygroscopic coefficients between 14 and 3 this bears a rather simple relation to the hygroscopic coefficient and that in coarser soils, while it bears a much less simple relation, this is still one that may be experimentally determined. As the great majority of the tillable soils of dry-land regions fall within the limits of hygroscopicity mentioned it would appear that through the determination of both the moisture content and the hygroscopic coefficient in the case of samples of the deeper subsoil we could learn both the percentage of the physiologically important water and the departure of this from the maximum which the particular subsoil could retain.

The investigation was carried out in 1912 and 1913, while we were at the Nebraska Agricultural Experiment Station, the field studies being conducted partly in the humid eastern portion of Nebraska and partly in the semi-arid western end of that state.

The soils involved in the experiments included both surface soils and subsoils and ranged in texture from a Dune sand, with a hygroscopic coefficient of 0.6 and a moisture equivalent of 1.5, to a silt loam with

corresponding values of 13.3 and 29.5. In the laboratory experiments we employed cylinders 4 to 6 inches in diameter and of various lengths. These were filled with soils of known hygroscopic coefficients and moisture equivalent, and, except where saturation was desired, we determined the initial moisture content.

Five of the loams, placed in capillary connection with the natural subsoil mass, saturated with water and allowed to stand protected from surface evaporation for several months, lost water until the amount retained bore a close relation to the hygroscopic coefficient, being from 2.1 to 3.1 times this value, according to the particular soil. When a layer of coarse sand or gravel separated the column of loam from the natural subsoil mass or interrupted it the downward movement of the water in the soil above this layer was much delayed. Where the column consisted of successive 2 inch layers of loams differing widely in texture the order of their arrangement exerted no influence upon their final water content.

Soil columns, 30 to 36 inches long, while protected from all loss of moisture at the sides and bottom, were freely exposed to evaporation at the surface for periods varying from a few weeks to half a year. The moisture content, originally uniform and lying between 2 and 3 times the hygroscopic coefficient, fell until it reached, at depths below the first foot, an almost constant minimum with the ratio 1.9 to 2.2.

Employing 2-foot columns of 12 different loams, each with an initial moisture content approximately equal to its hygroscopic coefficient, enough water was added to raise the average moisture content of the column to 1.5 times the hygroscopic coefficient, the water being applied in one experiment to the top and in another to the base of the column. After the cylinders had stood for three or four months, fully protected from evaporation, the distribution of moisture, with regard to the surface to which it had been applied, was found to be the same in both experiments. The maximum distance through which an effect was shown was about 2 feet but in most cases the distance was much less. The maximum final ratio of moisture content to hygroscopic coefficient was found in the section adjacent to the surface of application, where it lay between 1.7 and 2.4. The ratio, while falling within these limits, is not a constant, it not being the same for all the soils that have the same hygroscopic coefficient.

The water-retaining capacity of the loams, as determined by laboratory experiments, was found to bear a somewhat closer relation to the moisture equivalent than to the hygroscopic coefficient, the ratio varying between 0.8 and 1.2.

Coarse sands exhibited a behavior very different from that of the loams. The ratio in the surface 6 inch section, even three months after one inch of water had been applied to the surface, was as high as 6 or 7 while in the second foot it was only 1. The field studies on coarse sands showed as high a final ratio as was observed in the laboratory experiments.

The very limited studies on fine sands indicate that these occupy a position intermediate between the loams and the coarse sands, the ratio of the water-retaining capacity to the hygroscopic coefficient rising as the latter value falls.

Field studies show that when loams, after rains sufficiently heavy to thoroughly moisten them, are protected from losses by evaporation and transpiration, they lose water by downward movement until the ratio of moisture content to hygroscopic coefficient lies between 1.8 and about 2.5, and, accordingly, that on the uplands of dry-land regions this is the ratio to be expected in the deeper subsoil—the portion below the range of plant roots.

A comparatively abrupt transition from the moistened soil to the thoroughly exhausted underlying layers, with ratios of 2 to 2.5 and 1 to 1.1 respectively, is found even several months after liberal rains have fallen, if the subsoil to a considerable depth had previously been exhausted of available water.

The moisture of the deeper subsoil will be able to move upward only so slowly and through such a short distance in a single season that it will be at most of no *practical* benefit to annual crops. To make use of any portion of the precipitation which penetrates beyond the reach of the roots of annual crops it will be necessary to follow such crops at intervals by deep-rooting perennials.

Further experiments of a long-time character are necessary to definitely decide whether the deep subsoil may not in a decade or so contribute sufficient moisture to the subsoil within the reach of the roots of such perennials, 20 to 30 feet, to make such a contribution of some practical importance for such crops.

This is a preliminary extract of a paper transmitted to the *Journal of Agricultural Research.*

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THE TRANSFORMATION OF PSEUDOGLOBULIN INTO EUGLOBULIN

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Communicated by R. Pearl, February 16, 1917

In several publications Banzhaf[1] states that when diphtheria serum is heated as it is in the preparation of antitoxin, part of the pseudoglobulin is transformed into euglobulin. Details regarding the methods of analysis, or the analytic data on which Banzhaf based his conclusion were not found in the publications examined.

This transformation has both a practical and a theoretical interest. It facilitates the concentration of the antitoxin present in the serum by removing protein without removing antitoxin, so that the final product contains all the antitoxin associated with much less protein. This is desirable because certain of the serum proteins have very little therapeutic value. In applying the heat treatment for the first time to a serum such as anthrax serum, for example, it is obviously desirable to be certain that a similar transformation takes place. Otherwise there would be no need of the heat treatment, and besides, the heating may cause loss of potency.

On the theoretical side, the fact that pseudoglobulin can be transformed into euglobulin without affecting the total number of antitoxic units is almost conclusive proof that the antitoxin is a substance separate from pseudoglobulin. That this transformation may take place in some sera, but not in all, is indicated by the following experiments, in which the heating of the serum was carried out under carefully controlled conditions and the analytic data obtained by improved methods. In all, four horse sera were used, which will be designated as follows:

Anthrax 48. The serum obtained from Horse 48; used in a previous work.[2] Potency high.

Anthrax 96. The serum obtained from Horse 96; used in a previous work. Potency lower than Serum 48.

Diphtheria 1. A mixture of sera obtained from 2 horses. Potency high.

Tetanus 1. A mixture of sera obtained from 2 horses. Potency fair.

The first attempts to detect the transformation failed almost certainly because the experiments were made in connection with the preparation of globulin for therapeutic use. Better analytic results were obtained when using smaller amounts of serum under conditions adapted to quantitative analysis and separate from antitoxin preparation.

Heating the serum.—The procedure with a single serum was as follows.

Into each of 2 erlenmeyer flasks of 200 cc. capacity, pipet 50 cc. portions of the serum. To each flask there were added 25 cc. of water and 32 cc. of saturated ammonium sulfate solution; resulting in 30% saturation. At this concentration euglobulin was precipitated. One flask, not to be heated, was stoppered and set aside. The other flask, to be heated in the water bath, was stoppered with a rubber stopper carrying a standard thermometer. The bath had been so adjusted that during the experimental heating of the serum mixtures, the bath temperature did not rise beyond 61°.2 nor fall below 60°.5. The temperatures inside the flasks (it was convenient to work with 2 sera at a time) rose from that of the room, about 27°, to 56° in the first five minutes of the heating, then to 59° in the next five minutes. After ten minutes heating the temperatures inside the flasks were exactly 60° or below it by a small fraction of a degree. The heating was then continued for exactly thirty minutes during which time the temperatures inside the flasks did not exceed 60°. At the end of the heating period the flasks were transferred to a pan containing cold water. This brought the temperature down to that of the room, in about five minutes. The next step is the separation and estimation of the precipitated euglobulin.

Method of analysis.—All four serum mixtures, 2 heated and 2 not heated, were transferred to 110 cc. centrifuge tubes and centrifuged for twenty-five minutes at about 2500 revolutions per minute. The sedimentation was perfect; the sticky euglobulin was firmly packed at the bottoms of the tubes. The supernatant liquids containing the pseudoglobulin, albumin and the antitoxin, were poured into 100 cc. volumetric flasks. The tubes could be inverted without loss of precipitate. The euglobulin was dissolved in water, coagulated by heat, filtered, dried and weighed by methods already described.[2] In the supernatant liquid estimations were made of (1) pseudoglobulin; (2) albumin and (3), these two together.

Results.—The figures in table 1 show unmistakably that more euglobulin precipitate was obtained from the heated than from the unheated serum. The figures for euglobulin in table 2, item (b), are the same as those in table 1, divided by 5. The main object of obtaining the other data was to ascertain the source of the excess of euglobulin in the heated precipitates and to ascertain how accurate or inaccurate the method of analysis was. Theoretically, in table 2, (a) should equal the sum of (b) plus (c), and (c) should equal the sum of (d) plus (e). Although this equality is absent, the differences appear to be consistent and indicate a uniformity of error due to the absence of corrections for the compara-

TABLE 1

WEIGHTS OF EUGLOBULIN OBTAINED FROM 50 CC. PORTIONS OF SERUM

SERUM	NO.	WITHOUT HEAT	WITH HEAT	INCREASE IN WEIGHT
		grams	*grams*	*grams*
Anthrax	48	0.774	1.201	0.427
Anthrax	96	0.547	0.793	0.246
Diphtheria	1	0.406	0.719	0.313
Diphtheria	1	0.435	0.642*	0.207
Tetanus	1	0.243	0.304	0.061

* Low result, due to incomplete flocculation.

TABLE 2

ANALYSES OF HEATED AND UNHEATED SERA. ALL WEIGHTS CALCULATED TO GRAMS IN 10 CC. SERUM

	ANTHRAX				TETANUS		DIPHTHERIA			
	48 not heated	48 heated	96 not heated	96 heated	1 not heated	1 heated	1 not heated	1 heated	1 not heated	1 heated
(a) Total coagulable protein	0.618*	0.618	0.788	0.788	0.558	0.558	0.665	0.665	0.665	0.665
(b) Euglobulin	0.155	0.240	0.109	0.159	0.049	0.061	0.081	0.144	0.087	0.128†
(c) Pseudoglobulin and albumin estimated together	0.437	0.378	0.668	0.625	0.447	0.451	0.534	0.487	0.523	0.484
(d) Pseudoglobulin	0.290	0.211	0.568	0.535	0.385	0.378	0.475	0.397	0.479	0.438
(e) Albumin	0.191	0.191	0.133	0.135	0.103	0.103	0.090	0.098	0.087	0.097
Pseudoglobulin converted into euglobulin	27%		6%		0.2%		16%		9%	

* The same figure for total coagulable protein applies to both the heated and not heated serum; these are not exact duplicates.

† Low result; see table 1.

tively large volumes of the precipitates, solubility of precipitate in the washwater, occluded ammonium sulphate, etc. All the errors however, do not invalidate a comparison of the data in one column with those on the same serum, in the next column.

The figures for albumin are practically the same in the heated and unheated sera. The figures for pseudoglobulin are consistently lower in the heated sera than in the unheated; this is an almost necessary consequence of the transformation of part of the pseudoglobulin into some protein having several of the precipitation characteristics of euglobulin. What is most important for the present investigation is that the *loss of pseudoglobulin in the heated sera corresponds almost quantitatively with the gain in euglobulin in the same sera.*

To calculate the percent of pseudoglobulin transformed into euglobulin by the heat treatment; the difference between the amount of

pseudoglobulin in the unheated and heated serum divided by the amount of pseudoglobulin in the unheated serum gives the percent transformed; these are tabulated at the bottom of table 2. Thus for Anthrax 48, the figures are $(0.290-0.211)/0.290=27\%$. This is not the only way to calculate this figure. Figures for pseudoglobulin may be obtained from a different set of results; thus, they may be calculated by subtracting the figures for albumin (e) from those or pseudoglobulin plus albumin (c). If the percent of transformation be calculated from these lower values for pseudoglobulin the figures are, 24, 8.4., -1.2, 12.4 and 11.2%; to read across the bottom of table 2.

Further details are given in the *Journal of Agricultural Research*, 1917.

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A CASE OF NORMAL EMBRYONIC ATRESIA OF THE ESOPHAGUS

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Communicated by A. G. Mayer, January 20, 1917

In a series of loggerhead turtle embryos, collected and used originally for a study of the history of the primordial germ cells, the esophagus was observed to be solid for a greater or less extent, approximately from the point of origin of the respiratory anlage to its bifurcation into the bronchi, from the twelfth to the thirty-second day of incubation. At the latter stage the esophagus is still occluded at its oral end, though now fenestrated for a considerable extent caudally; and it seems probable that the esophageal atresia persists practically to near the end of the incubation period (eight weeks) at the level just behind the opening of the larynx.

The points of special significance in regard to this material are: (1) the relatively longer persistence of the occlusion than has yet been described for any other form; (2) the absence of contributory yolk in the stenosed area; and (3) close relation of the atresia to the point of origin of the respiratory anlage, which fact may disclose its possible functional significance.

Balfour[1] was the first to describe a similar phenomenon in the esophagus of certain selachii. Kreuter[2, 3] confirmed these observations in the case of *Pristiurus* and *Torpedo*. Dean[4] reports a solid esophagus in larvae of *Amia calva*. An occluded esophagus is said to occur also in certain bony fishes, e.g., herring, trout, salmon (Balfour;

Oppel[5]). In cyclostomes the esophagus remains patent throughout development (Kreuter[3]). In certain amphibia (*Bufo; Rana*) the esophagus becomes occluded, in part through the medium of contributory yolk globules (Meuron[6]); and the same is true for certain reptiles (*Anguis fragilis*, Oppel;[5] *Lacerta*, Meuron[6]). According to Meuron[6] the esophagus of the chick embryo of the fifth day is occluded for a length of 115 microns, but regains partial patency again in the sixth day through the appearance of vacuoles.

Kreuter[3] was the first to describe an epithelial obliteration of the esophageal lumen in the human embryo; contrary to the teaching of Kollmann[7] and other embryologists that no solid stage of the esophagus occurred in mammals and in man. Kreuter[3] describes also similar obliterated areas in the mid- and hind-gut of embryos between the fourth and tenth weeks. In four human embryos, measuring from 8.4 to 16 mm., Lewis[8] describes an esophagus whose lumen is pervious throughout. He, however, describes vacuoles in the epithelial lining of these stages similar to those described by Kreuter as stages in the opening of the solid esophagus. But he regards an atresia of the esophagus in the human embryo as abnormal at all stages (p. 368).

It would seem that an embryonic normal atresia of the esophagus is a widespread phenomenon among vertebrates, and is essentially similar from elasmobranch fishes to man.

The phenomenon has not yet, as far as I am aware, been described for turtles, a circumstance which adds to the interest of this investigation. Nor has its intimate spatial relationship to the respiratory anlage, and its probable functional significance, been hitherto pointed out. The more important results may be summarized as follows:

1. During the tenth and eleventh days of incubation the epithelial lining of the oral end of the esophagus (esophageo-respiratory anlage) thickens greatly dorsally, the result of extensive cell proliferation in this region. During the twelfth day the cylindric tube of the esophagus becomes compressed dorso-ventrally, thus bringing the dorsal and ventral epithelial walls in close apposition. Only the minutest central lumen persists in the oral end of the esophagus for a distance of about 0.25 mm. During the thirteenth day the oral end of the esophagus is rectangular in cross section and completely solid for a distance of about half a millimeter. The opposed central cells have fused and formed a plug of tissue, essentially like a mesenchymal syncytium.

2. The initial point of atresia is *over*, or just behind, the orifice of the separating laryngo-tracheal anlage; and its inception is coincident with the earliest stage in the division of the original esophageo-respira-

tory anlage into an esophageal and a laryngo-tracheal tube. By the sixteenth day the atresia has extended into the orifice of the larynx, due in part perhaps to pressure exerted by the lateral arytenoid swellings.

3. The chief factor in the temporary closure of the originally open esophagus is the change in shape of the esophagus from a tube approximately circular in cross section to a structure of wide rectangular form with at first a slit-like lumen and finally a minute central aperture. The cause of the change in shape, upon which the obliteration of the lumen largely depends, is the combination of growth within the esophagus in opposition to the denser lateral mesenchymal plates, by the invasion and medial fusion of which the laryngo-tracheal groove becomes converted into a tube and incidentally separated from the esophagus distally. This process is assisted, as concerns the obliteration of esophageal lumen, by the active cell proliferation in the dorsal wall of the esophagus.

4. In the sixteen day embryo, the atresia of the esophagus extends through about 1500 microns. Beyond the oral end vacuoles begin to form in the lining epithelium. These represent dilated 'intercellular' spaces chiefly within the central syncytial plug of tissue. They increase in number, and enlarge caudally, where they become confluent. During succeeding stages this process of vacuolization continues, until at the thirty-second day stage only the extreme oral end of the esophagus remains closed.

5. Both the closure and the reëstablishment of the lumen of the embryonic esophagus involve mechanical as well as growth processes, but are normal for a certain stage of the embryonic development. The closure is not largely dependent upon intrinsic cell division; and the fenestration process involves no tissue degeneration or resorption. The level of initial closure and the level of final perforation are approximately the same, namely, the laryngeal level of the esophagus.

6. In the process of vacuolization upon which the opening of the temporarily stenosed esophagus depends, the larger spherical vacuoles are drawn into irregular areolae as if through traction exerted from without. This traction no doubt inheres in the growing and expanding periphery of the esophagus. The esophagus now has a fenestrated appearance in section; its lumen is spanned by more or less delicate nucleated septa which may anastomose, giving to the whole the appearance of a wide-meshed syncytium. Ultimately the trabeculae are drawn into the lining epithelium, and their nuclei incorporated among the entodermal cells of the mucous lining.

7. The temporary atresia of the esophagus in the *Caretta* embryo would appear to be a device for the protection of the lung during its development against yolk material from the gut; which material could not be digested but would interfere with normal development of the lung.

8. This hypothesis can comprehend and correlate conditions in embryos of forms with meroblastic, homoblastic telolecithal, and alecithal eggs. Where yolk is very abundant as in the meroblastic eggs of fishes, reptiles and birds, the atresia is relatively extensive and of longer duration; in amphibia the closure is largely of the nature of a stenosis in which yolk globules are involved, probably in process of digestion while their forward progress is delayed by reason of the constricted lumen. In most mammals and in man such mechanism is functionally superfluous, and consequently absent except in slight and variable degree. As such it may persist or become accentuated, and produce congenital atresia or stenosis of the esophagus.

A more detailed description will appear in Publication No. 251 of the Carnegie Institution of Washington.

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STUDIES OF MAGNITUDES IN STAR CLUSTERS, V. FURTHER EVIDENCE OF THE ABSENCE OF SCATTERING OF LIGHT IN SPACE

By Harlow Shapley

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Communicated by G. E. Hale, February 15, 1917

The interest and importance attached to an accurate quantitative knowledge of the scattering of light in space, for all studies of the extent and character of the stellar universe, has been commented upon by various writers, particularly by Professor Kapteyn. In the first communication of this series[1] the matter was discussed briefly and evidence was presented showing that interstellar space, at least in the direction of the Hercules cluster, is free of the kind of light absorption that modifies the color of the stars. The generalization of this

hypothesis into the conclusion that such light scattering is practically negligible in all parts of the sky awaits, first, the verification of the original observational result; second, the confirmation of the proposed physical interpretation of such data; and, finally, an investigation of the faint and most distant stars in a wide range of galactic latitudes and longitudes. The present note contains a further contribution to the subject and permits a fairly definite solution.

In the first study of the magnitudes of the stars in the Hercules cluster normal values of color indices were obtained. Among a thousand objects no abnormally large or small values were found; and among the fainter stars, photovisual magnitude 14 to 16, a great number of negative indices appeared. The essential correctness of this result has now been checked in two different ways—by a new series of polar comparisons, and through a determination of the color indices by the method of exposure ratios.[2]

Assuming the observed colors to be reliable, the presence of negative color indicates two significant conditions, namely, the absence of light scattering, and the great distance and size of the cluster. There appears to be no adequate reason for modifying this adopted interpretation of the blue faint stars. The decrease in the intensity of light by terrestrial atmospheric scattering is known to vary inversely as the fourth power of the wave length. The dissipated atmospheres and meteoric dust, to whose agency spatial light attenuation is commonly ascribed, are recognized as similarly capable of the preferential scattering of blue light. Such a differential light absorption certainly would make all stars appear redder. The presence of very blue stars, therefore, points to the inefficiency of the light-scattering residue in space, provided the objects involved are sufficiently remote. As all studies of B-type stars (negative color indices) indicate a high absolute luminosity and relatively small dispersion around a mean value, such stars, when apparently faint, are an immediate and apparently reliable index of great distance, and distance is the most important factor in testing the scattering of light in space.[3]

The systematic increase of redness observed toward the center of the Hercules cluster[4] may be interpreted as a spurious effect caused by the crowding of images on the photographic plate, or as a scattering of light in the denser portion of the cluster itself. The former is almost surely the true explanation. The possibility, however, that the latter cause contributes, suggests that the absence of light absorption in the direction of the Hercules cluster does not imply an inappreciable absorption in the lower galactic latitudes where stellar material appar-

ently is much more concentrated. Accordingly it becomes appropriate to investigate clusters in the Milky Way and the faint stars in the galactic clouds. The presence of negative color indices, or a normal range of color, in such regions, assuming of course the distances to be great, will indicate the essentially complete transparency of space throughout the whole stellar system.

The position, with respect to the galactic plane, of the fields in which magnitudes and colors have been studied in the course of this work are listed in the following table. With two or three exceptions the regions are in or around stellar clusters. The fourth and fifth columns give for each region the limiting values of the color indices. In several of these fields the survey is not yet complete, and values somewhat larger and smaller may be found later. This applies especially to Messier 5

PRELIMINARY RESULTS BEARING ON THE EXTENT OF THE GALACTIC SYSTEM

REGION	GALACTIC LONGITUDE	GALACTIC LATITUDE	OBSERVED COLOR INDEX		AVERAGE P_V MAG.	AVERAGE COLOR INDEX	PARALLAX	
			Largest	Smallest			M = 0	M = +2
Messier 3*	12°	+78°	+1.77	−0.39	15.1	−0.16	0".0001	0".00024
Messier 13*	27	+40	+1.42	−0.52	16.54	−0.34	0.00005	0.00012
Messier 15*	33	−28	+1.50	−0.21	16.0	−0.14	0.00006	0.00016
XX Cygni**	60	+13	+1.33	−0.14	12.64	−0.02	0.0003	0.0007
In Lacertae	69	− 3	+1.58	−0.37	12.65	−0.01	0.0003	0.0007
Messier 38	139	+ 1	+2.12	−0.45	13.5	−0.16	0.0002	0.0005
Messier 36	142	+ 1	+1.50	−0.30	12.5	−0.23	0.0003	0.0008
Messier 35	154	+ 3	+1.31	−0.15	11.5	−0.07	0.0005	0.0013
Messier 50	189	− 1	+2.00	−0.04	12.3	−0.02	0.0003	0.0009
Messier 5*	332	+46	+1.67	−0.11	14.6	−0.10	0.0001	0.0003
Near M 11	354	− 3	+2.23	−0.42	14.2	−0.14	0.00015	0.00036
Messier 11	355	− 3	+2.06	−0.16	14.32	−0.08	0.00014	0.00034
Near M 11	356	− 4	+1.81	−0.14	13.9	−0.01	0.0002	0.0004

*Globular cluster. **Three stars.

and Messier 3, for which the magnitudes of the fainter and probably more typical b-class stars are not yet available. The sixth and seventh columns give for each region the mean photovisual magnitude and mean color index of a group of the faintest blue stars. The average number in each group is ten.

The accidental errors affecting the colors are so small that the dispersion of color index and the negative and small positive values of the index cannot be attributed to observational uncertainty. The systematic errors have been carefully examined for several of these regions and they are similarly inadequate, at least as far as our present knowledge of them extends. The blueness of the faint and distant stars, thus

shown to exist for numerous directions in space, may be accepted, therefore, as an indication of the general ineffectiveness of differential light scattering. This result also naturally implies the absence of all diminution of the intensity of light through the agency of interstellar media.

The daily encounter of the earth with millions of meteors is frequently cited as evidence of the prevalence of highly efficient light-scattering and light-obstructing material throughout interstellar space. If, as seems likely, the meteoric dust, which is closely allied to the meteoric streams and comets, is a definite and original part of the solar system and is not cosmic in origin and motion, this argument loses its force; and whatever extinction of light such matter produces, either in the solar system or in immediate gravitational field of other stars, is wholly unimportant in its bearing on stellar distribution and distances.

Disregarding light absorption, we may compute for each region the parallax of stars of the tabulated mean photovisual magnitude, as soon as we know the mean absolute luminosity corresponding to the average color. Upon the basis of the researches of Kapteyn, Russell, Charlier, and others, we make two assumptions for the mean absolute magnitude of the blue stars, $M = 0$ and $M = +2$, and compute the results given in the last two columns of the table. For the globular clusters the smaller parallaxes are undoubtedly preferable, since in these systems we are certainly treating only the brightest of the bluer stars. Moreover, the smaller value is in keeping with results derived by means of variables and other stars of high luminosity. For the open clusters the larger values of the parallax are probably better. In a few of the regions, however, the magnitudes and colors are provisional; and the adopted mean absolute magnitudes of B-type stars, though not improbable, are of course somewhat uncertain. Hence the estimates of the extent of the galactic system (including its open clusters), and the indications of the relatively greater distances of the globular clusters, are here proposed as only preliminary results; their main value lies in the definite evidence of the vast dimensions of the visible stellar universe and in the problems suggested for further investigations along this line.

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[2] Seares, F. H., *Ibid.*, 2, 1916, (521–525).
[3] Shapley, H., *Mt. Wilson Contrib.* No. 116, 1915, (1–92), pages 52–58 and 88.
[4] *Ibid.*, pages 58–61.

THE HISTORY OF THE PRIMORDIAL GERM CELLS IN THE LOGGERHEAD TURTLE EMBRYO

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Communicated by A. G. Mayer, January 20, 1917

In view of the wide discrepancies in the published accounts of the germ-cell history in various vertebrates, it seems very desirable that the problem should be reinvestigated among a greater range of forms, and by additional workers.

The present study represents an attempt to trace the complete early history of the genital cells in a peculiarly favorable form, and, in the light of this data, to correlate the mass of apparently discordant observations recorded for other vertebrates.

The material employed consists of embryos of the loggerhead turtle, *Caretta (Thallassochelys) caretta*, from the two- to the thirty-two day stage of incubation. Parallel series were prepared by the Flemming-iron-hematoxylin and the Helly-Giemsa technics.

This material possesses the following combination of unusual advantages for such study: (1) it can be procured in great abundance; (2) the developmental process is relatively slow (the incubation period extends through fifty-six days), yielding thus a closely graded series of stages; and (3) the germ cells are abundant and at all stages sharply demarked, by size, shape, and nuclear and cytoplasmic characteristics, from the cells with which they seemed possibly to have been confused in some forms, especially the less differentiated yolk-laden entoderm cells, and certain blood granulocytes (eosinophiles).

I am indebted to the kindness of the Carnegie Institution of Washington and to Dr. Alfred G. Mayer, Director of the Department of Marine Biology, for the opportunity of collecting this material on Loggerhead Key, Florida, during the summer of 1914.

The fundamental controversy concerning the history of the germ cells in vertebrates centers about the question of their relationship to the so-called 'germinal epithelium' of the genital ridge. Waldeyer[1] maintains that in the chick certain cells of the portion of the peritoneal epithelium covering the primitive gonad differentiate into germ cells. Nussbaum[2] was the first to controvert this theory, claiming that the germ cells are early segregated (in trout, frog and chick embryos) from the soma cells and that they subsequently wander from originally widely scattered regions into the differentiating sexual glands.

These two rival theories have had their adherents to the present day. Gatenby,[3] for example, claims that in certain amphibia (*Rana temporaria*, *Salamandra*) the definitive germ cells arise by process of transformation of peritoneal cells, as first described by Waldeyer; in similar forms (*Rana pipeins; Bufo lentiginosus*) Allen[4] and King[5] claim that the definitive germ cells (gonia) and the primordial germ cells are the same, the former being genetic derivatives of the latter, and that neither are genetically related to the peritoneal epithelium.

A number of investigators, e.g., Felix,[6] Firket[7] and Dustin[8] claim the existence of two generations of germ cells, the primary and secondary genital (sex) cells or gonocytes. The earlier generation is supposed to have only a phylogenetic significance and to early disappear after a more or less nomadic career; while the later generation is claimed to have its origin from the cells of the peritoneum covering the gonads.

As regards the chick, Nussbaum's original claim is now fully sustained both on the basis of morphologic and of experimental data. Swift[9] has discovered the original entodermal locus of the primordial germ cells in this form in a cephalic crescentic area, in the early primitive-streak stage. As the spreading mesoderm invades this region the germ cells become largely involved in the blood vessels, by which they are carried to the splanchnic mesoderm of the hind-gut region; from whence, by ameboid activity, they wander up the forming mesentery and across the celomic angle into the developing gonads. An unbroken lineage has now been traced by Swift[10, 11] between these primordial germ cells of the gonads and the oögonia and spermogonia of the ovary and testis respectively, without any contribution from the peritoneal epithelium. Reagan[12] has put Swift's observations to a crucial experimental test by removing the crescentic germ-cell area of the primitive-streak stage, and reincubating such operated specimens. Sections of such chick embryos revealed a complete lack of germ cells.

Subsidiary more important matters concern the migration route of the primordial germ cells, and the possibility of a persistence of latent 'stray' germ cells to serve as possible future foci for neoplasms. In the chick (Swift) and in the duck (von Berenberg-Gossler[13]) the germ cells are said to migrate largely via the blood vessels. These observations involve the possibility of a confusion with hypertrophied blood granulocytes. Moreover, von Berenberg-Gossler[13] records the anomolous observation that in the lizard, *Lacerta agilis*, 'primordial germ cells' ('entodermal wandering cells') contribute to the formation of the caudal extremity of the Wolffian duct. He regards these cells

as representatives of a dilatory process of mesoderm derivation from entoderm.

As regards amphibia, the later observations incline Allen[14] to conclude that in anurans the germ cells have an entodermal origin (i.e., original segregation), in urodeles a mesodermal origin.

The germ-cell history of *Caretta* is very similar to that first described by Allen[15] for *Chrysemys marginata*—and more recently confirmed by Dustin[16] —and to that described by Woods[17] for the dogfish. This may be summarized as follows:

1. The primordial germ cells in *Caretta* migrate during the second day (5 somites; 2 mm. length) from the yolk-sac entoderm, where they were more or less widely scattered caudally, into the lateral border of the area pellucida on each side of the embryonic disc. Here they become sharply segregated by the beginning of the third day (10 somites; 3 mm. length) into bilateral cords situated in the entoderm of the area pellucida laterally, in the caudal half of the disc. In the two-day embryo they extend from the neurenteric canal to the end of the primitive streak; in the three-day embryo from the sixth somite to the caudal extremity of the streak. The cords become more medially placed, make a linear connection with the overlying visceral mesoderm, and their cells migrate during the fifth day into this mesoderm, and thence medially (during the sixth and seventh days) towards the root of the forming mesentery of the closing hind-gut. Individual cells migrate medially also within, or back into, the entoderm of the gut. The germ cells in the medial entoderm become included in the tunica mucosa of the closed hind-gut, those in the mesoderm in the enveloping mesenchyma and the gut end of the mesentery. From these locations the majority of the germ cells subsequently (seventh to twelfth day) migrate up the mesentery and across the celomic angle into the future sexual gland. They become incorporated among the mesenchymal cells of the gland and the covering peritoneal epithelium, where they suffer no striking change in form, size or content at least as late as the thirty-second day of incubation.

2. The germ cells migrate by ameboid activity, assisted in small part probably by the factor of unequal growth, involving the shifting of the medial portion of the splanchnopleure to the mesentery, and the dorsal portion of the mesentery to the gonads.

3. The migration period is not sharply limited. It is at its height from the seventh to the twelfth day, and practically ceases about the sixteenth day. But occasional extra-regional cells may still be found in the gut and mesentery at the thirty-second day stage, usually, however, showing signs of degeneration.

4. A certain number of germ cells migrate out of the regular germ-cell route and go astray. Such 'strays' are especially numerous in the periaortic mesenchyma where they may become incorporated among the neuroblasts of the developing peripheral sympathetic ganglia. The majority of these strays probably degenerate *in situ*, but some may possibly persist to form, under the proper pathologic stimulus, a focus of neoplastic growth. An occasional cell is found also in the blood vessels of this region. Such may be carried by the blood stream to distant regions and perhaps again enter the mesenchyma or degenerate within the vessels.

5. The total number of primordial germ cells counted in a twelve-day embryo is 352, the number within the gonads being about equally divided between the two (118, left to 127 right).

6. Occasional cells may divide by mitosis or undergo degeneration at any stage of their history or at any point of the route. Mitoses are relatively more numerous during earlier stages and among the entodermal cells; degeneration is more general during the later stages and in the mesenchyma of the closed hind-gut.

7. No germ cells were found contributing to the formation of the Wolffian duct. There is no evidence in this form in support of von Berenberg-Gossler's claim, on the basis of his observations on the lizard embryo, that the so-called primordial germ cells represent simply a belated stage of mesoderm formation from entoderm.

8. The germ cells do not differ from young somatic cells in the character of their mitochondrial content. The mitochondria include granular as well as beaded rod and filamentous forms.

9. No transition stages between celomic epithelial cells and germ cells appear up to the thirty-second-day stage. From the sixteen-day stage on, when the nuclei of some of the germ cells within the gonads became coarsely granular and the reticulum stains more deeply, apparent transition stages occur between the larger of the mesenchymal cells and the smaller included subepithelial germ cells. But no secure histologic basis can here be found for separating the germ cells of the gonads into large "primary genital cells" and smaller "secondary genital cells" (Felix) or gonocytes (Dustin), derived by process of differentiation from the cells of the germinal (peritoneal) epithelium or the subjacent mesenchyma. The size variations among the germ cells of the gonads of the older stages are no greater than in the original cords of the area pellucida or in the subsequent early stages; and the cytologic similarity between the two dimensional grades of cells is much closer than between the larger mesenchymal cells and the smaller germ cells.

10. The evidence derived from a study of the *Caretta* embryos is

in complete harmony with the idea of a single uninterrupted line of sex cells from primordial germ cells to oögonia and spermagonia, and with the hypothesis of a vertebrate 'Keimbahn' or continuous germinal path.

11. The variations in the distribution of the primordial germ cells during earlier embryonic stages described by various investigators for a number of vertebrate forms—as pertains both to their presence in blood vessels (chick, Swift; duck, von Berenberg-Gossler) and in various regions and tissues remote from the more direct and more usual germinal route (Wolffian duct and somatopleure in the lizard, von Berenberg-Gossler; and sympathetic ganglia in the loggerhead turtle, Jordan); and to their apparent primary (urodeles) or secondary (anurans and other vertebrates) derivation from the splanchnic layer of the lateral mesoderm—are incidental to their original location with respect to the embryonic area and the vascularizing mesoblast of the blastoderm, and to their ameboid capacity. Since the primordial germ cells are genetically directly related to neither of the secondary germ layers their origin in either (entoderm or mesoderm) has no fundamental significance. Since they are capable of ameboid activity, and may become included in blood vessels, they may migrate anywhere, and so occur in any location, from where they may subsequently migrate again to the more direct germinal path, or perhaps disintegrate. The fact of fundamental significance with respect to the primordial germ cells is their original extra-regional distribution and their direct genetic independence of the soma cells.

A more complete paper, with illustrations, and a fuller review of the literature, will appear in a forthcoming volume from the Tortugas Laboratory of the Carnegie Institution of Washington.

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[6] Felix, W., Keibel, and Mall, *Human Embryology, Philadelphia*, **2**, 1912, (882–890).
[7] Firket, J., *Arch. Biol., Liège*, **29**, 1914, (478–521).
[8] Dustin, A. P., *Ibid.*, **23**, 1907, (411–522).
[9] Swift, C. H., *Amer. J. Anat., Philadelphia*, **15**, 1914, (483–516).
[10] Swift, C. H., *Ibid.*, **18**, 1915, (441–470).
[11] Swift, C. H., *Ibid.*, **20**, 1916, (375–410).
[12] Reagan, F. P., *Anat. Rec., Philadelphia*, **2**, 1916, (251–268).
[13] von Berenberg-Gossler, H., *Anat. Anz., Jena*, **47**, 1914, (241–263).
[14] Allen, B. M., *J. Morph., Philadelphia*, **22**, 1911, (1–36).
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STUDIES OF MAGNITUDES IN STAR CLUSTERS. VI. THE RELATION OF BLUE STARS AND VARIABLES TO GALACTIC PLANES

By Harlow Shapley

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Communicated by G. E. Hale, February 19, 1917

The possibility of the invariable presence of axes of symmetry in globular clusters is attested by the results given in a recent communication by Mr. Pease and the present writer.[1] The observed elliptical distribution of stars may denote a prolate spheroidal form; or, as appears more probable, it may signify the projection of a discoidal figure, in which case the axis of symmetry represents the lines of intersection with the celestial sphere of the central plane of a system analogous in form to our own Galaxy. In any case the phenomenon of symmetrical elongation may be highly significant in problems of stellar dynamics, and as such is deserving of further study.

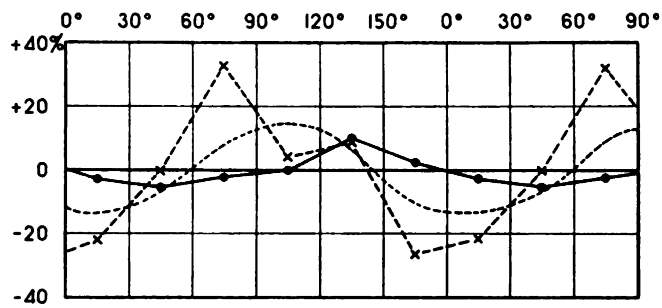
One method of investigation is the study in globular clusters of the distribution of those objects which show a peculiar arrangement in our galactic system with respect to the plane of the Milky Way. It does not follow, of course, that they should also show condensation along a symmetrical plane in all large stellar systems, but it will be of interest and of much importance to know whether or not they do.

Stars of spectral class B and the longer-period Cepheid variables are two types of objects which in the local system show a strong galactic preference. Data bearing on both of these are available for Messier 13—the only globular cluster for which a detailed study of magnitudes and colors has been made.[2] Earlier discussions of the distribution with respect to the center of the brightest thousand stars of Messier 13 failed to show a definite tendency toward symmetrical elongation,[3] but, from an investigation of some 30,000 faint stars in this system, we now know with considerable accuracy the position of its axis of symmetry. With the orientation of the density ellipse as a starting point, the bright stars have been rediscussed from the standpoint of color index. The method of treatment is sufficiently indicated in the earlier communication relative to axes of symmetry.

The smooth dotted curve in figure 1 is based upon the count of 10,000 stars between the seventeenth and nineteenth magnitudes, and is inserted merely to show the true position of the axis, and, by means of the amplitude of the curve, to indicate the degree of the ellipticity. The ordinates are the percentage deviations of the number of stars in each 30° sector from the mean number for all sectors; the abscissae are

angles of direction from the center (expressed as position angles), the data for opposite sectors being combined into means. Compared with this dotted curve it is apparent that the thousand brightest stars, represented by the full line, show little trace of the elliptical distribution.

Of this group of bright stars, 130 have negative color indices. They correspond, at least approximately, to the B-class stars in our galactic system, which show marked galactic concentration. Their distribution is plotted in figure 1 as a broken line. Though the accidental variations are influential, because of the relatively small numbers concerned, the elongation is definitely shown and the degree of concentration appears to be about double that for the faint stars of all color classes.



Full line, brightest thousand stars; broken line with crosses, blue stars; dotted line, all stars between magnitudes 17 and 19. Ordinates, percentage deviation from the mean; abscissae, angle of direction from the center.

There are seven variable stars in the Hercules cluster. Four of them, including the two that are known certainly to be Cepheid variables, are in the sectors that contain the axis of symmetry, and one is in an adjoining sector. As some allowance for the inclination of the supposed plane to the line of sight must be made, this proportion is as much as could be expected, even if all these are Cepheids and in their distribution a strict comparability to the condition in our system is required.

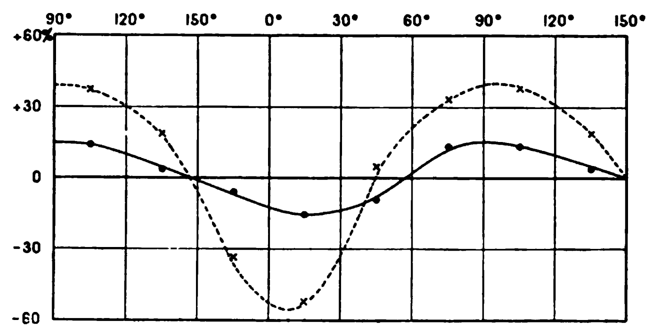
The elliptical form of ω Centauri, the largest and brightest globular cluster in the sky, is immediately evident upon photographs that show several thousands of its stars. The apparent elongation was mentioned by Bailey in discussing the variable stars,[4] but was not considered in his discussion of the distribution.[5] That the star counts, as published by Bailey, verify the appearance of the photographs and show a distinctly elliptical symmetry, has been noted in earlier papers.[6]

The data upon which this conclusion was based are now given in the table, the thousand stars nearer the center than 3′ being excluded because of the difficulty and consequent uncertainty in counting and arranging the data in sectors. The successive lines of the table show that

THE DISTRIBUTION OF 5000 STARS IN ω CENTAURI

WIDTH OF RING	SECTORS												MEAN
	15°	45°	75°	105°	135°	165°	195°	225°	255°	285°	315°	345°	
3′ to 6′........	148	129	151	182	165	159	164	173	203	182	198	163	168
6 to 9........	84	89	151	142	139	113	105	119	153	163	125	117	125
9 to 12........	66	81	96	74	64	79	64	68	88	97	74	68	77
12 to 15........	44	54	61	61	57	57	40	59	57	59	51	45	54
3 to 9........	232	218	302	324	304	272	269	292	356	345	323	280	293
9 to 15........	110	135	157	135	121	136	104	127	145	156	125	113	130
3 to 15........	342	353	459	459	425	408	373	419	501	501	448	393	423
Variables...............	8	6	17	12	9	5	2	16	11	17	16	9	11

the elliptical distribution is present at all distances from the center. The irregularities and other evidence of the lack of exact symmetry in this cluster, as well as in others, probably represent actual conditions; their significance may be much the same as that of the open star groups and the breaks in the Milky Way in the symmetry of our own galactic system.



The full line shows the distribution of all stars, the minimum corresponding to the direction of the cluster's galactic pole. The dotted line shows the distribution of variables. Ordinates, percentage deviation of the number of stars in each sector from the mean for all sectors; abscissae, angle of direction from the center, expressed as position angle.

There are 128 variables (nearly all short-period Cepheids) in ω Centauri. Their distribution is given in the last line of the table, and their relation to the axis of symmetry is shown in figure 2. In the diagram, numbers for opposite sectors are combined and the percentage deviations from the means are plotted together with a similar curve for the five thousand stars of all kinds outside the central area. The preference for the sectors that lie along the axis of symmetry is very conspicuous, the relative amplitudes of the two curves showing that these short-period variables are three times as condensed toward the supposed plane of symmetry as the stars in general. A correction for superposed stars increases the general ellipticity by about one-tenth of its amount.

The hundred or so brightest short-period Cepheids (cluster-type variables) in the galactic system do not show a marked concentration to the plane of the Milky Way. This result is not conclusive, however. There are hundreds of known variable stars in low galactic latitudes whose periods and variations are not yet recognized. If, as may well be the case, these prove to be short-period Cepheids which appear faint because of distance, though intrinsically fairly luminous, our ideas relative to their galactic concentration will need modification. In fact, we probably have much more complete information about the Cepheid variables in ω Centauri than in our own system, and for the former the information is also homogeneous.

Summary. (1) When based on photographs with exposures long enough to show ten or twenty thousand stars, the study of stellar distribution in the so-called globular clusters reveals an underlying elliptical symmetry that may be universally present. (2) The 130 brightest stars of color class B in Messier 13 show a distinct preference for the sectors containing the axis of symmetry, which was previously found for faint stars but is not shown by the thousand brightest objects of all color classes. (3) The Cepheid variables in Messier 13 also aline themselves along this axis. (4) The southern cluster ω Centauri shows a conspicuous elliptical distribution, even when only the brightest 5000 stars are examined. (5) The 128 short-period variables in this cluster show a much higher concentration toward the axis of symmetry than do the other stars. (6) Because of the analogous condition in our Galaxy, the peculiar concentration of blue stars and variables strongly supports the hypothesis that these axes of symmetry in reality represent the projections of more or less oblate systems of stars; and it indicates that in this flattened form, which appears not only as a characteristic of various kinds of nebulae, but also of the solar system, of the whole galactic system, and now even of globular clusters, we have a property that is general and fundamental in the dynamics of stellar groups.

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[3] *Ibid.*, page 87.
[4] Bailey, S. I., *Ann. Obs. Harvard Coll., Cambridge*, 38, 1902, (1–252), page 5.
[5] Bailey, S. I., *Astr. and Astroph., Northfield, Minn.*, 12, 1893, (689–692).
[6] Shapley, H., *loc. cit.*, and *Observatory, London*, 39, 1916, (452–456), page 456.

ZUÑI CHRONOLOGY

By Leslie Spier

AMERICAN MUSEUM OF NATURAL HISTORY, NEW YORK

Communicated by H. F. Osborn, February 19, 1917

In connection with ethnological studies in the southwestern United States, the American Museum of Natural History is conducting an archeological survey of the same area. This provides the necessary background for a complete understanding of the ethnography of the region. While much energy has been expended in the Southwest, chiefly on the description of cultures, little progress has been made in the delineation of its culture-history through want of a chronology for the region.

The problem takes definite form when related to the ethnologic study of the social structure of the Zuñi tribe now being made by Prof. A. L. Kroeber of the University of California. It is conceivable, for instance, that a far-reaching effect may have resulted from the concentration of the Zuñis into a single huge community from their several villages of early historic times (the famous 'Seven Cities of Cibola'). What can archeological method tell us of the former groupings of these people, of their ultimate origin and their relations with other peoples? The answer to the problem rests on the establishment of a chronology which will order the confused mass of cultural data into a culture-history.

The possibility of attacking the problem in the vicinity of Zuñi itself was made clear by the richness of the finds of the Hemenway expedition of 1888 under Cushing, Bandelier, and Hodge, by a later hasty reconnoissance of the region by Fewkes in 1890,[1] and finally by Kroeber in a suggestive study incidental to his ethnologic work in 1915 which indicated the main direction the chronology would take.[2] The present study was undertaken by the writer in the summer of 1916.

The pueblos of the Zuñi tribe occupy a strategic position for the study of the Southwest. They lie on the headwaters of the Little Colorado River in central western New Mexico, occupying a central location in the area. The two great areas of ruined pueblos lie one along the northern margin of the Southwest culture province in the drainage of the San Juan, the other along its southern margin in the Gila and Salt River watersheds. To the east, the occupied villages of the Pueblo Indians stretch along the Rio Grande, and from that center an extension of the modern culture including Zuñi stretches westward into northern Arizona. We now have Nelson's partial chronology of

the Tano tribe[3] and Kidder's suggested sequences for the Pajarito plateau[4] and for Pecos pueblo,[5] all on the Rio Grande. A chronology from the western section of the Pueblo area is therefore a desideratum.

The procedure involved in this task presents some novel aspects. Simultaneously in 1915, N. C. Nelson, A. L. Kroeber and A. V. Kidder arrived by three different inductive methods at the same conclusion, viz., that the characteristic Southwestern pottery was of the highest evidential value for the purpose of reconstructing the sequence of pueblo occupation, or, in the widest sense of the term, establishing a chronology. Their methods of reconstruction were respectively stratigraphic observation of refuse deposits, the hypothetical ranking of surface finds and the observation of concurrent variations, and the hypothetical seriation of the several pottery techniques. Of these three methods, the advantages rest with the first, but its application in the case of Zuñi finds was precluded by the shallowness of the refuse deposits at most of those ruins. The writer was therefore led to combine the two methods of stratigraphic observations and hypothetical ranking of surface finds.

Where stratigraphic observations could be made it was observed that the variable value of one fluctuating pottery type, 'corrugated' ware (so named for its characteristic plastic decoration), could serve as an index of stylistic variations in the whole pottery art. On analyzing for their type content the samples of potsherds collected from the surface of ruins for fifty miles up the Zuñi valley and from their refuse deposits, it was found that the resulting data fell into two groups. In both groups corrugated ware was present in the samples in amounts varying from 2% to 60%, but in each group the accompanying wares differed in type or in proportions, or both. For instance, samples of sherds collected from two ruins showed corrugated ware present in each to the extent of 40% of the whole pottery art, but associated with this at one ruin was 60% of a white ware, while at the other 23% of red ware and but 37% of white were found. For synthetizing these data we had at hand three premises: some of the ruins were claimed by the Zuñi as their former villages and others were so mentioned in historic records, the whole group of ruins stand somewhat isolated from other groups in the Southwest, and finally, the decorations on the pottery found in them had sufficient individuality to set off the whole group from the rest of the Southwestern ruins.

To synthetize these data we had two guides. It became clear while making stratigraphic observations at the historic ruins that while corrugated ware is represented to the extent of only 2% in historic ruins

and in modern Zuñi, it had played a larger rôle in the pottery art of earlier times. Similarly, the oldest remains in the valley showed an analagous variation in the use of corrugated ware, but in the opposite direction suggesting its coming predominence at a later period. Assuming then that we had here the end and the beginning of a stylistic pulse —an assumption made strongly presumptive by observed sequences in other sections of the Southwest—we ranked the data according to the ascending values for corrugated ware up to the maximum and then in descending order to the values for the recent villages. The validity of such procedure lies in the observed seriation of the accompanying wares: when a series of three or more distinct, but mutually dependent values are ranked according to some postulated sequence for one, and the other values are found to present serially concurrent variations, it may be safely concluded that the result is other than fortuitous. The results in this case amply justified the assumption. The fluctuations in the ceramic art of the Zuñi thus stand revealed and are directly translatable in chronological terms.

The results obtained from the application of this chronology were the following:

1. The chronological scale itself shows a sequence of the pottery types, or rather of the predominating wares in the order of white, corrugated, red, black, and with the last buff and white. Of these black is a dull unsliped ware, corrugated shows the familiar indented coils, and the decorated wares are black-on-white, black-on-red, black and white-on-red, brown-on-buff, and brown and red-on-buff, and the modern varieties of white ware. The sequence of the techniques parallels Nelson's for the Tano ruins in its general outlines: two color and later three color painted ware, two color glazed ware, three color combination glazed and painted ware, and modern painted wares, with corrugated ware appearing at all times and black ware in later periods.

2. The Zuñi valley has been occupied continuously from an early period. The occupation has been transitory, no site being occupied for any considerable period. Certain major shifts of population are observable however: first to the northeast and east barely crossing the continental divide, later a return to the Zuñi basin still occupied by the Zuñis.

3. The Zuñis first occupied single houses and later communal dwellings o the well-known Pueblo type. The former are uniformly small, the latter rectangular or circular structures of considerable extent. It is not clear that the first type developed into the communal dwelling. This sequence of architectural types has been often postulated by older writers, but never proved.

The problem has then found a partial solution, inasmuch as we have traced the growth of Zuñi communities and the ancient isolated character of their habitat. On the other hand the evidences for cultural isolation lie only in the development of such items as pottery designs of specifically Zuñian character, the general nature of their culture-history as a whole showing a common growth with their Rio Grande neighbors.

The few data now at hand for the Southwest suggest a marked uniformity of culture throughout that area from the earliest times, with the gradual individualization by the several tribes of certain minor culture traits.

Whatever its specific results, this study has shown that the method of assumed seriation can be applied to archeological phenomena. So far as the method is concerned, the novelty lies in its application to American culture-history.

The full data will be published in the *Anthropological Papers of the American Museum of Natural History.*

[1] Fewkes, J. W., *J. Amer. Ethn. Arch., Boston*, **1**, 1891.

[2] Kroeber, A. L., *Anthrop. Papers Amer. Mus. Nat. Hist., New York*, **18**, 1916; these PROCEEDINGS, **2**, 1916, (42–45).

[3] Nelson, N. C., *Amer. Anthrop.*, N. S., **18**, (159–180).

[4] Kidder, A. V., *Memoirs Amer. Anthrop. Assn.*, **2**, (407–462).

[5] Kidder, A. V., these PROCEEDINGS, **2**, 1916, (119–123).

THE AGE OF THE BOLIVIAN ANDES

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Communicated by H. F. Reid, February 26, 1917

During the joint explorations of Messrs. J. T. Singewald, Jr., and B. L. Miller in South America extending over several months in 1915 fossil plants were collected at two localities in the highlands of Bolivia. One, an entirely new locality at Corocoro[1] near the western edge of the *altiplanicie* or high plateau of Bolivia and the other at Potosi in the *Cordillera Real* or Eastern range of the Andes, from which fossil plants had previously been described by both Engelhardt[2] and Britton.[3]

In the series of volcanic tuffs which contain the fossil plants at the latter locality and from a slightly lower level a few marine fossils were collected and as the age of these tuffs has never been determined and as they throw an unexpected light on the age of the eastern range of the Bolivian Andes and of the extensive mineralization of that region a brief preliminary announcement seems desirable.

Although the textbooks tell us that the Andes date from Cretaceous

times the evidence is accumulating that their final elevation was only accomplished in the late Tertiary or even in the Pleistocene and there is some evidence that this upward movement is still going on. Thus Steinmann considers the dioritic rocks of the copper belt as of late Tertiary age and Stille[4] states that the uplift of the eastern cordilleras in Colombia must have been in the late Tertiary because of the part taken in the movement by the Miocene Honda beds. Neither Engelhardt nor Britton in their studies of the fossil plants from Potosi venture beyond Tertiary in their age determinations. In my own work I have always regarded this flora as probably of Pliocene age because of its resemblance to the existing flora in this general region. The collections made by Singewald and Miller but emphasize the opinion that the flora is very young and I am not yet sure that it will not eventually have to be considered Pleistocene.

More spectacular than the floral evidence is that of the fauna which is found in the same series of tufaceous materials at a somewhat lower level. The only determinable form is a new species of *Discinisca* which has been determined by Professor Schuchert, who will describe it in the final report on this region. Professor Schuchert states that this form is related to the existing *Discinisca lamellosa* (Broderip) which is found in shallow water along the west coast of South America from Panama to Chile, and that it cannot be older than Miocene and may be Pliocene or Pleistocene.

The extreme youthfulness of these beds indicated by the Brachiopod and confirmed by the more extensive evidence furnished by the flora shows that the sea deposited a part of these strata in late Tertiary or Pleistocene time and since that time there has been differential vertical movements amounting to a minimum of 13,500 feet.

The fossil plants denote a much more humid climate than prevails today in this region. For example at Corocoro which now lies at a little over 13,000 feet above sea level, the country is practically a treeless desert. The fossil plants from this locality include a fern (*Polystichum*), fruits of *Terminalia* and *Copaifera*, both tropical trees of the eastern subandean hills and Amazon Basin; leaves of *Mimosa arcuatifolia* Engelhardt, *Mimosites linearis* Engelhardt, *Acacia uninervifolia* Engelhardt, and *Cassia ligustrinoides* Engelhardt, the last four common to Potosi and suggestive of the existing deciduous forests of the so-called Pantanales region of the eastern plains of Bolivia.

If the moisture carrying winds were from the east at that time as they are at the present time, the lowering of the eastern Andes would enable such a flora to flourish in the present inter montane region.

The flora from Potosi is extensive and not yet fully elaborated. It includes about sixty species and the following genera are represented: Acacia, Acrostichum, Amicia, Caesalpinia, Calliandra, Capparis, Cassia, Copaifera, Cuphea, Dalbergia, Desmodium, Drepanocarpus, Enterolobium, Escallonia, Euphorbia (?), Festuca, Gaylussacia, Gymnogramme, (?), Hedysarum, Inga, Lomariopsis, Lonchocarpus, Machaerium, Mimosa, Mimosites, Myrica, Myrteola, Passiflora (?), Peltophorum, Pithecolobium, Platipodium, Poacites, Podocarpus, Polystichum, Porliera, Ruprechtia, Sweetia, Terminalia, and Weinmannia.

A perusal of these genera, already recognized, is sufficient to convince any botanist or indeed any visitor to the region, that this flora is very different from that now found in the Potosi region. While the botanical exploration of the present Bolivian flora leaves much to the future it is obvious that if we seek for representatives of this fossil flora in the recent flora of Bolivia, nearly all the genera are to be found represented in the more or less well watered country east of the present eastern range, and particularly on the lower eastern slopes. Moreover most of the fossil species are very close to still existing species of the latter region and this resemblance is so close that I cannot conceive of this flora being older than Pliocene.

There is then definite evidence that parts of the high plateau and of the eastern Cordillera stood at sea level in the late Tertiary.

[1] Singewald, J. T. Jr., and Miller, B. L., *Engin. Min. J.*, *New York*, 103, 1917, (171–176).

[2] Engelhardt, H., *Dresden*, *Sitz-Ber. Isis*, 1887, Abh. 5, (36), 7 figs.; *Ibid.*, 1894, Abh. 1, (1–13), 1 pl.

[3] Britton, N. L., *New York*, *Trans. Amer. Inst. Min. Engin.*, 21, 1893, (250–259).

[4] Stille, H., Geol. Studien im Gebiete des Rio Magdalena, *von Könen Festschrift*, 1907, p. 356.

LARGE CURRENT-RIPPLES AS INDICATORS OF PALEOGEOGRAPHY

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Communicated by J. M. Clarke, January 28, 1917

In the Eden and in parts of the Richmond Group (Upper Ordovician) large ripples, measuring 50 to 150 cm. from crest to crest, are rather common throughout the region of the Cincinnati Anticline, in Kentucky,[1] Indiana,[2] and Ohio.[3] From a careful study of very numerous rippled layers of these formations in southwestern Ohio and north-central Kentucky, of 13 rippled layers in the Brassfield formation of east-central Kentucky[4] and of one in the Blackhand formation (Mississippian) of eastern Ohio,[4a] the following data were obtained:

1. The crests of all large ripples are broadly rounded like the troughs. More than half of all seen in limestones of the Ordovician and Silurian of the Cincinnati Anticline were more or less symmetrical; the others are distinctly asymmetrical.

2. Not one showed any sign of assortment. Shells of Rafinesquina, over 5 cm. long and wide, Bryozoans 8 to 10 cm. long and over 1 cm. thick, and, in the Richmond, calices of Streptelasma, over 10 cm. long and over 3 cm. wide are found scattered equally over crests, sides and troughs of the ripples, mixed in a most any proportion with finer shell fragments down to the finest matrix filling the interstices. The same broadly rounded, symmetrical crests with complete absence of assortment of any kind were observed in ripples measuring 145 cm. from crest to crest in the conglomeratic Berne member of the Black-hand formation. Here, pebbles of ± 0.5 cm. diameter are uniformly mixed with coarse and fine sand.

In the growth of oscillation ripples, the to and fro motion of the oscillating current produced by waves on the bottom of a water body involves a constant tossing of the grains. This results in a sifting and complete assorting of the grains. Its absence in the large ripples in question proves that they have not formed by the action of waves, but of some current. Clues to its nature are found in the following observations.

3. In Kentucky the Brassfield formation of the Silurian east of the Cincinnati Anticline shows one or two rippled layers within its 18 feet of thickness. West of it no traces of ripples were found according to Foerste.[5] The ferruginous oolitic facies of the same formation is also limited to the east side of the anticline, extending over a distance of nearly 120 miles from Madison County, Kentucky, to Clinton County, Ohio, in a belt running roughly north-south (perhaps slightly east of north). On the west side of the Anticline nothing but a salmon brown color of the limestone betrays the (relative) neighborhood of ferruginous deposits.[6] From this the inference appears justified that the shore-line of the Brassfield sea was somewhere to the east with a general north-south trend.

Of the 13 measured exposures of rippled layers in the Brassfield, ranging over a distance of nearly 50 miles, 12 showed directions of strike between N 50 W and N 110 W, averaging N 76 W, i.e., at right angles to the direction of the assumed shore-line. The current, therefore, must have run parallel to this shore-line. This excludes the undertow and similar currents from discussion.

4. Large current-ripples are found only on rocks of relatively coarse

grain, as conglomeratic sands (Blackhand formation) or fragmental limestones, never on fine-grained sediments, e.g., on dense blue argillaceous limestones. These are, however, frequently covered with small current-ripples, ranging in wave-length from 1 to 30 cm., and are often interstratified with fragmental limestones of coarse grain covered with large ripples and separated from them only by thin layers of shale. The calcareous layers show delicate tracks of gastropods or trilobites well preserved which practically exclude any current action.

This seems to indicate that the current in question varied in intensity from a maximum to nil, in relatively short intervals. The finer sediments could record only the weaker movements, as stronger currents would have thrown them into suspension.

5. In the Ordovician, I have repeatedly found large asymmetrical ripples on two successive limestones, not more than a foot apart, with nearly the same strike, but with their lee sides facing in opposite directions. The current, therefore, reversed its direction in relatively short intervals.

Observations 4 and 5 exclude ocean currents of larger dimensions, while they point consistently to tidal currents. These, too, are the only marine currents, flowing parallel to the shore-line, in which velocities of at least 1 m/sec., which seem necessary to produce the effects observed, are found over large areas.

In 1901 Cornish[7] described from the English Coast large current-ripples corresponding closely to those observed in the fossil state. On open shores, such as at Mundsley (Norfolk, p. 183), above the mouth of Barmouth Estuary (p. 173), or especially on the Goodwin Sands (p. 189), about six miles off the shore of Kent, these tidal ripples invariably trend at right angles to the shore, often at right angles to the waves. On the open shore, too, their wave-length is the same as that of most large Paleozoic ripples, while those observed in estuaries, where the velocity of the tidal current is greatly increased, have a greater wave-length.

I therefore infer that the large current-ripples described were produced by tidal currents. Those of the Brassfield formation in Kentucky offer a direct analogy to those of the English Coast.

The ripples of the Lorraine and Richmond Formations, however, offer an additional problem.

1. They are not limited to a relatively narrow zone in the neighborhood of the shore, but formed (probably more or less synchronously) throughout the area of the Cincinnati Anticline, that is, over an area of at least 15,000 square miles and probably much more.

2. They trend in all directions, although a north-south trend is more common than an east-west trend.

At first sight this seems to offer a serious objection to my interpretation, since in open waters the direction of the current passes through all the points of the compass in the course of twelve hours, which would render the formation of permanent ripples impossible. The following observations, however, offer a clue to this problem.

In 1881 Hunt[8] visited the broad open gulf of Torbay on the south shore of Devonshire two weeks after a heavy storm. In Midbay, at a depth of over 12 meters, where the bottom usually is a soft muddy sand that clogs the dredge in a few minutes, he found the ground hard, producing "not a single shell or a particle of the usual muddy sand." Four weeks after the storm "the ground was still very hard, both the dredge and a fishing-lead tied to a line *bumping along as though over ridges*." Over six weeks after the gale the same spot had returned to its normal state.

Similarly, Cornish[9] found Pegwell Bay (Kent), in which ordinarily the tide never produces anything but small current ripples, covered with large tidal ripples after a heavy gale blowing into the bay.

These observations indicate that the drift produced by periods of storms may so strengthen the tidal current as to produce large current ripples. This I suggest as the probable origin of our large Eden and Richmond ripples.

The ripples observed by Hunt formed at a depth of over 12 meters with a tide of over 2 meters. In open waters the range of the tides and the velocity of the resulting currents would be much smaller than in the channel. With gales of similar strength, therefore, the same mechanical effect of the currents would be possible only at a much smaller depth of water. Allowing, however, for extreme conditions, we may safely say that it is probable that our Ordovician ripples formed in water less than 25 meters deep rather than more. The Persian Gulf offers an interesting analogy. With an area of about 90,000 square miles, it has a mean depth of but 25 meters.[10] The tidal range along all its shores is 3 to 3.75 meters.[11]

The fact that at least three independent factors must combine for the production of these ripples, namely sufficiently strong tidal action, storms, and small depth of the water, explains why such large current ripples are not found in other seas, the sedimentary record of which is otherwise almost identical; e.g., certain parts of the Middle Triassic Muschelkalk of Western Europe.

A fourth factor of no less importance is brought out by consideration of the following facts:

1. The formations of the Upper Ordovician consist essentially of the following sedimentary units, interstratified in irregular order: (a) Calcareous shale, varying from highly fossiliferous to barren. (b) Dense, blue, argillaceous limestones, mostly barren (except in many cases the surfaces of the layers). (c) Fragmental limestones, varying from fine-grained to regular shell breccias.

2. None of these units have a great horizontal extent, i.e. all formed simultaneously on different parts of the sea bottom.

3. The shale may have any thickness, while the limestones are seldom over one foot thick.

4. Single valves of brachiopods and isolated joints of Crinoids are common in the shales.

5. The dense argillaceous limestones almost always show delicate cross-bedding.

6. Current-rippled fragmental limestones are always overlain by shale.

7. In fragmental limestones thin and delicate shells and skeletons are commonly found broken, but the fossils in general show little wear.

8. Large current-ripples are found in such formations as show a predominance of shale over limestones, e.g., the Eden and parts of the Richmond formations. They are absent, however, where limestones predominate, especially in the Fairview and McMillan formations.

9. Fragmental limestones dominate in the Fairview and McMillan formations and show ample evidence of a stirring of the sediment, often of violent character, as e.g., the Rafinesquina breccias of the Bellevue horizon.

From these data I conclude that the typical sediment of the Upper Ordovician seas in the region which now forms the Cincinnati Anticline was a clay with an abundant animal population, varying in density from place to place. The almost constant agitation of the sea bottom (obs. 4 and 5) furnished the fine shell powder which mixed with the clay, gives it its calcareous character.[12] Densely populated areas supplied it, especially after periods of storms, to adjoining areas of scarce population in sufficient quantity to form argillaceous limestones. This areal relation explains why argillaceous limestones are generally poor in fossils if not entirely barren.

During exceptional periods of storm many feet of the muddy sediments were thrown into suspension. In order to cloud water 20 to 50 meters deep with sediment, as has been observed repeatedly in the

channel and on the banks of Newfoundland[13], a considerable thickness of sediment must be stirred up. The heavier particles, shells and skeletons, however, are concentrated on the bottom, drifting with the current and eventually thrown into large current ripples. The relatively short duration of this current action explains observation 7. The suspended clay settled later, forming the layer of shale overlying the rippled limestone layers (observation 6). Since the thickness of sediment which could be thrown into suspension during such storm periods is limited, the thickness of the resulting fragmental limestone is limited, while shale on shale could accumulate indefinitely as long as no abundant animal population settled on it (observation 3). The rippled layers were preserved if sufficient shale was added between two exceptional storm periods to prevent the stirring of the sediment from reaching down to it. A decrease of the clay supply, however, would reduce the rate of sedimentation and allow ordinary storm waves to expose the fragmental layer, which, under the action of the shifting normal tidal current, would suffer a surficial redistribution of material, resulting in complete leveling of the surface. A sufficient rate of sedimentation must, therefore, be considered as a fourth factor determining the formation of large current ripples (observation 8).

From the above it follows that the rough rhythm of sedimentation, shown by the recurrence of fragmental limestones between shales and argillaceous limestones, is due to the interference of two, probably not strictly periodic processes: the shifting of the centers of animal population on the sea bottom and the occurrence of storm periods of exceptional violence.

If the interpretation presented above is correct, the geographic conditions indicated by the large current ripples of the Upper Ordovician of the Cincinnati Anticline may be summarized as follows:

1. A sea having sufficient connection with the open ocean to allow relatively high tides.

2. Sufficient area to permit the formation of strong winddrifts in most directions during periods of storms.

3. A depth small enough to admit of a strong action on the bottom sediments by winddrift and tidal current combined, probably 25 meters or less on the average.

4. Atmospheric conditions providing for the occurrence of storms, blowing from all points of the compass, such as tropical cyclones or those of intermediate latitudes.

[1] Linney, W. M., *Rep. Geol. Garrard Co.*, 1882, (16); *Washington Co.*, 1882, (10–11); *Lincoln Co.*, 1882, (13); *Mason Co.*, 1885, (8); *Bath and Fleming Cos.*, 1886, (10, 62–69); *Shelby*

Co., 1887, (6). Knott, W. T., *Rep. Geol. Marion Co.*, 1885, (11). Foerste, A., in Kindle, E. M., *Chicago, J. Geol. Univ. Chic.*, 22, 1914, (709–711).

[2] Shannon, W. P., *Indianapolis, Proc. Acad. Sci.*, 1895, (53–54). Moore, J., and Hole, *Ibid.*, 1902, (216–220). Culbertson, G., *Ibid.*, 1903, (202–205).

[3] Locke, J., *Ohio Geol. Surv.*, 1838, (246). Orton, *Rep. Geol. Surv. Ohio*, 1, 1873, (377). Perry, N. W., *Amer. Geol., Minneapolis*, 4, 1889, (326–336). Foerste, A., *Chicago, J. Geol. Univ. Chic.*, 3, 1895, (50–60, 169–197). Prosser, C. S., *Ibid.*, 24, 1916, (456–475).

[4] See Foerste, A., *Kentucky Geol. Surv., Bull.*, No. 7, 1906.

[4a] Dr. J. E. Hyde called my attention to this very interesting occurrence.

[5] Foerste, A. F., *Amer. J. Sci., New Haven*, (Ser. 4), 18, 1904, (321–342).

[6] See Foerste's Map of the distribution of the facies of the Clinton formation.

[7] Cornish, V., *London, Geog. J.*, 18, 1901, (170–202).

[8] Hunt, A. R., On the formation of ripple marks. *Proc. Roy. Soc.*, 34, 1882, p. 4.

[9] L. c., p. 190.

[10] Supan, A., *Grundzüge der Physischen Erdkunde*, 1911, p. 260.

[11] Kruemmel, O., *Handbuch der Ozeanographie*, 1911, vol. II, p. 383. See also Berghaus, A., *Atlas der Hydrographie*, 1891, Pl. XX.

[12] To what extent inorganic or organic precipitation may have played a rôle I am in no position to judge. Observation 7 seems to point to it as a source of finely divided calciumcarbonate.

[13] Krümmel, O., *loc. cit.*, p. 112.

THE BEARING OF SELECTION EXPERIMENTS WITH DROSOPHILA UPON THE FREQUENCY OF GERMINAL CHANGES

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Communicated by C. B. Davenport, March 5, 1917

Biologists generally agree that changes in the germ plasm do occur. On the other hand, there appears to be considerable disagreement regarding the frequency of these spontaneous changes. Some experiments, as illustrated by the work of Pearl, Hj. Nilsson, De Vries, Tower, and Johannsen,[1] are more easily analyzed by supposing that changes in the germ plasm occur very rarely in comparison with the number of generations of individuals studied, so that the origination of new races by selection is not generally possible. Other experiments, notably those of Castle, Smith, Middleton, and Jennings (on *Difflugia*),[2] have been interpreted as showing that such changes are occurring so frequently that they may be found in each generation, and so afford a basis for selection to make continuous progress. As long as different experiments lead their authors to such different conclusions, no broad generalizations as to the scope of the evolutionary significance of selection may be drawn without the most intimate and critical consideration of all other related investigations, and, accordingly, all additional evidence that may be secured has an important bearing.

The familiar Mendelian units are currently conceded to arise sud-

denly, as conspicuous changes, or mutations. But the question is not settled as to how frequently, after their appearance, further changes may occur in these units. It is upon this special phase that the results here presented have their bearing.

Most banana flies (*Drosophila ampelophila*) have four conspicuous bristles on their backs. A mutation occurred which permitted more than the normal four bristles to develop in this special region of the back. From one pair of flies produced by germ plasm bearing this mutation, an extra-bristled race was established (MacDowell)[8]. This race was distinguished from the normal wild race by one Mendelian unit, as was shown by crosses which gave first generations with no extra bristles and second generations (F_2) in which one quarter of the flies had extra bristles. The number of extra bristles in this race was variable; experiments showed that this was largely due to the amount of food eaten during the development of the flies. Large flies, those from flourishing culture bottles, had numerous extra bristles, while small flies, those from mouldy, or old, dried up cultures, had few, or even no extra bristles. But even when no extra bristles developed, these flies, when given fair breeding conditions, produced offspring all of which had extra bristles. To discover any changes in the inherited basis of this character (extra bristles), selection for increased bristle-number was carried on for 49 generations. During this time every mating was made between brothers and sisters in pairs. Considerably over 100,000 bristle-counts form the basis of the following discussion.

Two conditions must be met before selection can modify the means of a race. First there must be genetic differences between individuals, and second, these differences must be manifest to some extent in the somatic structures of the individuals. In other words there must be some tendency for extreme variates to bear extreme germ plasm. To measure this relation between soma and germ plasm, the coefficient of correlation is especially fitted. It is an expression of the degree of similarity existing between parents and offspring. All the data obtained from the inbred extra-bristled race were cast into tables correlating the grades of the parents and their offspring in each generation. The coefficients calculated from these tables show that there is an unquestionable positive correlation between the grades of the parents and offspring in the first six generations; the coefficients for these generations are statistically significant. In the subsequent generations no further evidence of any positive correlation is to be found that has biological significance. The coefficients are sometimes plus and other times minus, but without any consistency or regularity. In most cases they are not

significant in comparison with their probable errors; in some cases they are clearly significant. This means that in most cases there was no tendency for high grade parents to have higher grade offspring than low grade parents. In individual generations higher parents did produce offspring of somewhat higher grade, but then in others higher parents actually produced offspring of lower grade than did the lower grade parents. These facts indicate great independence between the grades of parents and offspring in all but the early generations; they lead one to expect to find that the selection of high variates during the early generations has modified the means of the race, but that later on the same process has failed to make further progress. What actually happened is shown in figure 1, which represents the means of the high selected race.

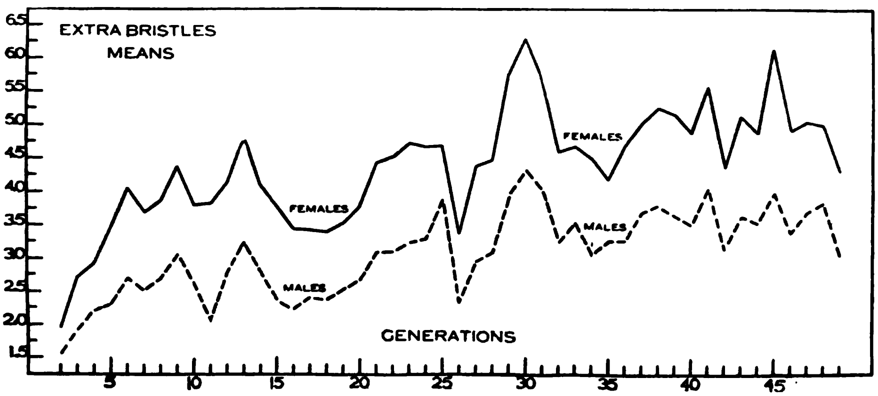

FIG. 1. MEANS OF THE OFFSPRING FROM 49 GENERATIONS OF SELECTION FOR INCREASED NUMBER OF EXTRA BRISTLES

Due to a change in method the means in generations following the 29th tend to be higher, and therefore can not be compared with the preceding ones.

The means of the males are consistently lower than those of the females, but the close parallelism of the curves serves to substantiate the correctness of the means as descriptions of the different generations. As expected, the means steadily rise for the first few generations; after this they fluctuate. It will be noted that the highest point in the curves appears in the 29th generation; this high point immediately follows the removal of the breeding bottles to a constant temperature room in which dry summer heat was automatically maintained. Moreover, due to a change in method, in the subsequent generations the means tend to be higher than those in the generation preceding the 29th. It remains, then, to compare the means before and after the 29th, separately. Where the means show regular advance, the correlation coefficients

are regularly positive and significant; where the means are irregulars and seem to depend mainly upon environment, the correlation coefficient, are irregular, plus and minus, and in general not statistically significant. It is important to understand that this failure of selection can not be explained by physical incompatibility in the soma. There is obviously plenty of room for many more bristles than are generally found and, in a few cases where very rare conditions happened to be experienced by individual flies, such extreme grades as 12, 13 and 16 extra bristles have been found, whereas the most frequent high extreme is 9 extra bristles, and the characteristic mean of the race seems to be 4.75. It may be concluded that at first there were differences in the germ plasm of the different flies and these differences were manifest in the somatic appearance, even though environment was also causing variations; later, due to the selection, such germinal differences between individuals as were clearly observed at the beginning were no longer found.

As further evidence for this conclusion the following experiment may be described, which meets the objection that the narrow range of parents selected in the later generations might account for the absence of correlation. Accordingly, selection was suspended for three generations and as far as facilities would permit, all the progeny of one pair of flies in the 50th generation were bred in pairs for three generations without respect to their grades, the only restriction being that matings were made of flies of the same or within one of the same bristle grade, and only virgin females were used. All the offspring were counted and graded. In the second generation of this experiment over 4000 flies were graded, in the third generation, 27,000.

For these generations the correlation coefficients were as follows:

Males—2d generation,	$r-0.1436 \pm 0.0995$;	3d generation, $r-0.0271 \pm 0.0391$
Females—2d generation,	$r-0.1378 \pm 0.0997$;	3d generation, $r-0.0221 \pm 0.0391$

This shows that the higher grade parents did not have any tendency to produce higher grade offspring than the lower grade parents, and therefore, that the failure of selection to produce further changes in the means of the race, is due to the absence of such genetic differences, as were originally present.

From quite a different source comes evidence that the early generations of selection modified the genetic constitution of the race. Starting from the second selected generation a race of low grade flies was formed in one generation by selecting low grade flies as parents. The means of this race were entirely distinct from those of the high selected race; as the high race rose, this race remained low. Since it is known

from the correlation coefficients, that the higher grade flies were producing higher offspring at this time, it is entirely within expectation to find that the lower grade flies could produce lower grade offspring. On the other hand, when flies of similar low grades were selected from the 16th generation of the high selected race, and the selection of low grade parents continued for 8 generations as a return selection series, the means of the offspring were not significantly lower than those of the corresponding generations in the high selected race. In fact the successful result of selecting low grade parents of the same grades in the second generation was not even approximated. The correlation coefficients indicated that flies with different grades were not differentiated genetically in the 16th generation; these breeding tests support the same conclusion. The line from low parents started from the 16th generation, was lost after 8 generations; since low grade flies are the smaller ones, this selection, besides separating low grade flies, isolated weak ones. This accounts for the reduction of vigor and the final loss of the race. A second return selection was started from the 27th generation of the high race. For three generations the means of this line closely resembled those of the high race, but the weakening effect was soon manifest in the small numbers of offspring and the lowered means, and shortly, in the loss of the race through the failure of the selected flies to reproduce.

A still further test has been made of the conclusion already stated. If the dissimilar genetic behavior of the unselected and the long selected flies rests on a sorting out of differences in the germ plasm, it should be possible to bring back these differences by crosses with flies that might be supposed to have germ plasm still bearing these differences. Flies from the 17th generation were crossed with wild normals; from the extra-bristled flies that appeared in the second generation, low grade parents were selected. These *at once* established a low grade race that gave means constantly and unquestionably lower than the high selected race; no variation in the environment was great enough to confuse them. This low race did not show any signs of weakening on account of the selection of low grade flies; it was continued for 19 generations and gave large families. The reason for this is that all the flies, even the large well fed ones, had fewer extra bristles than the flies in the uncrossed high selected race; their germ plasm was different. It is very evident that the results of this crossing can not be explained on the basis of non-genetic physiological causes, as Castle[4] has suggested may explain the very closely parallel results of his crosses with self-colored and hooded rats. In spite of this recent interpretation, it is well to note that Castle has given unmistakable evidence that his crosses involved genetic modi-

fication. By plus selection from hooded rats that came from a cross between the minus selected race and wild, the minus race was immediately transformed into the plus race, whereas similar plus selection from the minus race *without* the cross with wild, required 6 generations to move the means even up *to* the "0" grade.

There are then, four lines of evidence that support the conclusion that selection, in the case of extra bristles has separated genetic differences, or units, that existed at the beginning of the experiment, and that did not reappear *de novo:* (1) at first correlation coefficients are constantly positive and significant, later they are mainly not significant and fluctuate between positive and negative; (2) selection advances the means at first, later it does not; (3) before selection makes its advance, return, or low, selection separates a distinct race, while afterwards, the same procedure fails to accomplish the same result, however, (4) if a cross with normal precedes, the low selection becomes as effective as it was at the beginning.

As the material under discussion does not constitute a pure line in any sense, the conclusions drawn do not have any immediate bearing on the pure line theory. Moreover, environment plays such a conspicuous role that nothing positive can be said about any hypothetica variations in the germ plasm that may be hidden by variations in the environment, and accordingly be too small to be determinable. It may be suggested that if the environment were not such a controlling factor, or if it were possible to reduce the variability of the environment, smaller changes in the germ plasm might be discovered. This claim may be made for any work in selection that could be imagined; no failure of selection, however stationary the environment may seem to be, can escape this claim, namely that variations in the germ plasm may be taking place, although they are not distinguishable in the soma. The claim may be made, but it will give neither the breeder, nor natural selection any opportunity to make progress. It appears that instead of offering a fatal objection to the work here reported, the evident environmental factor serves well to emphasize the utter futility of attempting to deal in theory or fact with supposed germinal phenomena that can never be demonstrated or utilized. The variations in environment commensurate with the viability of the fly are great, but they do not hide the fact that there are germinal differences in regard to the numbers of extra bristles; they do not hide the fact that two races may be raised at the same time under the same conditions and maintain their individuality; nor do they explain the fact that after selection has made a certain amount of progress it is no longer possible to raise the means of

the race any further and no longer possible to separate two distinct races by selection without a cross with normal.

The conclusions that logically follow from the preceding discussion are that (1) extra bristles are primarily occasioned by one germinal unit and further influenced by other germinal units, and (2) that no change that could have either evolutionary or practical significance has occurred in these units during the 50 generations of the experiment.

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[4] Castle, W. E., and Wright, S., *Washington, Pub. Carnegie Inst.*, No. 241.

PRESSURE PHENOMENA ACCOMPANYING THE GROWTH OF CRYSTALS

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Communicated by J. C. Branner, February 26, 1917

Under suitable conditions crystals grow in directions in which growth is opposed by external force. This fact appears to have been first observed by Lavalle in 1853.[1] It was denied, however by Kopp, who, after making certain experiments, stated that he was never able to observe anything tending to confirm the view that a crystal can raise itself in order to grow also on the side on which it rests.[2] Subsequently the observations of Lavalle were confirmed by Lehmann[3] and others.

Becker and Day seem to have made the first attempt at determining the magnitude of the force developed during crystal growth. In their experiment, a crystal of alum supporting a weight was covered with a saturated solution of alum, and supersaturation was induced by evaporation. The crystal increased in size through growth on the lateral exposed faces which were also extended downward, thus lifting the crystal together with its load. The deposition of new material on the lower surface was restricted to the periphery, so that a hollow face was gradually formed by the downward extension of the new growth, and the crystal rested on a very narrow outer rim. The area of this rim was determined with difficulty, but repeated measurements led to the conclusion that "the force per unit area which the crystals exert . . . .

is of the same order of magnitude as the ascertained resistance which the crystals offered to crushing stresses."[4]

In 1913 Bruhns and Mecklenburg published a paper[5] in which they claim to have repeated the experiment of Becker and Day with negative results. In a reply to this paper Becker and Day explain the different results obtained by Bruhns and Mecklenburg as due to the fact that the original experiment was not duplicated.[6] This conclusion was reached independently by the present writer.[7] By placing an unloaded crystal in the same solution with the loaded one supersaturation with respect to the supporting surface of the loaded crystal was prevented.

Bruhns and Mecklenburg[5] (pp. 106–108) describe another experiment in which the crystallization of chrome alum resulted in raising porcelain fragments, loaded with weighted beaker-glasses, but they thought it essential that evaporation be carried to completion. The elevation of these beakers and similar evidences of pressure phenomena accompanying crystal growth they attribute to the "forces of adsorption and capillarity" and not to a "force of crystallization." Becker and Day show that in this experiment the lifting action occurs in spite of capillary attraction rather than because of it, and that adsorption merely diminishes the rate of growth by delaying diffusion.[8]

As a result of their investigations Becker and Day conclude: (1) that there is "a linear force, apart from the volume expansion, exerted by growing crystals;" (2) that this force enables them to grow in directions in which growth is opposed by external force "notwithstanding unrestricted opportunity for growth in other directions; and (3) that the linear force thus exerted is of the order of magnitude of the breaking strength of the crystal."[9] "The crucial experiment [briefly described above] offered in support of this conclusion" is not, in the opinion of the present writer, decisive. It is significant that no growth occurs on the upper face of the crystal although it is subjected to less pressure than the lower face, and crystallographically both are the same. The elevation of the crystal is due to the fact that it rests directly on a thin layer of solution which is supersaturated by diffusion from without; and in this experiment the solution at the bottom of the crystallizing dish is of higher concentration than elsewhere. Furthermore, the effect of expansion in volume is not eliminated in this experiment, for alum separates from solution with increase in volume.

A crystal is enlarged through the addition of layers of material to its outer surfaces, and this takes place when these surfaces are in contact with a supersaturated solution. A very thin coating of impervious material is sufficient to prevent crystal growth. In the Becker and Day

experiment the supporting edges of the crystal rest directly on a thin layer of solution which is therefore under pressure. Now pressure tends to reduce the thickness of the supporting film to a minimum. With perfectly smooth parallel surfaces the minimum thickness of the separating film is perhaps equal to the diameter of the space occupied by a molecule of the liquid, and this is probably approximated in the present case, for deposition would be most rapid where the thickness of the supporting film is greatest. It would require great pressure to completely expel the solution from such a narrow space, and, if the solution is thus excluded, growth in a vertical direction would cease. The fact that this growth continues is proof that the solution is not expelled from under the crystal. Therefore when alum separates from solution and is added to the base of the crystal, the accompanying increase in volume must result in some elevation, irrespective of other causes.

The cavity or hollow formed on the under side of crystals in the experiment described above results from malnutrition, due to the slow rate of diffusion under the crystal as compared with the relatively rapid growth in other directions. The writer succeeded in eliminating this cavity by supplying the supersaturated solution through capillary openings under the base of small crystals. Since new material can be added only at the base, its area remains small and tall slender columns are formed.

The pressure phenomena observed during the growth of crystals have been attributed by the present writer "to the molecular forces associated with the separation of solids from solution and to the attraction and orientation of the physical molecules as they are brought into position on the surface of a growing crystal"[7] (pp. 553–554). He suggested "that the force is due chiefly to the expansion in volume which accompanies the separation of most solids from solution, for, as yet, he has obtained no pressure effects during the crystallization of substances that separate from solution with decrease in volume."[10] Recently, however, he has obtained definite evidence of pressure accompanying the crystallization of ammonium nitrate.[11] The experiment is decribed below.

A cup of porous porcelain was placed bottom up in a small jar half full of a concentrated solution of ammonium nitrate, and a piece of paraffine-coated card-board, cut to fit snugly around the cell, was cemented with paraffine to the top of the jar and to the walls of the cup. The jar was then placed in a desiccator containing calcium chloride, and allowed to stand undisturbed for nine months. The solution was drawn by capillary attraction to the upper and exposed walls of the cup where a crust was gradually formed by evaporation, but this crust was en-

larged chiefly or entirely through the addition of new material to the outer exposed surfaces, the solution reaching these surfaces through capillary pores in the crystalline mass. Under the card-board, however, a few long acicular crystals were formed on the surface of the cup, and these were gradually pushed outward by the addition of new material to their base. At one place the solution penetrated the card-board which was gradually split apart by the slow growth of a lens-shaped veinlet of finely crystalline salt about 3 mm. in thickness. The cup was not broken as in similar experiments with certain other salts that separate from solution with increase in volume.

The pressure phenomena observed in this experiment are explained as follows: Crystallization is retarded or prevented in supersaturated solutions which occupy small capillary or subcapillary spaces; therefore crystals may be supplied with material for growth by diffusion through solutions occupying such spaces, and the increase in volume due to the entrance and deposition of new material must result either in the expulsion of part of the solution or in the enlargement of the space occupied by the growing crystals. If the spaces occupied by the solution are relatively large, the solution will be gradually expelled as crystalline matter is deposited in its place, but, on the other hand, if the spaces are sufficiently small, less force may be required to enlarge the space occupied by the growing crystals than is necessary to expel the solution. The diffusion of a solid through a solution and its separation therefrom are attributed to osmotic pressure and the relation between osmotic pressure and solution pressure. According to the writer's theory, the force observed in this experiment with ammonium nitrate is analogous to the pressure developed when an anhydrous salt, confined in a limited space, combines with water that has diffused as vapor through capillary openings.[12]

Molecular attraction between solid and liquid causes the thin layer of solution in contact with a crystal to adhere to it; and this contact film, on account of adsorption, is of different concentration from the bulk of the solution. Enlargement or solution of a crystal is brought about by the diffusion of dissolved substance across this layer. When the solution in contact with a crystal surface is supersaturated with respect to that surface, new material is deposited on the crystal, thus forcing the contact film to move outward from the growing crystal. If this film approaches a foreign body, growth in that direction is gradually retarded as the space through which diffusion must supply new material becomes more limited. Consequently growth will be more rapid in other directions, providing diffusion is not similarly restricted, and the

crystal will tend to surround the foreign body. Deposition of material between the crystal and the foreign body will continue, however, as long as diffusion can maintain supersaturation in the solution occupying this space; and, with continued crystal growth, the contact film must continue to be displaced. When this film comes in contact with the foreign body, any further growth must result in either (1) the displacement of the foreign body or (2) the rupture and expulsion of the film; and the outcome will depend on the resistance offered by the foreign body, the dimensions of the space occupied by solution and the mutual attraction between the molecules of the liquid and solids.

If the crystal is of a substance that goes into solution with decrease in volume, increased pressure will make it more soluble, thus increasing the degree of concentration requisite for further growth; but for those salts that have been tested a large change in pressure is required to produce an appreciable change in solubility. If the solubility of the foreign body is increased by pressure, it may be gradually removed in solution as the growing crystal replaces it.

The tendency of a crystal to assume a regular polyhedral form is important as a factor in the development of pressure during crystal growth only in so far as it affects the relative solubility of the crystal in different directions. A crystal growing in a solution of uniform concentration tends to build that form which is the least soluble under existing conditions, or, in other words, for which the total surface energy is a minimum. If the surface tension were the same in all directions this form would be a sphere, but in crystals the surface tension differs in different directions, or, on different faces, and is the same only on faces that are crystallographically the same. A solution that is in equilibrium with the flat face of a crystal will be supersaturated with respect to a concavity on the face and undersaturated with respect to a convexity. When a crystal having a concave face or an artificially truncated angle is placed in a solution of uniform concentration which is kept saturated with respect to the normal crystal faces, growth may be limited to a single direction until the imperfection is repaired, but growth can not continue indefinitely in a single direction as the superficial area and hence the surface energy would increase too rapidly in proportion to the volume. The variation in the solubility of a crystal in different directions is slight, and therefore it is probable that the difference is small in the pressure that may be developed in different directions by a growing crystal in contact on all its surfaces with a solution of uniform concentration. The surface tension on the different faces of a growing crystal probably depends on many factors, such as, the number of molecules per unit

area, their structure and orientation, pressure, temperature, and the composition of the solution. Hence, different faces may develop under different conditions of growth.

Most of the phenomena hitherto cited in support of the hypothesis that there is a 'linear force of crystallization' are to be explained by the fact that the growing crystals have been in contact with a supersaturated solution in only one direction or that the concentration of the solution has been greater in one direction than in others. It is probable that the pressure effects observed during crystallization are due chiefly to the separation of solid matter from solution rather than to the growth of crystals, and, under favorable conditions, the pressure developed in this way may greatly exceed the crushing strength of the substance.

Crystals grow in directions in which external forces oppose growth whenever the surfaces under pressure are in contact with a film of supersaturated solution, and it is possible to supply the material for growth by slow diffusion through subcapillary spaces, as great resistance is offered to the expulsion of solution from such openings. The conditions requisite for the growth of crystals under pressure commonly obtain in the rocks of the earth's crust, and many phenomena connected with the metamorphism of rocks, the growth of concretions, and the formation of mineral deposits are difficult of explanation under any other hypothesis than that growing crystals have made room for themselves by exerting pressure on the surrounding material.

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[9] *Ibid.*, (313).

[10] Taber, Stephen, The origin of veins of the asbestiform minerals, these Proceedings, **2**, 1916, (662).

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[12] For description of this experiment see The genesis of asbestos and asbestiform minerals by Stephen Taber, *Bull. Amer. Inst. Min. Eng.* No. 119, p. 1987, Nov. 1916.

A NEW METHOD OF TRANSFORMING ESTERS OF α-AMINOACIDS INTO THEIR CORRESPONDING ISOTHIOCYANATES

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Communicated by L. B. Mendel, February 28, 1917

In a previous paper from this laboratory,[1] the writer called attention to the chemistry of polyketide mustard oils of the type $C_2H_5OOC.CHR.NCS$, and indicated the need of a practical method for preparing such combinations in quantity for synthetical work. This problem was undertaken by us and a method of synthesis was developed whereby isothiocyanates of the above type can be prepared directly from esters of α-aminoacids by subjecting the latter to the action of thiophosgene in dry toluene solution. For example, ethyl aminoacetate, in the from of its hydrochloric acid salt, interacts smoothly with thiophosgene in boiling toluene giving an excellent yield of the corresponding mustard oil—ethyl isothiocyanacetate. The transformation is expressed by the following equation: This reaction

$$CSCl_2 + HCl.NH_2CH_2COOC_2H_5 = 3HCl + SCN.CH_2COOC_2H_5,$$

has now been applied successfully in other cases and the results obtained will be published later in the *Journal of the American Chemical Society*.

While the above method of synthesis offers no difficulty so far as manipulation is concerned, it does involve in its application, however, the used of thiophosgene, which is an unpleasant reagent to work with and requires much time to prepare in quantity. We have now developed a method of operation whereby we are able to convert esters of α-aminoacids into their corresponding isothiocyanates without the use of this disagreeable reagent. In this preliminary note we record the principle of this method and its application for the preparation of ethyl isothiocyanacetate.

Emil Fischer[2] has shown that the ethylester of glycocoll interacts quantitatively with carbon-bisulphide in ether solution to form the dithiocarbamic acid salt represented by Formula I. The reaction is expressed by the following equation:

$$\begin{array}{rl} CS_2 + 2NH_2CH_2COOC_2H_5 = & NH.CH_2COOC_2H_5 \\ & | \\ & CSSH.NH_2CH_2COOC_2H_5. \quad (I) \end{array}$$

As far as we are aware, this is the only ester of an α-aminoacid that has

been coupled with carbon-bisulphide in this manner. Fischer attempted to convert this salt into ethyl isothiocyanacetate by digestion in aqueous solution with silver and mercury salts (Hoffmann's mustard oil reaction). He found that it underwent decomposition by such treatment giving solutions having the odor of a mustard oil, but ethyl isothiocyanacetate was not isolated by him.

We now find that this dithio carbamic acid salt I of Fischer's can be transformed easily and almost quantitatively into ethyl isothiocyanacetate by application of an unique reaction discovered by Andreasch.[3] This investigator has shown that simple amine salts of the above type interact with ethyl chloroformate at ordinary temperature giving smoothly isothiocyanates with evolution of carbonoxysulphide and ethyl-alcohol. The reaction is expressed by the following general equation:

$$R.NH.CSSH.NH_2R. + ClCOOC_2H_5 = RNCS + COS + C_2H_5OH + R.NH_2.HCl$$

Fischer's glycocoll-dithiocarbamic acid salt I also interacts with ethyl-chloroformate according to the above expression giving an intermediate thiolester having the Formula II. Therefore the first phase of this transformation may be expressed by the following equation:

$$\begin{array}{l} NHCH_2COOC_2H_5 \\ | \\ CSSH.NH_2.CH_2COOC_2H_5 + Cl.COOC_2H_5 = \\ \quad \text{(Fischer's Salt)} \\ NH.CH_2COOC_2H_5 + C_2H_5OOC.CH_2NH_2.HCl \\ | \\ CSS.COOC_2H_5 \end{array} \qquad \text{(II)}$$

This thiolester displays greater stability than combinations of this type, which have hitherto been described. Below 100° the ester II is quite stable. If heated above this temperature in a vacuum, it undergoes a smooth decomposition. Carbonoxysulphide is copiously evolved together with alcohol, and nearly a theoretical yield of ethyl isothiocyanacetate is produced. The mustard oil agrees in all its properties with the isothiocyanate prepared by the thiophosgene method, and boils at 102–104° at 10 mm. pressure. The second phase of this interesting reaction is expressed by the following equation:

$$\begin{array}{l} NH\cdot CH_2COOC_2H_5 \\ | \\ CSS\cdot COOC_2H_5 \end{array} \rightarrow COS + C_2H_5OH + SCN\cdot CH_2COOC_2H_5$$

Starting with 35 grams of ethyl aminoacetate we recovered practically one-half of the aminoacid ester in the form of its hydrochloride, and obtained 19 grams of the isothiocyanacetate. This reaction is being investigated further and will be applied for the preparation of other new types of polyketide mustard oils. If this method of synthesis finds as wide an application as we anticipate, it will enable us to obtain several isothiocyanates of new types, which should be of great biochemical interest.

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THE GEOLOGY OF THE FIJI ISLANDS

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Communicated by W. M. Davis, February 28, 1917

The period between July, 1915 and March, 1916 I spent as a Sheldon Travelling Fellow of Harvard University, in a study of the geology of the Fiji Islands. Special attention was given to the structure and relations of the elevated limestones. Of the larger islands, Viti Levu, Vanua Levu, Taviuni, Kandavu, Mbengha, and Ovalau were visited. Three islands of the Yasawa group and eighteen of the Lau group were likewise studied. The following paper records the principal facts concerning the geology of the major divisions of the group.

1. Viti Levu.—Viti Levu is the southern of the two larger islands of Fiji. It is 94 miles long from east to west and 55 miles broad. The southeastern side of the island and a large portion of the eastern and northeastern sides are low delta flats overgrown with mangrove bushes. The flats merge into a young, narrow coastal plain which extends 5 to 10 miles inland, where it meets an uplifted coastal plain of soft marls, now carved into mature hills, 70 to 100 feet in height. The older plain slopes gradually upward for 5 or 6 miles from its edge, until it rests unconformably on the interior, volcanic hills at heights of 600 to 700 feet. Elsewhere the volcanic hills approach the shore and form the coastline for the greater part of the circumference of the island.

The whole interior of the island is characterized by maturely dissected insequent hills, though quite extensive flats are sometimes found near the rivers. The rocks forming the interior hills are frequently sandstones and marls in contrast to the volcanic rocks of the coast. The western and northern shores of the island are very irregular. Drowned valleys abound and many are so filled with delta deposits that

even small boats find difficulty in entering the river-mouths through the tangle of mangrove bushes.

The geological history of the island is more complex than has been previously stated.[1] From a hasty survey of the interior of the island I inferred that an old land of slates and red sandstones was intruded by a batholithic mass which solidified as gabbro, diorite and granite. The mountain block thus formed was deeply eroded and the igneous rocks were exposed. Volcanoes then burst forth and a series of andesitic and rhyolitic flows were poured out over the eroded surface.

The aggrading flows were later deeply eroded and submerged. A coraliferous conglomerate was laid down on the submerged surface and, during a period of oscillatory movements, several hundred feet of marl and claystones with occasional thin seams of coal were deposited. This period was followed by one of more decided submergence, during which approximately 150 feet of coraliferous limestones was laid down. The entire series will be spoken of, for reasons which will appear in the following paragraph, as the 'folded sediments.'

Compressive stresses were then developed in this portion of the earth's crust. The limestones and older rocks were uplifted and sharply folded along lines running in general N. N. W. by S. S. E. The folding was so intense that the limestones were occasionally transformed to marble. A period of faulting and volcanism followed during which the island assumed somewhat of its present form, and a series of volcanic hills were built up near the coast. Erosion and submergence then allowed marls and interbedded coraliferous limestones to be laid down uncomformably as a thin veneer about the edge of the island. These sediments will be spoken of as the 'coastal series.'

Again the island was differentially uplifted. The wide lagoon extending northwest toward the Yasawa islands was initiated by tilting in that direction and, at the same time, the coastal plain at the eastern side of the island was elevated. During the latter part of the epoch, volcanic rocks were injected into the coastal series and the Yasawa islands were built up. Recent differential movements have uplifted the southwestern shore of Viti Levu and at the same time depressed the Yasawa islands. In general the present coral reefs are developing on platforms which originated during the deposition of the coastal series.

2. *Vanua Levu*.—Vanua Levu, the northern of the two larger islands of Fiji, is 104 miles long from east to west and 20 to 25 miles in width. The eastern portion of the island is split by a long, narrow bay extending inland from northeast to southwest for 50 miles. To the eastern peninsula, thus formed, are appended several peninsulas which jut

out to the eastward. The whole circumference of the inland is marked by pocket harbors and jutting headlands.

Along the southern and eastern sides of the island a rough country of volcanic hills extends 6 or 8 miles inland. The principal divides are found in these hills. The larger rivers run north and northwest across elevated plains 200 to 300 feet high before forming flood-plains along the northern coast.

The geological history of the island is not so complex as that of Viti Levu. The fundamental rocks of the island are not exposed, to the writer's knowledge. Their character is, therefore, problematic. Into these rocks were intruded batholithic masses which solidified to form gabbro. The old land was then greatly eroded and the plutonic rocks were exposed. Later the island subsided. During the erosion and subsidence volcanic intrusions mantled the irregular land surface unconformably with a cover of acid andesites and rhyolites.

The island remained submerged throughout the period of sedimentation and folding in which the folded sediments of Viti Levu were developed. Submarine volcanoes were active and 2000 to 3000 feet of ash and agglomerate, largely composed of hypersthene andesite, were conformably laid down on the submerged surface. At the time of the uplift of the coastal series of Vitu Levu, Vanua Levu was elevated into an island form, The movements were accompanied by faulting and tilting and have continued to the present.

The period of uplift has been complicated by downward movements from time to time which may best be studied near Lambasa on the northern coast. During a still-stand a local peneplain was developed far into the interior which now is found at an elevation of 50 to 200 feet. The island was later uplifted and the peneplain just described was carved into sub-mature hills near the coast. A period of submergence followed during which coraliferous limestones were deposited about the spur-ends of the coastal hills. The region was then elevated and the limestones are now found from 50 to 100 feet above the sea. A recent submergence has allowed volcanic silts, brought down by the rivers, to fill up valleys eroded in the limestone. Since the uplift a series of basaltic eruptions have scattered their debris over the surface of the island but at a period so remote that the basaltic cones are now eroded to sub-maturity.

Elevated coral reefs are known from but one place along the northern coast. They occur at an elevation of 75 feet, just west of Lambasa. They are found within the bay separating the main island from the eastern peninsula[2], but are not known to occur on the peninsula itself. Reefs have very recently been elevated along the southeastern coast

and are found at an elevation of 75 to 100 feet. So recently have these reefs been uplifted that the lagoon flat and outer barrier reef are still preserved intact.

I visited only the eastern and central portions of Vanua Levu. The modern fringing reefs are here developing either along the shore-line of recently submerged volcanic rocks or on coastal flats formed of the fine ash swept from the elevated hills of submarine tuffs. The most recent movements have been differential and while uplift has taken place at the southeastern side of the island, subsidence has occurred to the east and north. The modern barrier reefs occur where subsidence has taken place either due to tilting or faulting during uplift.

3. *The Lau Islands.*—The Lau Islands lie east of the larger islands of Fiji and are scattered over 300 miles of the ocean floor from the 16th to the 20th parallel of south latitude and from the 178th to the 179th meridian of west longitude. There are from 40 to 50 islands ranging in size from a half a mile to 20 miles in length. The islands may be grouped empirically in three classes.

1. Islands composed of volcanic rocks and limestone.
2. Islands composed of limestone alone.
3. Islands composed of volcanic rocks alone.

The volcanic islands usually have a number of separate peaks rising to altitudes of 600 to 900 feet, whereas the islands composed of limestone rise to nearly a constant level about their edges but are often depressed toward their center.

The limestone is always coraliferous and is raised to a maximum elevation of 1030 feet in the island of Vatu Vara. In every case studied the limestone rests unconformably on a basement of eroded volcanic rocks, indicating subsidence. Certain of the elevated limestones have the atoll or barrier reef form and the inference is drawn, as no unconformities are found within the limestone, that the elevated atolls were formed during the subsidence of the underlying volcanic surface.

The corals included in the elevated limestones are Pleistocene or Recent in age according to the determination of Dr. T. W. Vaughan.

Many of the elevated limestone islands have been greatly reduced in size by atmospheric solution. Occasionally, as in the cases of Fulanga and Ongea, sea-level flats have developed. In most of the islands the flats have been submerged and the residual masses of limestone dotting the flats have formed undercut islets scattered over a lagoon 10 to 15 fathoms in depth. I believe the submergence is due to actual subsidence since it is more recent than the return of the waters to the ocean after the Glacial epoch, and since Pleistocene wave erosion would

have destroyed the fragile undercut islets if the lagoon floors from which they rise were fashioned by this agency.

The islands originated about the middle of the Tertiary period during the extrusion of the lavas of the second andesitic period of Viti Levu. The volcanoes thus formed were maturely eroded and subsided. Four or five hundred feet of coraliferous limestone were deposited unconformably on the subsiding surfaces and were later elevated. A second period of erosion followed and again basaltic volcanoes built cones on the surfaces of the eroded limestone. Within quite recent times the islands have subsided 50 to 90 feet and the modern coral reefs are developing on the eroded and submerged platforms.

4. Summary of the Theoretical Results of the Expedition.—My views concerning the origin of the barrier reefs and atolls of Fiji may be summarized as follows:—

1. The elevated reefs were deposited in all known cases unconformably on eroded surfaces of volcanic rocks. Atolls and barrier reefs were among the forms developed during this period of subsidence.

2. Many atolls and barrier reefs are now developing on platforms of elevated limestone which have been eroded to fairly even surfaces by atmospheric solution and later submerged.

3. Of the several theories concerning the origin of barrier reefs and atolls only Darwin's theory postulates a subsiding surface eroded above sea-level. But this theory is firmly based on the conception of progressive, though intermittent, subsidence of large segments of the earth's crust. If such subsidence is conceived to have begun in the early Tertiary period, the writer is convinced that it characterized the larger part of the Tertiary and Pleistocene periods in which the elevated limestones of Fiji developed. But since the Pleistocene period the algebraic sum of the movements has been positive and uplift has resulted, although the sum, if reckoned from the early Tertiary, is negative and the ultimate result has been subsidence. The present reefs are, however, dependent upon the Pleistocene and Recent movements for their form. Such movements have been progressively upward with only minor periods of subsidence. They are also local in their effect and are dependent undoubtedly on volcanic activity, in part due to the transfer of material from the inner to the outer portions of the earth's crust, and in part to the secular cooling and consolidation of the extruded lavas.[3,4] Hence it cannot be said that the modern reefs of Fiji fully support Darwin's theory.

4. The data assembled by Daly[4] and Vaughan[5] convince the writer that Pleistocene platforms exist very generally throughout the coral seas.

Yet while this is true, the platforms in Fiji are post-Pleistocene in their development. The writer was unable to discover any evidence of Pleistocene wave-cut platforms.

[1] Woolnough, W. G., *Sydney, Proc. Linn. Soc. N. S. Wales*, **32**, 1907 (431–474).
[2] Guppy, H. B., *Observations of a Naturalist in the Pacific*, vol. 1, Macmillan, 1903.
[3] Gerland, G., *Beitr. Geophys., Leipzig*, **2**, 1895, (56).
[4] Daly, R. A., *Boston, Proc. Am. Acad. Arts Sci.*, **51**, 1915, (157–251), p. 232.
[5] Vaughan, T. W., *Washington, J. Acad. Sci.*, **6**, 1916, (53–66).

DOMINANCE OF LINKED FACTORS AS A MEANS OF ACCOUNTING FOR HETEROSIS

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Communicated by W. M. Wheeler, February 26, 1917

The increase of growth derived from crossing in both animals and plants, which has been called heterosis, and the converse fact of decreased vigor resulting from inbreeding have been known for a long time but have never been satisfactorily accounted for.

The investigations of East,[2] G. H. Shull[3] and Hayes[4] show that inbreeding does not result in a continuous degeneration but that the effects of inbreeding gradually become less as complete homozygosis is approached and for all practical purposes finally become constant. Unlike strains are isolated which differ in the amount of growth they produce. In many species these homozygous strains are always less vigorous than either parent. The decrease in vigor due to inbreeding has been shown to be correlated approximately with the decrease in the number of heterozygous factors present but without showing why there should be such a relation. It was simply stated that "greater developmental energy is evolved when the mate to an allelomorphic pair is lacking than when both are present in the zygote."[4]

The conception of dominance as proposed by Keeble and Pellew[5] as a means of accounting for these facts has had two serious objections. If heterosis were due to dominance of characters it was thought possible to recombine in generations subsequent to the F_2 all of the dominant characters in some individuals and all the recessive characters in others in a homozygous condition. Such homozygous individuals could not be changed by inbreeding. Moreover, if dominance were concerned it was considered that the F_2 would have an asymmetrical distribution.

Both of the above objections to dominance have failed to take into consideration the facts of linkage. If the factors which govern an organism's development are distributed in all the chromosomes and passed

from one generation to another in groups it would be practically impossible to recombine all the dominant characters in one individual and all the recessive in another. Hence the failure to obtain both the complete dominants and complete recessives which would breed true is accounted for. This view of the situation also explains why symmetrical distributions are obtained in F_2. The development of each individual is assumed to be correlated with the number of different factors present. The individual with the greatest number of heterozygous chromosomes would have the greatest number of different factors present if the factors were distributed among all the chromosomes. The theoretical distribution of the F_2 individuals according to the number of heterozygous chromosomes contained is in the ratio of the expanded binomial $(a+a)^n$. The expanded binomial is often used as an illustration of a normal frequency distribution.

To account for the increase in growth in F_1 it is necessary to have the favorable characters for the most part dominant over the unfavorable ones. This seems probable from the numerous cases of abnormalitites which are recessive to the normal condition. It is not necessary that there should be perfect dominance. It is necessary, however, to accept the conclusion that *many factors in the 1n condition have more than one half the effect that they have in the 2n condition.*

Inbred strains of maize have been obtained by inbreeding which either lack chlorophyll entirely or are partially deficient in chlorophyll. Some strains are partially sterile. Some have fasciated ears. Some are susceptible to a bacterial wilt disease. Some have contorted stems and still others have brace roots so poorly developed that they can not stand upright when the plants become heavy. Similar instances can be cited in many naturally cross pollinated species. Some of the strains may have more than one of these unfavorable characters. No one strain so far known has them all.

Crossing these strains together gives perfectly normal F_1 plants. They are able to grow better than their parents because the characters necessary for maximum development that one strain lacks are supplied by the other and conversely. This increased growth is heterosis.

Dominance of characters gives a reason why heterozygosis should cause the F_1 generation to grow more than the parents and not less. According to previous views it would have been just as reasonable to suppose that hybridization had a depressing or an indifferent rather than a stimulating effect. It also makes it easier to understand why heterozygosis should operate throughout the life of the individual even through innumerable generations of vegetative propagation.

This conception of dominance of linked factors to account for the facts as so far known does not preclude the possibility of a physiological effect resulting from hybridization apart from hereditary factors if such an effect can be demonstrated. It simply coördinates the existing knowledge of heredity to give a comprehensible view of the way in which heterosis may be brought about.

[1] Contribution from the Connecticut Agricultural Experiment Station and the Bussey Institution of Harvard University.

[2] East, E. M., *Connecticut Agric. Exp. Sta. Rep.*, 1907, 1908, (419–428); *Amer. Nat., Lancaster, Pa.*, **43**, 1909, (173–181).

[3] Shull, G. H., *Amer. Breeders Assoc. Rep.* **4**, 1908, (296–301); *Amer. Nat., Lancaster, Pa.*, **45**, 1911, (234–252).

[4] East, E. M., and Hayes, H. K., *U. S. Dept. Agric., Bur. Plant Ind. Bull.* No. 243, 1912.

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CHEMICALLY INDUCED CROWNGALLS

By Erwin F. Smith

UNITED STATES DEPARTMENT OF AGRICULTURE

Communicated, January 26, 1917

In 1911 in Bulletin 213 on "Crown Gall of Plants: its Cause and Remedy" (Bureau of Plant Industry, U. S. Dept. of Agric.) I expressed the conviction that while the disease was clearly due to *Bacterium tumefaciens* we would eventually be able to go a step farther (l.c., p. 175) and determine just what by-products of the organism were the direct cause of the over-growth. With this end in view, on several occasions I prepared flask cultures of the organism for use of the chemist of the Department and with substances said by him to be present in the culture flasks and absent from the controls I have recently made experiments which tend to confirm my earlier supposition and expectation.

It is not maintained for a moment that these are the only substances that are able to cause overgrowths in plants but only that they are the most interesting ones in that they are the products of a cancer parasite, or, if one prefers so to express it, of a schizomycete which is the cause of a plant tumor possessing many features in common with animal cancers.

The substances produced by *Bacterium tumefaciens* in very simple culture media, *i.e.*, in flasks of distilled water containing 1% dextrose and 1% peptone with a little calcium carbonate added to neutralize any acids formed and thus to favor long continued growth since the crown gall organism is very sensitive to its own acid products, are—alde-

hyd, ammonia, amines, alcohol, acetone, acetic acid and formic acid, to which I think I may add traces of CO_2.

These substances, it will be observed, are, for the most part, just those compounds which Jacques Loeb and others have observed to be the most efficient in starting growth in unfertilized eggs of the sea urchin, to wit ammonia, amines and fatty acids (vide Loeb: *Artificial Parthenogenesis and Fertilization*. The University of Chicago Press, 1913). Their action in all probability is purely physical, that is, due to withdrawal of water from neighboring cells by increase of osmotic pressures whereupon the cells so acted upon begin to grow, at least it is possible to obtain the same phenomenon in plants with a great variety of substances, not the product of parasites and not likely to come into contact naturally with the growing tissues.

Thus far I have made no experiments with aldehyd or formic acid and have only just begun to experiment with acetone, but experimenting with the other substances named I have obtained from young tissues a prompt response in the form of over-growths. At first I used water dilutions of these substances painted on or injected, but eventually I used their vapors with marked success. The ethyl alcohol, however, was used mixed with acetic acid, and I do not yet know its unmixed effect. These experiments have been made on several kinds of plants subject to crowngall, especially on Ricinus, cauliflower and Lycopersicum.

The tumors obtained have been small because only a single application was made and consequently the stimulus was soon exhausted, but there is no reason to doubt that a continued application of these substances in high dilution, after the manner of the parasite itself, would result in tumors essentially like those occurring naturally in crowngall, or resulting from our bacterial inoculations.

These tumors are either vascularized hyperplasias, mixed hypertrophy and hyperplasia, or simple hypertrophies. In them the cells are much more closely compacted than the parent cells and free from chlorophyll. The cells of the hypertrophies are frequently a hundred times the volume of the cells from which they have originated. In the acetic alcohol tumors there has been a great increase in the number of the cells, *i.e.*, the development of a true hyperplasia, while in the hypertrophies the component cells appear to be the original cells greatly enlarged.

Curious vascular displacements and duplications have also been ob-

tained including in one instance an entire extra vascular cylinder in the pith of Ricinus.

A fuller account accompanied by photographs and photomicrographs will be published in the *Journal of Agricultural Research.*

[Since this was written I have also obtained small overgrowths on cauliflower leaves with both formaldehyde and formic acid but not with vapor of ethyl alcohol or of acetone. E. F. S.]

DYNAMICAL SYSTEMS WITH TWO DEGREES OF FREEDOM

By George D. Birkhoff

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Communicated by E. H. Moore, March 12, 1917

The present note contains a brief summary of a paper with the same title which is about to appear in the *Transactions of the American Mathematical Society.*

The equations of motion of the dynamical system under consideration are taken in the variational form due to Lagrange

$$\frac{d}{dt} L_{x'} - L_x = 0, \quad \frac{d}{dt} L_{y'} - L_y = 0,$$

where x, y represent the two coördinates of the system and where L is a quadratic function of their time derivatives x',y'. By an appropriate change of variables the principal function L is reduced to

$$L = \tfrac{1}{2}(x'^2 + y'^2) + \alpha x' + \beta y' + \gamma,$$

and the equations of motion then take the simple form

$$x'' + \lambda y' = \gamma_x, \qquad y'' - \lambda x' = \gamma_y, \qquad (\lambda = \alpha_y - \beta_x),$$

involving only the two arbitrary functions λ, γ, of x, y. In the *reversible* case, i.e., the case when linear terms are lacking in L, we have $\lambda \doteq 0$. The normal form just written is known in this special case, but is new in the general case so far as I have been able to determine. Any conformal transformation of the variables x, y joined with the corresponding transformation of t leaves this normal form unaltered.

By means of the transformation theory thus obtained it is established that a necessary condition for the existence of an integral linear in x', y' is that the curves λ/γ = const. form an isothermal family in the x, y-plane When this condition is satisfied suppose the particular transformation of the variables x, y to be made which takes the isothermal family into the family y = const.; if and only if a linear integral exists will the resulting functions λ, γ become functions of y alone; and in this case

the equations of motion can be directly integrated. As a second application of the transformation theory the equations of displacement are derived.

For reversible problems it is known that the equations of motion can always be interpreted as those of a particle constrained to move on a fixed smooth surface. In the irreversible case I have been able to show that x, y may be regarded as the coördinates of a particle constrained to move in a smooth surface which rotates uniformly about a fixed axis and carries with it a conservative field of force.

Among all types of orbits the most fundamentally important are those which are periodic.

The existence of periodic orbits is intuitively evident in the reversible case, when the orbits may be looked upon as geodesics. If proper restrictions are imposed a closed geodesic will exist along which the arc length is a minimum, and this geodesic will correspond to a periodic orbit of minimum type. Orbits of minimum type may also be derived by means of an interesting criterion for the reversible case due to Whittaker[1] first rigorously established by Signorini.[2] Notwithstanding the fact that the integral analogous to arc length may change sign in the irreversible case Whittaker stated the direct formal extension of his criterion for this case. I have established that such an extension is legitimate if λ is of one sign, but not otherwise. This restriction on λ is fulfilled in the restricted problem of three bodies.

Unfortunately, as Poincaré pointed out, only unstable periodic orbits can be of minimum type.

Another method may be employed to find a large class of stable periodic orbits which I call of *minimax type*. This entirely new method may be stated in a special case as follows: There is a minimum length of string, constrained to lie in a given surface of genus 0, which may be slipped over that surface. In some intermediate position the string will be taut and will then coincide with a closed geodesic.

Poincaré has proved that a closed geodesic exists on any *convex* surface by an entirely different method.

A third method for the discovery of periodic orbits is that of analytic continuation. Hitherto the application of this method has been limited by the restriction that the variation of the parameter involved be 'sufficiently small.' This restriction turns out to be unnecessary in the reversible case if the orbits near any orbit always intersect it. If the conditions

$$\lambda > O, \ \gamma > 0, \ \Delta\ (\log \gamma)\ > 0$$

hold in the irreversible case the restriction is also unnecessary.

An application of periodic orbits which is fundamental lies in the construction of surfaces of section. The existence of a ring-shaped surface of this sort was noted by Poincaré in the restricted problem of three bodies and allied dynamical problems. The results of my paper show that such surfaces exist in a wide variety of cases and may be of any genus and possess any number of boundaries. By means of a surface of section the dynamical problem reduces to a one-to-one analytic area-preserving transformation of the surface of section into itself.

Periodic orbits correspond to invariant points of this transformation. Consequently the discovery of further periodic orbits hinges upon the proof of the existence of such invariant points. Two theorems concerning such points are proved by me.

The first of these theorems is the following: If a surface of genus p admits of a one-to-one analytic transformation into itself which may be looked upon as obtained by a deformation then the excess of completely unstable invariant points over all other types of invariant points is precisely $2p - 2$.

If the transformation is merely restricted to be continuous the same argument shows that there exists at least one invariant point for $p \neq 1$. For the case $p = 0$ this last result constitutes a well-known theorem due to Brouwer.[3]

The dynamical application of the result for the analytic case is to a fundamental equality obtaining between the various types of periodic orbits.

The second theorem is the following: If the region outside of a circle in the plane is transformed into itself in such a way that points on the circumference are advanced in one sense, so that points distant from the center are regressed by more than a fixed positive angle about the center, and so that areas are preserved, then at least two points are left invariant by the transformation. This theorem is to be regarded as the extension of a theorem stated by Poincaré and proved by me.[4]

These two related theorems are used to prove that infinitely many periodic orbits exist whenever a surface of section is at hand.

[1] Whittaker, *Analytical Dynamics*, pp. 376-384, *Cambridge*, 1904.

[2] Signorini, *Palermo, Rend. Circ. Mat.*, 33, 1912, (187-193).

[3] Brouwer, *Math. Ann., Leipzig*, 69, 1910, (176-180).

[4] Birkhoff, *Trans. Amer. Math. Soc., New York*, 14, 1913, (14-22).

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VOLUME 3 MAY, 1917 NUMBER 5

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OF THE

NATIONAL ACADEMY OF SCIENCES

OF THE

UNITED STATES OF AMERICA

Publication Office: Williams & Wilkins Company, Baltimore
Editorial Office: Massachusetts Institute of Technology, Cambridge
Home Office of the Academy: Washington, D. C.

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PROCEEDINGS

OF THE

NATIONAL ACADEMY OF SCIENCES

Volume 3 MAY 15, 1917 Number 5

THE LAWS OF ELASTICO-VISCOUS FLOW

By A. A. Michelson

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Read before the Academy, November 14, 1916

When a solid is subjected to a strain beyond the 'Elastic Limit,'* its behavior may be summarized as follows:

First: The application of the stress results in a rapid elastic yield, which if inertia be negligible is practically instantaneous. If the stress be now removed, the specimen returns to its former position.†

Second: This is followed by a slower yielding whose rate, if the stress is not too great, diminishes with time, and which ultimately attains a constant value which may be zero.

If the stress be now removed the specimen returns almost instantaneously to a point short of its original position, and then continues at a much slower rate and ultimately comes to rest at a point short of its original position.

If the stress is too great, the slow yield may increase until rupture occurs.‡

The following may be considered as a provisional attempt to formulate the behavior of substances under stress by the simplest expressions which have been found to satisfy all the essential requirements.

* The term 'elastic limit' is very vague and should be replaced by limits which may be characterized as follows: a. *The first limit* is that within which the specimen returns instantly to its original zero. Beyond this limit, if stress be instantly removed, the specimen promptly returns to a position short of its original one, which may be designated the 'new zero.' b. *The second limit* is that beyond which the specimen does not return to its original position or to the 'new zero,' even after a long time. c. *The third limit* is that value of the stress which produces rapid yielding or rupture.

† In many cases the time interval between application and release of stress cannot be made sufficiently short for complete instantaneous recovery.

‡ Rupture may occur in consequence of such slow yielding, or, it may be practically instantaneous. In the former case, the result is due to separation of the viscous coupling; in the second to the snapping of the spring.

The formulae which follow are in fact sufficiently general to include every case thus far examined, including materials of widely different properties, such as lead, tin, copper, aluminum, zinc, iron, steel, quartz, glass, calcite, limestone, slate, marble, wax, pitch, gelatine and rubber. It may, however, be expected that a more thorough investigation will require modification in the formulae which may be made to fit special cases with greater accuracy.

The type of strain selected for this investigation is the torsion of cylindrical rods, as this is the only strain in which the form remains unaltered. It is very probable that the laws governing this special type may be made to include other distortions such as extension, compression, bending, etc.

Very decided changes may be expected from the effects of temperature and pressure,* but these may be taken into account by an appropriate alteration in the value of the 'constants' which enter into the formulae.

The apparatus employed for the investigation consisted in a light pulley with radius of 8 cm. over which passed two cords, the ends of which carried scale pans for holding weights.

The specimen to be investigated had a diameter of 12 mm. at the ends while the intervening portion (75 mm. long) had a diameter of 4 mm. One end was clamped to the supporting frame and the other to the pulley which rests on a knife-edge in the axis.

The tests consisted in measuring the angular position of the pulley by a micrometer at intervals of one minute while under a constant torque.

Laws of Elastico-viscous Flow.—The behavior of any solid under stress may be considered as the resultant of four elements:

a. The elastic displacement,

b. The elastico-viscous displacement,

c. The viscous displacement,

d. The lost motion.

These will be considered in turn.

The Elastic Displacement.—This is characterized by being approxi-

* A preliminary investigation of the effect of hydrostatic pressure on elasticity and on viscosity was begun several years ago, which it was hoped would show results in conformity with those which maintain in the body of the earth—whose enormous pressure produces an increase in both rigidity and viscosity sufficient to make the body of the earth (which at its actual temperature under ordinary conditions would certainly be in a molten state) as solid as steel. This expectation has been partially realized for a number of materials, metallic and non-metallic; the results notwithstanding certain anomalies—traceable to the effects of previous history—showing a perceptible increase in rigidity and a very marked increase in viscosity even with the relatively small pressures obtainable in the laboratory.

mately proportional to the stress and independent of time.* A closer approximation is given by

$$S_1 = C_1 P e^{k_1 P}$$

The Elastico-viscous Displacement.—This is manifested in a slow return when the stress is removed; and it is assumed that the same forces are brought into play during the direct motion.

This displacement is represented by the formula $S_2 = A_2(1 - e^{-\alpha\sqrt{t}})$ where $A_2 = C_2 P e^{k_2 P}$.

The Viscous Displacement.—Here the elastic force is absent or very small in comparison with the viscous resistance. The specimen does not return to zero even after a long time interval.† The viscous displacement is given by $S_3 = (Ft + F_0 t_0)^\rho - (F_0 t_0)^\rho$ in which $F = C_3 P e^{k_3 P}$ and F_0 the corresponding value, when P has the value P_0 during the time, t_0.

For a specimen which has not been subjected to previous strain the formula reduces to $S_3 = (Ft)^\rho$. Experiment gives $\rho = \frac{1}{2}$ approximately, until the specimen is near the rupture point when ρ approaches the value unity.

The Lost Motion.—If the stress be applied for a short time (even a small fraction of a second) the specimen does not return to the original zero. The difference between the original and the new zero is the lost motion, L.

It seems probable that the lost motion may be considered as a function of t such as t^r, where r is very small (less than 0.02 for zinc).

If this be considered as part of the viscous term

$$S_3 = A_3 f(t)$$

then the total viscous yield may be represented by

$$S_3 = A_3[f(t) + Ct^r]$$

(If the actual stress is between the limits 0 and P_0, $C = 0$)

The Return.—If after a time, t_0 the displacement has reached the value, S, and the stress is released, the specimen promptly returns to a displacement short of zero, and continues much more slowly in the same direction.

If the elastico-viscous displacement at the time, t_0 is given by

$$S_2 = A_2(1 - e^{-\alpha\sqrt{t_0}})$$

* Doubtless there is some viscous resistance to this displacement, but it is very small.

† In some cases it may be made to return to the original position by heating, or by alternation (alternate positive and negative diminishing stresses).

the corresponding return displacement at the time, t, counted from the instant of release, will be

$$R_2 = A_2 e^{-\alpha\sqrt{t}}(1 - e^{-\alpha\sqrt{t_0}})$$

To account for the viscous term assume*

$$F = \epsilon S^n \dot{S}$$

whence

$$S_3 = \left[\frac{1}{\rho\epsilon}\int F\,d\,t\right]^\rho, \qquad \rho = \frac{1}{n+1}$$

If F = constant, and F_0 the constant value of F during the preceding stress during the time, t_0,

$$S_3 = \frac{1}{\rho\epsilon}[(Ft + F_0 t_0)^\rho - (F_0 t_0)^\rho]$$

counting from the actual zero.

As shown by the formula, if the previous strain be considerable the new strain is relatively small. This strengthening by previous strain is one of the striking features of the behavior of every substance which exhibits viscous yield.

If, in this expression, F represents the actual stress, it assumes that the viscous force is proportional to the velocity, which is true for fluids; but for 'solid friction,' the force is independent of the velocity.

It may be assumed in the present case of internal viscosity of solids that the actual law may be between these two extremes, e.g.

$$P = a\,(\dot{S})^K \qquad \text{in which } K < 1$$

This would give P^n instead of P, or, in better agreement with experiment,

$$F' = PCe^{hP}$$

The elastico-viscous term is readily obtained by making the viscosity coefficient a function of the time.

Thus, if the restoring force be represented by $a\ s$ and the viscous resistance† by $\epsilon t^m \dot{S}$, the integration gives $S_2 = S_0(1 - e^{-\frac{\alpha}{r}t^r})$ where $\alpha = \frac{a}{\epsilon}$ and‡ $r = -m + 1$.

* Experiment gives $\rho = \frac{1}{2}$, (0.3 to 0.6) which makes $n = 1$. The usual assumption, $n = 0$ gives $\rho = 1$.

† The assumptions in both viscous and elastico-viscous hypotheses made the viscosity coefficient (that is the coefficient of $\dot{S}$) zero at the beginning of the motion and infinite at $t = \infty$ which is, of course, inadmissible. Instead of S^n and t^m we might substitute $(\beta + S^n) / (b + S^n)$ and $(\gamma + t^m) / (c + t^m)$ in which β/b and γ/c are very small; but the resulting equations are far less simple and are not appreciably more accurate in expressing the results than those here given.

‡ The usual assumption, $m = 0$, gives $r = 1$.

To determine the effect of temperature, the behavior of zinc, glass, ebonite, pitch and wax was studied. The results, together with the preceding may be summarized in the following formulae:*

$$S = A_1 + A_2 T_2 + A_3 T_3,$$

$$\begin{aligned} A_1 &= C_1 P e^{k_1 P}, & C_1 &= E_0 + E_1 e^{K_1 \theta}, \\ A_2 &= C_2 P e^{k_2 P}, & C_2 &= E_2 e^{K_2 \theta}, \\ A_3 &= C_3 P e^{k_3 P}, & C_3 &= E_3 \theta (T - \theta)^{-1}, \\ T_2 &= 1 - e^{-\alpha\sqrt{t}}, & h &= b\,\theta, \end{aligned}$$

$$T_3 = \left(t + \frac{A_0}{A_3} t_0\right)^p - \left(\frac{A_0}{A_3} t_0\right)^p,\ p = p_0 + \frac{a}{1 + \left(\frac{\pi}{P\theta}\right)^m};$$

S = displacement (twist), P = applied torque,
t = time, t_0 = duration of previous stress,
θ = temperature, T = melting point,
h, k, α, E, P_0, a, b, m, π, constants.

* Instead of this series coupling, the following may be substituted: The unit consists of four elements: (1), (2) and (3) are in viscous contact with (4). (1) and (2) are in elastic coupling; and finally (3) of this unit is connected with (1) of the next following unit by an elastic coupling. The resulting formulae, however, are not essentially different from those here given.

A NEW EQUATION OF CONTINUITY

By Frederick G. Keyes

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Communicated by A. A. Noyes, February 28, 1917

This paper presents a pressure, volume, temperature relation which has been carefully compared with the greater part of the available experimental data during the past ten years. The equation, valid when only one type of molecule is present, is

$$p = \frac{R}{v - \delta} T - \frac{a}{(v - l)^2} \tag{1}$$

where $\log \delta = \log \beta - \alpha/v$, and β, α, a, and b are constants; R is the universal gas constant.

The equation is based on considerations resulting from the inferences regarding atomic structure, obtained since the discovery of the negative electron. The model atom consists of a positive central portion about

which electrons are revolving. The negative electrons are assumed to possess a charge in the aggregate equal and opposite to the positive central charge. This model atom is essentially the atom discussed by Nicholson and Bohr.

Imagine an assemblage of such model atoms contained within an envelope and subject to their mutual attractive forces. The revolving electrons of a given atom generate a magnetic field and for convenience, therefore, the revolving electrons may be conceived as circles of current about the positive central portion of the atom. The positive central portion of one such atom repels the positive charge of another atom in a manner varying inversely as the square of the distance, and similarly the negative charges repel as r^{-2}. The negative charges attract the positive also according to r^{-2}; but since, if all of the atoms were suddenly fixed in position their average distances apart would be equal, it may be assumed that the attractions and repulsions of the positive and negative charges mutually cancel. The potential due to the fields of the revolving electrons however do not cancel and therefore on the whole the resultant attraction in the assemblage of particles varies approximately as r^{-4}.

The attempt to calculate the complete expression for the force between two such model atoms is difficult and will require a more detailed knowledge of the atom. Sufficient is known, however, to make it certain that the attraction increases slightly more rapidly than r^{-4}.

An envelope of volume v and containing n particles would provide a space for the habitation of each particle on the average equal to v/n, so that the distance apart of the molecules, if a is the radius of the spherical envelope, would be equal to $\sigma = a(4\pi/3n)^{\frac{1}{3}}$. Therefore the potential energy would be proportional to $\frac{1}{2}\ \Sigma\Sigma\sigma^{-3}$ or equal to h/v where h is a suitable constant. The attraction then directed toward the center, would be per unit of surface identical in form to van der Waals' cohesive pressure a/v^2. Now if the attractive force really increases slightly more rapidly than r^{-4} as appears to be really the case, it is easily perceived that a/v^2 would only be valid at very large volumes. A simple device to correct the expression is to assume that Φ, the cohesive pressure, may be represented by $a/(v-l)^2$, where l is a small constant which will of course make Φ increase more rapidly with diminution of volume than a/v^2 in the ratio of $v^2/(v-l)^2$.

The truth of the cohesive pressure expression $\Phi = a/(v-l)^2$ can be determined only by experiment since a rigorously complete solution for the attraction of an assemblage of the model atoms appears to be impracticable. If then the volume function for Φ given, is in agreement

with experiment and the sign of l comes out positive and not negative, as there is good reason to suppose it should, increased faith in the validity of the fundamental thought will have been established.

The term $(v-b)$ of van der Waals has given rise to much discussion. It now appears that a rigorous application of van der Waals' mode of thought really leads to a different expression for the 'volume correction.' It is, however, $(v-b)$ which seems to be the corrrect form as judged by comparison with experiment. With potential forces existing between the particles, it is difficult to see how collisions occur in the sense of the classical kinetic theory, at any rate within the mass of the gas. Indeed for an assemblage of particles such as here imagined a collision, as conceived by the older theory would not occur in general, except possibly at the boundary. It would, however, still be necessary to take account of the finite size of the atoms or rather more likely a certain space the center of which was occupied by an atom. In the case of a monatomic gas this volume would be fixed and the b of van der Waals therefore a constant. The equation of state for a monatomic gas at those volumes and temperatures where association is excluded would accordingly be

$$p = \frac{R}{v-\beta}T - \frac{a}{(v-l)^2} \tag{2}$$

where β, a, l are constants and R is the absolute gas constant defined by $R = pv/\mathrm{T}$, the equation of a perfect gas.

When two of the model atoms are united to form a diatomic molecule an effect comes into play which is absent so long as monatomic particles are alone considered. A diatomic molecule is assumed to be held together by forces due to the positive and negative charges and the magnetic effect of the field due to the rotating electrons. The distance apart of these diatomic molecules in general will be large compared to the distance separating the two atoms forming the di-atom; but in an assemblage of these di-atoms, the field due to the assemblage must react to vary the distance separating the two atoms of a diatomic molecule. This would be inferred from the fact that for a magnetic shell in a magnetic field the reaction is always such as to bring about a decrease in the mutual potential energy. This decrease in the molecular case under consideration would result in a shortening of the distance between the two atoms of the di-atom or a decrease in its apparent volume as the strength of the field due to the surrounding di-atoms increased. It appears at present an impossible task to calculate exactly the apparent volume corresponding to van der Waals' b from such a physical picture.

The original mode of guessing the form of function is accordingly given.

The value sought, denoted by δ, would be a function of the volume alone and such a function that at infinite volumes it becomes constant, since when large distances separate the di-atoms the effect of the field due to the surrounding molecules would be negligible. The value of δ for $v = \infty$ let it be assumed then is β, a constant. The rate of change of δ for changes in volume would presumably be a function of the field due to the assemblage of particles and the value of δ, since the effect is due to the mutual action of the forces holding the di-atoms together and the external field due to the diatoms conjointly. This may be expressed by writing $d\delta/dv = f(\delta F)$, where F is the force. Now the simplest assumption under the circumstances is to assume that $d\delta/dv = k\delta F$ where k is a constant. The value of F taken per unit of area is approximately a/v^2 and the primitive of the equation becomes then, $\log \delta = \log \beta - a/v$ where $ka = \alpha$ another constant, and β is of course the limiting value which δ assumes when v is infinite. The equation for a diatomic assemblage of particles becomes then equation (1).

The equation (1) arrived at by such crude methods has been found to accord with the facts in a surprisingly exact manner, but in the present paper I will exhibit the evidence which has to do with the gist of the whole argument underlying the deduction of equations one and two. A later paper will present a discussion of the equation from a more general point of view including a comparison with all the available experiment data for various substances. It accordingly suffices for the present purpose to point out that the whole thought is tested uniquely by comparing a diatomic system with a monatomic system. The value of δ for the monatomic substance reduces to the constant β independent of the volume while δ becomes a volume function when the system consists of diatomic particles alone.

Recently sufficient data have appeared for argon which enables the test to be applied, while the diatomic system nitrogen will serve to test the functional form of δ. The triatomic molecule carbon dioxide appears to follow the same functional form for both gas and liquid phases. It should be emphasized that only those regions are available as legitimate for comparison purposes in which one order of molecule is present. That is to say, there must be no doubling of the argon atoms to form diatoms or association of the nitrogen molecules within the region of experimental data where comparisons are made. Such regions are fortunately a part of the experimental data contained in the literature.

It is noted that when the volume is held constant equations one and two state that the pressure will increase linearly with the temperature.

This fact was observed and discussed by Ramsay and Young for ether and carbon dioxide. Amagat thought, from an examination of his own data, that the 'law' was nearly exact in the case of many substances, certainly not true, however, in the case of the alcohols and water. Unfortunately it is necessary to have very accurate pressure measurements, to test the rule and moreover the measurements have always been made along isotherms instead of along isobars. This makes it necessary to obtain the constant volume data by graphical means with the liability of introducing a trend into the experimental data. It is comparatively easy to measure temperatures and pressures, but volume measurements under pressure are extremely difficult. Every substance which may be used to serve as a container for the experimental substance expands with temperature and pressure increase and the expansion may be moreover a complicated function of the time.

There can scarcely be any doubt that the high pressure gauge used by Amagat for the measurements above 1000 atm. was more in error than his low pressure gauge. The values of pv for nitrogen were plotted accordingly for the several temperatures from 0° to 200°, with v as one coordinate in the region up to 1000 atm. Values of pv were read off at the different temperatures for various values of the volume thus yielding material for calculating the constant volume, pressures, and temperatures. These values were laid off on coordinate paper and the best straight line passed through the data. The equations of the several straight lines was corrected subsequently by calculating the deviations and constructing an additional graph by means of which a deviation equation could be evaluated and also any trend in the deviations detected.

The equations obtained were all of the form, $p = [\Psi T - \Phi]$, and by comparison with equation (1) $\Psi = R/(v-\delta)$ and $\Phi = a/(v-l)^2$ whence $\delta = v - R/\Psi$. The δ values may be treated best by using log δ and $1/v$ as the coordinates of the graph since it is propsed to test the functional form $\delta = \beta e^{-\alpha/v}$. The Φ values were tested by writing $v = \sqrt{a}/\sqrt{\Phi} + l$. It is seen that v should be plotted with $1/\sqrt{\Phi}$ as abscissae. The graph for nitrogen is given in figure one and for argon in figure two. It should be stated that the data given by Amagat above 1000 atm. give values of log δ which join on to the values in figure two, but begin to differ slightly in trend as the pressure becomes greater. In fact a straight line may be passed through the 1-1000 atm. series and the 1000–3000 atm. series with the lines intersecting at about 1000 atm. The effect is due undoubtedly to the inaccuracy of the high pressure gauge.

The graphs show the functional form of δ as related to volume changes is very accurately satisfied, while argon shows no trend. In the graph

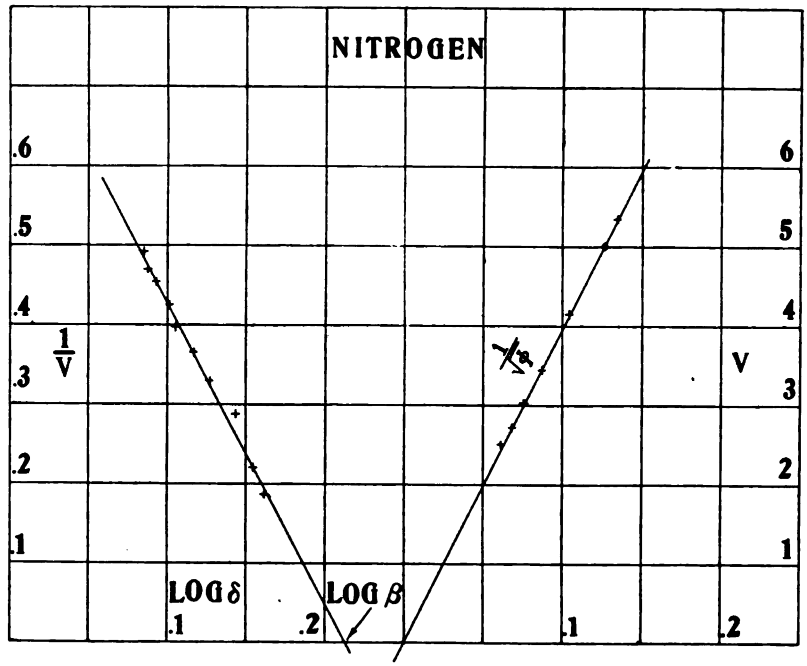
NITROGEN
.6
.5
.4
.3
.2
.1
6
5
4
3
2
1
1/V
V
LOG δ
LOG β
.1
.2
.1
.2

FIG. 1

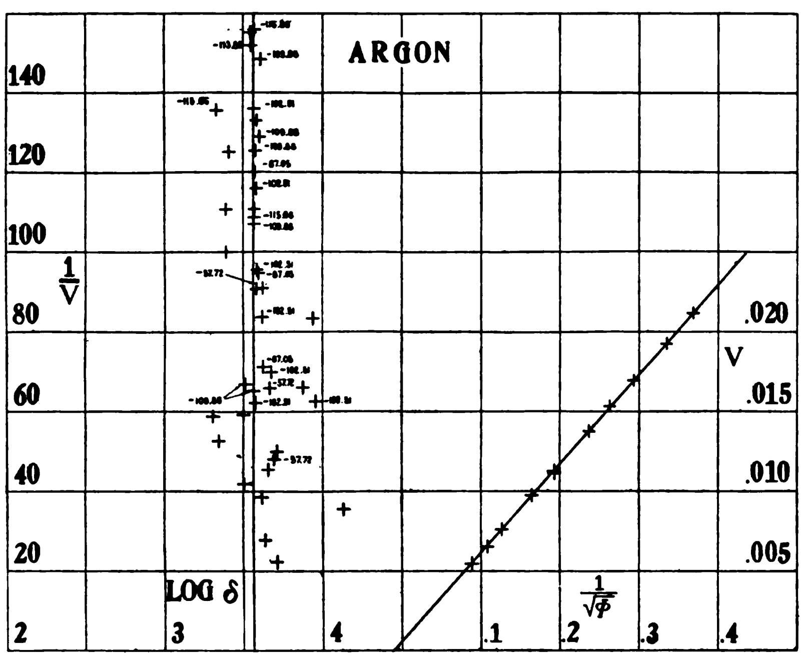
ARGON
140
120
100
80
60
40
20
1/V
V
.020
.015
.010
.005
LOG δ
2
3
4
.1
.2
.3
.4
1/√φ

FIG. 2

TABLE 1

NITROGEN EQUATION OF STATE

v	$p, T=\phi$	p, 16.03°	p, 99.45°	p, 199.5°
5.354	149.8	161.8	225.4	299.7K
	150.0	162.0	225.0	299 A
4.150	199.8	217.2	306.6	414.0
	200.0	217.0	307.0	414.0
3.460	250.4	273.1	390.3	531.3
	250.0	273.0	392.0	530.0
3.024	299.7	327.6	472.4	646.4
	300.0	328.0	474.0	644.0
2.729	347.7	381.1	552.9	759.5
	350.0	383.0	556.0	758.0
2.511	396.0	434.4	632.8	870.8
	400.0	439.0	637.0	869.0

The volume of 1 gram as determined by experiment is 799.1. This volume substituted in the equation of state gives 1.0004 instead of 1.0000.

$$p = \frac{2.9139}{v - \delta} T - \frac{1587.2}{(v - 0.007)^2}$$

$$\log_{10} \delta = 0.22001 - \frac{0.2839}{v}$$

TABLE 2

COMPARISON OF CALCULATED PRESSURES WITH PRESSURES GIVEN BY CROMMELIN FOR ARGON

TEMPERATURE	p_{cal}	p_{obs}	v	TEMPERATURE	p_{cal}	p_{obs}	v	TEMPERATURE	p_{cal}	p_{obs}	v
+20.39	61.932	61.741	0.016876					−119.2	37.753	37.641	0.010327
0°	31.478	31.572	0.030958	−113.8	42.692	42.682	0.009374		38.219	37.923	0.0101150
0°	36.671	36.61	0.026467		47.616	47.655	0.0077421		43.304	43.006	0.008002
−57.72	34.994	35.127	0.020782		52.074	51.752	0.006548		46.189	46.082	0.0069582
	45.876	46.209	0.015397		52.566	52.188	0.006435		47.95	47.272	0.0063954
	61.987	62.079	0.011026								
−87.05	33.231	33.296	0.017912								
	40.948	41.094	0.014003	−115.86	41.932	41.908	0.009257				
	51.431	51.533	0.010568		50.710	50.324	0.006446				
	61.822	61.830	0.008345		55.13	53.204	0.0055576				
−102.51	28.150	28.840	0.019037		55.83	57.493	0.0054536	−120.24	37.801	37.836	0.010083
	32.375	32.394	0.016070						41.566	41.668	0.0084389
	35.552	35.784	0.014294						44.346	44.510	0.0073363
	40.808	40.976	0.011916	−116.62	37.171	37.250	0.011042				
	45.000	45.088	0.010438		41.811	41.943	0.009075				
	51.323	51.398	0.0086296		46.164	46.496	0.0074801				
	56.996	56.882	0.007372		50.45	50.259	0.006261	−121.21	37.588	37.465	0.0099676
	63.262	62.239	0.0063285						41.848	41.932	0.0080740
−109.88	31.532	31.515	0.015351						45.266	45.282	0.0067138
	32.051	31.929	0.015031								
	49.402	49.515	0.007964								
	54.475	54.250	0.006742								

of the latter substance the logarithm of δ was plotted with the density in order to bring out more clearly the relation of the various δ values to the density although an inspection of the tabulated values show that δ is constant. It might be added that helium, while the data are less extensive, gives identically the same result as does argon.

In tables 1 and 2 will be found the comparisons of the pressures calculated for the volumes and temperatures as given by Amagat for nitrogen and Crommelin for argon. The nitrogen pressures calculated show about as good an agreement throughout with the observed pressures as could be expected.

THE CLASSIFICATION OF VASCULAR PLANTS

By Edward W. Berry

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Communicated by W. Trelease, February 14, 1917

A scientific knowledge of plants or other natural objects consists to a very considerable extent in a knowledge of their mutual (phylogenetic) relations, hence the necessity of a Taxonomy that will consistently express filiation, which is only another way of stating that it should be phylogenetic. Science does not consist merely of names, but it cannot very well progress without a terminology, and not until this terminology becomes an expression of evolution can it become consistent and itself scientific.

Botanists have rather effectually grappled with this problem in the case of the lower plants, but the classification of the so-called vascular plants remains largely as an inheritance from the study of the end products of their evolution, namely a study of the existing vascular plants, with but slight consideration of the recent progress of paleobotanical investigation.

The not so very long obsolete practice of considering the Angiosperms and the Gymnosperms as subclasses of the class Exogens was a no more pernicious mask of their true relations than the extant usage which considers vascular plants as separable into two great phyla—the Pteridophyta or Sporophyta and the Spermophyta. With the subdivisions of these two groups the present situation is equally inexpressive of our present state of knowledge. To the paleobotanist the Angiosperms and the Gymnosperms are obviously not groups of the same order, the latter including several groups of comparable rank with the Angiosperms as a whole, and extending over a period of time expressed by the ratio of 21 for gymnosperms to 6 for angiosperms.

Furthermore can any characters be mentioned which in the light of paleobotanical knowledge and evolutionary theory are more illogical as the basis for the characterization of great groups than siphonogamy, or the number of cotyledons, or the great stress that is laid upon the morphology of flower parts in the classification of the socalled flowering plants.

The attempt to force the vegetation that clothed the earth five or ten, or fifteen millions of years ago into the taxonomic bounds formulated for the flora of a single geological period, namely—the Present, suggests the petrified asters (Sphenophyllum) which Lehmann described from the Carboniferous, or the cacti, galiums and euphorbias which Lindley once described from the English Coal Measures.

During the long ages of the Paleozoic there were at least four dominant major groups of land plants and a fifth should probably be added, since while the true ferns were not as numerous as was once supposed, the other groups show more or less evidence of having had ferns for ancestors. These other groups that were dominant in the Paleozoic are those of the seed ferns, the Lepidodendrons (and their allies), the Calamites (and their allies) and the Cordaites (and their allies). What can be said of a practice which unites in a single order such complex arborescent quasi seed plants as some of the lepidodendrons and existing club mosses separated by a time interval of many millions of years and by an almost equally great structural gap?

During the Mesozoic the dominant plants were the ferns, cycad-like plants, conifers and ginkgoes, all of which underwent adaptive radiations on all of the continents and which should form the basis for a dozen natural orders instead of but four, in fact Nathorst has already proposed that the cycad-like plants shall constitute a separate phylum—the Cycadophyta.

The proposals that follow were formulated in 1910 and have been tested in university and research work during the interval that has elapsed. They were used in an article on Paleobotany for the new International Encyclopedia (1915) and are put forward in a somewhat categorical manner at the present time as a summary of the present status of paleobotanical knowledge and as an invitation for comments from competent critics.

Phylum **Angiospermophyta** (ἀγγεῖον, a receptacle). Berry, 1915 (Anthophyta).

Phylum **Coniferophyta.** Coulter, 1912, Including the Gnetales.
Coniferales.
Araucariales.
Taxales*
Ginkgoales*
Cordaitales.

Phylum **Cycadophyta.** Nathorst, 1902.

Megaphyllous; leaves compound; stems phyllosiphonic; with primary roots; ciliated sperms. Gymnospermic and strobiloid. Including Cycadales, Williamson iales and Cycadeoidales (Benettitales).

Phylum **Pteridospermophyta.** Ward, 1904. (Cycadofilices of Potonié, 1902).

Megaphyllous; phyllosiphonic; spermophytic, ♂ and ♀ sporophylls little differentiated from the vegetative foliage; gymnospermic but never strobiloid; filicinean in form and habit and much of their vascular anatomy. Including Cladoxylaceae, Lyginodendraceae, Medullosaceae, Cycadoxylaceae, Protopityaceae and incertae sedis including Aneimites, Gigantopteris, Glossopteris, etc.

Phylum **Lepidophyta** (λεπίς, ἰδος, a scale). Berry, 1915.

Microphyllous; cladosiphonic, with exarch protostele; homosporous, heterosporous and quasi spermophytic. Prevailingly strobiloid.

Lycopodiales { Lycopodiaceae, Selaginellaceae }
Isoetales
Psilotales†
Lepidodendrales { Bothrodendraceae, Lepidodendraceae, Sigillariaceae }

Phylum **Arthrophyta** (ἄρθρον, a joint). Berry, 1915‡

Stems articulated at the nodes, ribbed. Leaves verticillate, dichotomously compound in the Pseudoborniales and the Protocalamariaceae, palmately laciniate in some Sphenophyllales; pro-

* There is a question whether these two groups with possibly the Cordaitales should not be united as an independent phylum intermediate between the Coniferophyta and the Cycadophyta.

† The Lepidophyta correspond to the Lycopsida of Scott except that he refers the Psilotales to his Sphenopsida (Scott, 1909).

‡ Nearly equivalent to the Articulatae of Lignier and the Sphenopsida of Scott.

gressively reduced during the history of the phylum. Sporangiophoric and strobiloid. Homosporous and heterosporous.

Class Sphenophyllae-Sphenophyllales

Class Calamariae
- Pseudoborniales (Nathorst, 1902)
- Calamariales
  - Calamariaceae
  - Protocalamariaceae (Potonie, 1899)
- Equisetales

Phylum **Pteridophyta** (emended to correspond to the Filicales). Berry, 1915.

Megaphyllous; phyllosiphonic; fructifications on but little modified foliage leaves, never strobiloid; prevailingly homosporous. Heterosporic and quasi spermophytic in certain highly specialized Paleozoic lines, and in existing Hydropterales.

Class 1. Coenopteridae (κοινός, common or general, in allusion to their generalized characters). Seward, 1910*

Class 2. (?) Eusporangiatae
- Ophioglossales
- Marattiales
- Psaronius (Pecopteris), etc.

Class 3. (?) Leptosporangiatae, or Eufilices†
- Osmundales
- Gleicheniales (?)
- Matoniales (?)
- Polypodiales, including Hymenophllaceae, Schizaeaceae, Cyatheaceae, Parkeriaceae, Polypodiaceae.

Class 4. Hydropteridae
- Hydropterales
- Sagenopterales‡

* The Inversicatenales of Bertrand (1909) and the Primofilices of Arber (1906).

† Probably has additional fossil representatives.

‡ The subdivisions of this class are tentative.

DISPLACEMENT INTERFEROMETRY IN CONNECTION WITH U-TUBES

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Communicated, March 7, 1917

1. *Introduction.*—A variety of constants in physics may be found from the relative heights of two communicating columns of liquid. This is for instance the case in the classical experiment of Dulong and Petit on the thermal expansion of liquids. Again if one of the tubes is subject to a special force acting in the direction of its axis, this force in its bearing on the liquid may be evaluated from the resulting difference of heads of the columns. Thus one tube may be surrounded by a magnetizing helix and the effect of the axial magnetic field on the liquid in question (i.e., the susceptibility) found from the displacement of its surface by the presence and absence of the field; etc. It seemed to me worth while therefore to test whether it would be possible to measure small displacements of this kind by passing the two component beams of a displacement interferometer axially *through* the two columns respectively, and to measure the differential effects in question in terms of the resulting displacements of fringes.

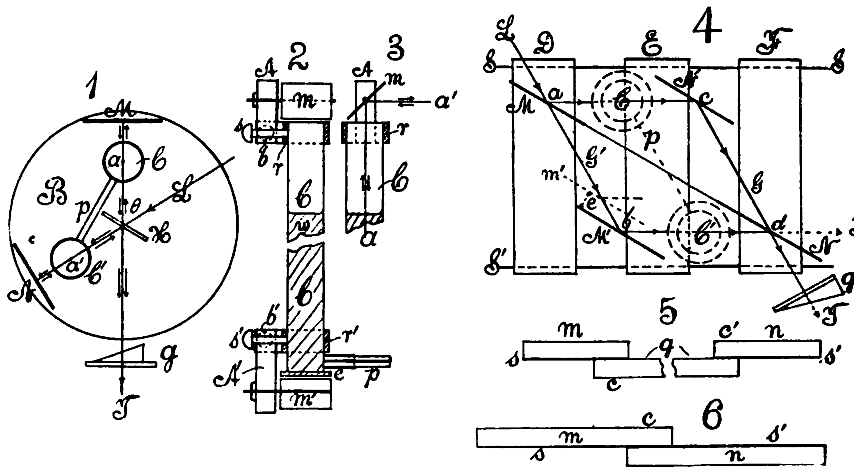
2. *Apparatus.*—The interferometer used was of the same form as that before described (these PROCEEDINGS, 3, 1917, 117), B in figure 1 being a heavy iron block, one foot in diameter and 1.5 inches thick, on which the mirrors M, N (the latter and preferably both on micrometers) are securely mounted with the usual direct rough and elastic fine adjustment for horizontal and vertical axes. A beam of parallel white rays L arrives from a collimator (not shown) and impinges on the half silver plate H, to be reflected and transmitted at a convenient angle θ (about 60°), thus furnishing the two component beams which are to traverse the limbs of the U-tube.

The vertical columns of this tube are shown at C and C' (with accessory mirrors removed) and they are joined to the capillary tube p near the bottom of C and C'. Details will be given in connection with figures 2 and 3.

The ray HM strikes a mirror symmetrically at 45° to the vertical below C, is thence reflected upward along the axis a striking another mirror above also symmetrically at 45° and parallel to the former, whence it is reflected to the opaque mirror M. The latter reflects the ray normally back so that it retraces its path as far as H, by which plate it is now transmitted to be observed by the telescope at T. Sim-

ilarly the transmitted component ray HN is guided by suitable reflectors at 45°, so as to take the path $Ha'Na'HT$, thus passing axially (a') through the tube C'.

It is necessary that the U-tube CpC' be mounted independently of the block B on suitable bracket or arm attached to the pier. Otherwise any manipulation at N will disturb the surfaces of water in C and C'. Ordinary clamps admit of raising or lowering or rotating CC' satisfactorily, always providing that it shall not touch B. The telescope at T is also mounted apart from B on the table below. The direct vision prism grating g is placed immediately in front of the objective and swivelled (as described, loc. cit.) so that either the white slit images or their spectra may be seen in the field of view, according as g is rotated aside or is in place.



In figure 2 a front sectional elevation of one of the shanks of the U-tube is given with all appurtenances, and a similar sectional elevation at right angles to the former is added in figure 3 for the top of the tube. In figure 2 the mirrors m' and m are on horizontal axes and the component ray coming from behind the diagram strikes m' below, is reflected axially upward through CC, impinging on the mirror m (also on a horizontal axis) whence it is reflected horizontally toward the front of the diagram. The ray a and mirror m are given more clearly in figure 3. The lateral capillary tube appears at p and the tube C is closed below with a plate of glass e, cemented in place.

To mount the mirrors m, m', snugly fitting rings r and r' encircle the tube C near its top and bottom and can be fixed by the set screws s and s'. In virtue of these rings the mirrors m, m', may be rotated at pleasure around the vertical axis a of CC. The horizontal axis of the

mirrors m, m', rotates at pleasure in the vertical arms A, A' of square brass tube. A, A' in turn may be slightly swivelled about the horizontal axis b, b', in a rigid lateral projection of the rings r, r'. Thus m, m' are capable of rotation around 3 axes normal to each other, and adequately clamped in any position.

The component ray HN may be adjusted to the center of the lower mirror m' by placing the collimator L and then guided axially by $m'mN$ as described each being adjustable. The component ray HM may be similarly adjusted to the center of the lower mirror m' (at 45°) by (slightly) rotating the half silver plate, H (on horizontal and vertical axes) and then guided axially by $m'mM$. As a whole the adjustment is difficult, though it need not be much refined. Clear white slit images in the telescope T are an adequate criterion.

In the absence of a liquid in CC figure 2, the fringes are easily found after careful preliminary measurement and they are strong and satisfactory. When this adjustment is given the presence of liquid in CC, if the two columns are of nearly equal length, does not much modify the adjustment. In fact the fringes were found much more easily than I anticipated and in quiet surroundings they are strong and fine. It is necessary however that the tube CC should be of sufficient width to avoid all curvature due to capillarity, at least in the axis. Tubes 2 cm. in diameter and 10 cm. long of thin brass were first tried, but proved to be too narrow. No sharp slit images could be obtained with reasonable care as to setting the mirrors. Thereafter tubes 4 cm. in diameter were used and these proved to be satisfactory. Slit images were sharp and parallel and could be easily brought to coincide.

3. *Equations.*—Some estimate of the increments to be anticipated may be given here, and expressed in terms of the Dulong-Petit experiment. If α is the mean coefficient of expansion of water at the temperature in question and ΔH the increment of the head H corresponding to the temperature difference Δt between the columns

$$\Delta H = \alpha H \Delta t \tag{1}$$

Again if ΔN corresponding to ΔH is the displacement of center of ellipses at the wave length λ and μ the index of refraction of water, so that $\mu = A + B/\lambda^2$, nearly,

$$\Delta H = \frac{\Delta n}{\mu - 1 + 2B/\lambda^2} \tag{2}$$

Hence Δt may be computed as

$$\Delta t = \frac{\Delta n}{(\mu - 1 + 2B/\lambda^2)\,\alpha H} \tag{3}$$

Since the value of ΔN is within 10^{-4} cm. and $H = 10$ cm. in the above apparatus, we may further write at mean temperatures (25°) $\alpha = 2.5 \times 10^{-4}$; $\mu = 1.333$; $B = 10^{-11} \times 3.1$, $2B/\lambda^2 = 0.018$ at the D line. Thus $\mu - 1 + 2B/\lambda^2 = .351$ and $\Delta t = 10^{-4}/3.51 \times 2.5 \times 10^{-4} \times 10 = 0.114°$. In other words in case of tubes 10 cm. long, the effect of a difference of temperature of about 0.1° between the tubes should be easily observable by mere displacement, whereas a difference of less than 0.03° would be equivalent to the passage of one interference ring.

Again from equation (2), if $\Delta N = 10^{-4}$ cm. then $\Delta H = 10^{-4}/.351 = 10^{-4} \times 2.8$ cm. or about 9×10^{-5} cm. per vanishing interference ring are the displacements to be anticipated. These are equivalent to pressures of about 0.3 and 0.1 dynes per cm².

A number of experiments were made with this apparatus but for these there is no space here.

5. *Jamin's Interferometer.*—The ease with which the Michelson interferometer may be adjusted and its remarkable adaptability have led to its general preference over the older form of Jamin. Nevertheless the latter furnishes two parallel rays which for such purposes as the present are desirable. Hence if the four faces of the interferometer be separated in the manner suggested by Mach, a very available form of interferometer is obtained. But the trouble with the arrangement is the difficulty of adjusting the *four* surfaces. Not only are the centers of ellipses liable to be remote from the center of the field, but it is often hard, without special equipment, to even find the fringes.

If however the device which I suggested elsewhere is adopted, i.e., if figure 4, the half silvered plates, M, N, are at the ends of a simple strip of plate glass, so that rays terminating in $M\ M'\ N\ N'$ after adjustment necessarily make a rhombuslike figure symmetrical to MN the fringes are found at once: for they appear when the white slit images in T coincide horizontally and vertically and the rays bd and cd intersect in the common point d. Hence the mirrors, M', N', should be on carriages D, F, adapted to move on the parallel slides S, S'. M, N may also be put on a carriage E though this is not necessary. S, S' need not be parallel to ac or bd. If the mirror M' and N' are wide, considerable latitude of adjustment is thus obtained.

If $M\ N$ is half silvered on the same side (i.e., toward N') a compensator is needed in ac or cd, if path difference is to be annulled (symmetry). If however M is half silvered on the N' side and N on the M' side, no compensator is required. In the latter case however, if ordinary plate glass is taken, M and N are not quite parallel and the ellipses will be

eccentric. This however is not necessarily a disadvantage, unless the strip MN is excessively wedgeshaped.

The ellipses obtained are usually long vertically, so that the fringes soon become straight and the rotation is extremely rapid whenever the center of ellipses is out of the field. It is therefore possible to adjust relative to horizontal fringes (parallel to shadow of wire across slit), as these incline very obviously for a displacement of less than 10^{-4} cm. and rapidly become vertical. For this reason it makes little difference whether the half silvers are on the same or on opposite sides, or whether observation be made at T (*cd* prolonged) or at T' (*bd* prolonged). Moreover the plate MN may be conveniently constructed as in figure 5, of two mirrors m, n, attached to the clear strip of plate glass, g, by aid of strong steel clips at c, c'. With the half silvers s, s' on the same side, the wedge angle of the glass is excluded. For shorter diagonals, the plan of figure 6 with the silver surfaces s, s' held together by clips at c is preferable.

If the mirror M', figure 4, is displaced a distance e where a glass plate compensator of thickness E, and refraction constants μ and B is introduced normally either into *ab* or *bd*, the equation is easily seen to be, at wave length λ

$$E\ (\mu - 1) + 2\ B/\lambda^2 = 2e \cos \theta$$

where θ is the angle of reflection at M. Using the plate $E = 0.434$ cm. treated above, the first member is 0.2428 cm. Values of e of 0.2420, 0.2409, 0.2427 were roughly obtained. Hence the mean value of θ should be about 60°, as it actually was. Certain outstanding difficulties may be met by making MN, figure 4, the *short* diagonal of the rhombus and using the strip figure 6. In such a case θ at M' is small and in view of the nearly normal reflection at M and N relatively little reflection comes from naked glass, sliding is largely avoided and no compensator is necessary. In this case the fringes for no path difference are actually black horizontal lines on a colored ground and far enough apart that 1/10 fringe could easily be estimated. A test experiment with the above plate showed $e = 0.1244$ cm. corresponding to the small angle θ a little over 12°.

When the U-tube CC', figures 2 and 4, is introduced, the strip $M\,N$ will have to be at a considerable angle (about 45°) to the horizontal, so as to raise the N end about 15 cm. above the M end, corresponding to the height of m above m' in figure 2. The new condition however in no way changes the general procedure. In case of figure 5 the mirror N'

must be high and M' low. If C' is at G' the whole of each component beam may be caught and passed through the respective shanks of the U-tube. The fringes are strong, easily found and large, so that the center of ellipses is not far outside of the field of the telescope.

Finally if the connecting tube p is nearly horizontal when in place, the fringes are usually found at about the same position of the micrometer (at M') after the liquid is introduced into the U-tube.

Experiments were also made with this apparatus. Displacement interferometers in which the rays do not retrace their respective paths have an important special property which I wish to accentuate in conclusion. If either opaque mirror is displaced on its micrometer normal to itself or if a plate compensator rotates in one beam on a vertical axis, the center of ellipses moves parallel to the length of the spectrum. If however the plate compensator rotates on a *horizontal* axis, the center of ellipses moves nearly *transversely* to the length of the spectrum. The phenomenon is quite sensitive. To this result I shall return in a succeeding note, in connection with the development of the Jamin design for displacement interferometry.

The present note will be presented in more extended form in a report to the Carnegie Institution of Washington.

ATTEMPT TO SEPARATE THE ISOTOPIC FORMS OF LEAD BY FRACTIONAL CRYSTALLIZATION

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Communicated, March 10, 1917

Although the complete inseparability of isotopes by chemical means has been frequently asserted, the evidence on which this assertion is based has always seemed insufficient. The methods used have been fractional crystallization and precipitation, but these processes have seldom been carried out more than ten times in a particular case, and frequently six or seven crystallizations have been thought a sufficiently thorough test of inseparability. A search of the literature revealed only one investigation, that of Radiothorium and Thorium by McCoy and Ross,[1] where as many as one hundred repetitions of a given process had been made.

It seemed worth while, therefore, to apply to the important generalization of Fajans, Russell, Fleck and Soddy a more searching test carrying the fractionation further, and using as a criterion of success not only the measurement of radioactivity but also the determination of atomic weight.

The lead from Australian carnotite, with which the kindness of Mr. S. Radcliff and Mr. E. R. Bubb had supplied us and which had been used in previous atomic weight and density determinations in this laboratory,[2] seemed well suited to our purpose.

About 1 kilogram of this lead was converted into nitrate and used for the fractional crystallization. Lead nitrate crystallizes from aqueous solution in compact, large octahedra which can be very completely freed from mother liquor merely by drainage, and come rapidly to equilibrium with the mother liquor.

The whole of the nitrate was dissolved in distilled water and evaporated until a crust began to form. This was called fraction 1. After cooling to room temperature the mother liquor was poured off and formed fraction 2. To the crystals from fraction 1 was added sufficient water to dissolve them, and the solution was called fraction 3.

Fractions 2 and 3 were evaporated to saturation and cooled as fraction 1 had been, the liquor from 2 forming fraction 4. Fraction 5 was made by adding the liquor from 3 to the crystals from 2, and water was added to the crystals from 2 to form fraction 6. This process was continued until, as occurred first with fraction 16, one of the end fractions became too small to work with conveniently and was consequently added entire to the mother liquor from fraction 17 to form fraction 22. This expedient was resorted to wherever the end fractions became inconveniently small. In all, seventy-five courses or tiers of fractions were obtained, most of the last of which contained eighteen fractions to a tier. The number of effective crystallizations was 904.

The crystallization was carried out entirely in porcelain, at first in large evaporating dishes, then, as the fractions became smaller, in casseroles covered with watch glasses. The hot casseroles were usually set in evaporating dishes full of cold water, which was frequently changed to hasten cooling.

Centrifugal drainage was not used because, as determined by calculation, the gain in effectiveness of separation would have been less than the disadvantage caused by the increased time necessary to treat the precipitates centrifugally.

Purification.—Two samples, one from the more soluble and one from the less soluble end of the final row of crystallizations were treated in an identical manner. Chiefly to remove the large amount of silica which had dissolved from the casseroles during the fractionation, the crystals were first electrolyzed in warm concentrated solution between platinum wires. The well formed flat leaflets of lead were removed from time to time, with glass rod, rinsed with a stream from a wash bottle, and pre-

served in quartz dishes until all the metal had been deposited from the solution. The crystal were further washed from time to time, and all washings returned to the electrolysis bath, which was gradually reduced in volume as the concentration of lead became smaller. The lead crystals were then dissolved in pure nitric acid, and the excess of acid was evaporated. The crystal of lead nitrate were dissolved in the purest water, filtered through a platinum Gooch crucible, and precipitated by the addition of a large excess of redistilled nitric acid and cooling with ice. The crystal meal thus obtained was freed from adhering acid in the platinum centrifuge, washed centrifugally with purest concentrated acid and redissolved in the least possible amount of pure boiling water.

Five precipitations with centrifugal drying were made in this way. The crystals from the fifth crystallization were placed in a quartz dish and dissolved. From this solution lead chloride was precipitated by passing in pure hydrochloric acid gas made by warming the purest acid of commerce. The chloride thus obtained was dried in the centrifuge, washed with cold water, and redissolved in boiling water. Four crystallizations as chloride followed. A little hydrochloric acid was added to the solution each time to prevent the formation of basic salt and to render the chloride less soluble. The last crystals, after centrifugal washing and drying were preserved in an exhausted desiccator over fused potash. Nothing but platinum and quartz vessels were used.

The Determination of Atomic Weight.—For these determinations the usual procedure was employed. About five grams of the dried crystals were placed in a platinum boat, which with its container had been carefully weighed. The boat was gradually heated in a quartz tube attached to the usual 'bottling apparatus,' in a current of pure dry hydrochloric acid gas. After nearly all the residual water had been driven from the crystals, the temperature was raised so as to fuse the salt, which was then cooled as quickly as possible in a current of pure dry nitrogen prepared by the Wanklyn process.

After the salt had cooled, and the nitrogen had been replaced by dry air, the boat was pushed into the weighing tube, bottled, and reweighed.

The boat and salt were next heated at 80°–90° on an electric stove in a glass flask containing about a liter of water to which enough pure nitric acid had been added to prevent the formation of basic salt, until the lead chloride had all dissolved. As usual[3] a small amount of siliceous and carbonaceous residue had to be removed by scrupulously quantitative filtration through a tared Gooch-Monroe crucible. On the average, about twice as much was found in the more soluble fraction as in the

other. The weight thus removed was subtracted from the weight of the fused salt. Before filtration the boat had been carefully lifted out and thoroughly washed; and the filtration was collected in the precipitating flask.

A supposedly equivalent weight of silver was next dissolved in pure nitric acid mixed with an equal bulk of water in a flask with a chain of bulbs ground in to the neck, a steam bath being used to heat the solution. More water was then added and the temperature raised until a few seconds' gentle boiling had removed nitrous fumes. The solution having been made up to a liter, it was added very slowly with constant agitation to the lead chloride solution. Actinic light was henceforth rigorously excluded from the precipitated silver chloride. The precipitate was coagulated by violent shaking, usually for fifteen minutes, and allowed to stand at least twenty-four hours, with further occasional shakings, before samples were removed for tests. The end point was found by the customary method by means of the nephelometer, and tested for several days to be certain that it did not change.

Vacuum corrections were applied as usual, and all customary precautions were taken. The results follow.

TABLE 1

ATOMIC WEIGHTS

	ORDINARY LEAD	CARNOTITE A	LEAD B
	207.187	206.426	206.406
	207.187	206.409	206.422
		206.431	206.399
Average	207.187	206.422	206.409

This table includes all the analyses of these materials which were completed; any which had become the object of suspicion were rejected at an early stage.

Obviously, if the fractional crystallizations produced any change in the relative concentrations of radium G and lead, this change is very small. The means of the two series of determinations of isotopic lead differ only by about 6 parts in 100,000. This is certainly within the bounds of possible experimental error, though the low 'average errors' of the two sets (0.008 unit in each case) tempt one to assign a meaning to the observed difference. At any rate, we are safe in concluding that assuming the atomic weights of 'radium G' and lead to be 206.0 and 207.2, respectively, no change in concentration greater than 13/1200 or 1.1% has been obtained by nine hundred crystallizations. Even supposing that a real difference in solubility exists, it is evident that

complete separation could not be attained (if at all) in less than eighty thousand fractionations.

Estimation of Radioactivity.—The β-ray activity of the lead used indicates that it contains in about 2 parts so-called 'radium G' and 1 part lead, an amount of 'radium D' of the order of 10^{-7} parts. This is betrayed by the steady growth of a penetrating β-ray product (RaE) which comes to practical equilibrium with its parent in about a month after they are separated. Evidently the ionization—if the α-rays from the polonium present are cut off—caused by a weighed amount of the material under constant conditions, is a measure of the concentration of radium D relative to its isotopes 'radium G' and lead. Thus the determination of the activity of the end fractions gives information as to the relation between the solubility of the nitrate of 'radium D' and the mean solubility of the nitrates of 'radium G' and ordinary lead, a relation of great interest because it cannot be tested by atomic weight determinations. This case should afford an especially favorable test of the theory of complete identity.

The method was as follows: To eliminate differences in moisture content, and consequently in absorbtive power, the two samples were kept in the same exhausted desiccator after purification. When sixty-six days had elapsed 10.25 $\pm$ 0.03 gram of each sample was weighed into a marked metal dish and tamped with a platinum spatula. The material formed a layer of thickness defined by the expression 0.88 g/cm.^{2} This is far too thin to give a maximum ionization due to the complete absorption of the β-rays from the lowest layers, but is thick enough so that no great accidental changes in thickness needed to be feared, and the effect of small changes was annulled as described below. The dishes were covered in turn with the same thin sheet of paper and introduced into a grounded aluminum-leaf electroscope which was very kindly loaned by Prof. William Duane. The rate of fall of the leaf was then observed with a stop-watch in the usual manner. All errors arising from differences of potential, differences of stiffness of the leaf in different positions, and inhomogeneity of the scale were eliminated in the usual way. The two dishes were introduced alternately into the electroscope to eliminate the effect of changes in the natural leak, and were thoroughly stirred and repacked between every two measurements to allow for small accidental differences in thickness, and other precautions to cut out extraneous effects and secure exact comparability were employed.

It is believed that the table below, which includes all the observations affected by no known source of error, represents about as good reproducibility as this electroscope can be made to give.

TABLE 2

β-RAY ACTIVITIES

SAMPLE A

11.30; 11.43; 11.69; 11.56; 11.61; 11.47; 11.49; 11.50; 11.41; 11.58 **Mean 11.504.**

SAMPLE B

11.32; 11.62; 12.06; 11.43; 11.70; 11.34; 11.32; 11.43; 11.47; 11.36; **Mean 11.505.**

The ionization is expressed in arbitrary units (scale divisions per minute.) The natural leak (which must have been the same for both as they were taken alternately) was found to be 0.50 ± 0.01. The agreement of the averages to one part in ten thousand must be regarded as fortuitous, since their mean probable error is about 0.11 unit. This error, divided by the mean value from which has been subtracted the natural leak may be taken, roughly, as a measure of the accuracy of the determination. Its value is $\frac{11}{1150-50} = \frac{1}{100}$. This work definitely shows, then, that no change in the relative concentration of radium D and its isotopes as great as one per cent has occurred in nine hundred fractionations, and certainly gives no indication that *radium D* could be completely separated from its isotopes by this method even by crystallizing as many as 90,000 times.

If ordinary lead is a mixture of isotopes, this mixture must have been made very long ago while the earth was still in a highly mobile condition (since all the ordinary lead throughout the world seems to have the same atomic weight[4]). Could the composite nature of ordinary lead be proved, the identity of the several samples through geological aeons would form another argument in favor of the inseparability of the constituents.

To the Carnegie Institution of Washington we are greatly indebted for some of the apparatus and material used in this research, which is to be continued in the near future.

Summary.—Lead from Australian carnotite (believed to contain about one part of ordinary lead to two parts of radium G, with a mere trace of radium D) has been fractionally crystallized over nine hundred times as nitrate, and the end-fractions purified.

The atomic weights of the samples so obtained from the crystal and the mother liquor ends of the series respectively agreed within the experimental error of 6 parts in 100,000.

The β-ray activities agreed within the experimental error of 1%.

These observations indicate that the nitrates of Radium D and lead on the one hand and radium G and lead on the other hand could hardly be separated, if at all, by less than 80,000 or 100,000 crystallizations.

Hence one might infer that the molal solubilities of the nitrates are probably essentially identical. The outcome gives strong experimental support for the hypothesis that isotopes are really inseparable by any such process as crystallization.

[1] *J. Amer. Chem. Soc., Easton, Pa.*, **29**, 1907, (1709).
[2] Richards and Wadsworth, These PROCEEDINGS, **2**, 1916, (505, 694).
[3] Richards and Wadsworth, *J. Amer. Chem. Soc. Easton, Pa.*, **38**, 1916, (2616).
[4] Baxter and Grover, *Ibid.*, **37**, 1915, (1058).

HYBRIDS OF ZEA TUNICATA AND ZEA RAMOSA

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Communicated by E. F. Smith, March, 16, 1917

Zea tunicata and *Zea ramosa* are the two most striking variations or mutations from normal maize. Though usually referred to as agricultural species they seem to deserve a place with the so-called species of Oenothera which have originated by mutation.

The chief characteristic of *Zea tunicata* is that the glumes of the female inflorescence, or ear, are developed so that each seed is entirely enclosed. Associated with this character is a less conspicuous lengthening of the glumes of the staminate inflorescence that results in a thickening of the tassel.

The origin of *Zea tunicata* is not known, but its occurrence in widely separated and isolated regions would indicate that it has originated independently more than once, presumably as a mutation from ordinary maize.

In hybrids with non-tunicate varieties the tunicate character behaves as a dominant, but in our experiments it has never been possible to secure a homozygous tunicate strain. Progenies resulting from the selfing of tunicate plants have always shown segregation into approximately three tunicate plants to one normal.

The tunicate plants in self-pollinated progenies are separable into two classes, one producing typical tunicate ears and thickened tassels like the parent plant, and the other with greatly enlarged tassels containing both staminate and pistillate flowers, and with the ear either aborted or bearing greatly enlarged and usually sterile spikelets. This last class represents approximately one-third of the tunicate plants. Although these plants produce what appears to be normal pollen in the terminal inflorescence the long glumes never open and the pollen is not shed, and we have not been successful in securing selfed seed of this form.

The ratios in which the different classes occur indicate that the class with the bisexual terminal inflorescence is the homozygous form, and that the ordinary tunicate plants represent the heterozygous form, a cross between the form with the bisexual inflorescence and the normal non-tunicate maize.

From experiments involving forty-three progenies derived from three distinct sources, it would appear that the ordinary type of tunicate ear represents an example of imperfect dominance as unfixable as the Andalusian fowls, but since other workers report the existence of pure tunicate strains it may be that other stocks behave differently.

The class with bisexual terminal inflorescence, which is here considered homozygous, will be referred to as full-tunicate and the ordinary tunicate type, which is looked upon as heterozygous, will be termed half-tunicate. The term tunicate or podded will be used as a general term including both of the above classes.

Zea ramosa or branched maize, discovered by Dr. W. B. Gernert[1] at the Illinois Agricultural Experiment Station, differs from normal maize in having the pistillate inflorescence or ear which is normally simple replaced by a compound inflorescence. There is also a less striking but equally significant change in the branching of the terminal inflorescence or tassel. In normal maize the terminal inflorescence bears a number of branches at its base. Above the uppermost branch the axis is continued into what is termed the central spike where the pairs of spikelets are borne directly on the axis of the inflorescence. There is thus in passing from the base to the tip of the tassel an abrupt transition from the uppermost branch to simple pairs of spikelets. In the *Zea ramosa* tassel the branches are much more numerous and gradually decrease in size from the base upward, the transition from branches to pairs of spikelets being imperceptibly gradual.

Unlike *Zea tunicata*, *Zea ramosa* is a recessive variation. The dominance of normal maize over this variation seems complete. It has not been possible in any way to distinguish between plants heterozygous for the ramosa character and normal maize. So far as observed, the character behaves as a simple Mendelian unit.

Both *Zea ramosa* and *Zea tunicata* are variations from normal maize toward the general type of grasses, and as such may be looked upon as reversions, since both cases involve a loss of a specialization that distinguishes maize from practically all other grasses.

Description of the Hybrid.—The cross between *Zea tunicata* and *Zea ramosa* was made in 1914. Nine first-generation plants of this cross were grown in 1915. Of these four were tunicate, and five normal,

indicating the heterozygous nature of the half-tunicate parent plant. The tunicate plants were all half-tunicate, and no trace of the ramosa characters could be seen.

Five self-pollinated first-generation ears were selected for planting in 1916. Three of these ears were tunicate and two normal. Four hundred and eight second-generation plants were matured in 1916—326 from the three tunicate ears, and 82 from the two non-tunicate ears of the first generation.

The progeny of the non-tunicate or normal F_1 plants showed segregation into normal and ramosa in the ratio of 3 to 1, the numbers being 65 normal and 17 ramosa. In none of these plants was there evidence of tunicate characters.

It was possible to classify the F_2 plants descended from the tunicate F_1 ears into (1) Normal, (2) Half-tunicate, (3) Full-tunicate, (4) Ramosa, and (5) Tunicate-ramosa. The last class comprised those plants in which both tunicate and ramosa characters could be recognized. When both the staminate and pistillate inflorescences were considered the classes were well marked.

In the tunicate-ramosa group there were many plants with an entirely new type of inflorescence. In these the branching habit, which suggests that of the ramosa parent, was developed to a grotesque extreme. As soon as branches formed these again branched. This division continued until the end of the growing season when the tissue was still in an embryonic condition and nothing resembling floral or foliar organs was formed. The result was a white succulent mass. This peculiar formation occurred in both lateral and terminal inflorescences, though it was much more common in the former, and in terminal inflorescences it was usually confined to the basal branches.

This type of inflorescence is similar, if not identical, with an abnormality discovered by Blaringhem in a strain of *Zea tunicata*[2] and termed by him 'cauliflower.'

It was found possible to account for the ratios by the assumption of relatively simple gametic composition. The tunicate character is represented by T and its recessive allelomorph, the normal condition, by T′. The recessive ramosa character is represented by R′ and its dominant allelomorph, the normal condition by R.

The observed numbers compared with the numbers expected in accordance with this gametic composition are given in the following table.

NUMBER EXPECTED OUT OF EACH 16	GAMETIC COMPOSITION	CHARACTERS OF PLANT	EXPECTED NUMBERS	OBSERVED NUMBER
1	T′ T′ R R	Normal	61.2	64
2	T′ T′ R R′	Normal		
2	T T′ R R	Half-tunicate	122.0	121
4	T T′ R R′	Half-tunicate		
1	T T R R	Full-tunicate	61.2	61
2	T T′ R R′	Full-tunicate		
1	T T R′ R′	Tunicate ramosa	61.2	64
2	T T′ R′ R′	Tunicate ramosa		
1	T′ T′ R′ R′	Ramosa	20.4	16
Total........			326.0	326.0

In each of the parent varieties there is a deviation from normal maize in both the staminate and pistillate inflorescences. It is of interest, therefore, to determine so far as possible whether the changes in the two parts of the plant are due to a single gametic change, or whether they may be inherited separately.

With this point in mind the plants were classified with respect to their tassels, and ears, independently, with the following results:

The normal plants were all normal in both tassel and ear.

The distinction between half- and full-tunicate is somewhat arbitrary, but with one exception all the 121 plants classified as half-tunicate by their tassels also had ears classed as half-tunicate. That the distinction is genetic and not merely physiological is indicated by the absence of any sensible correlation between the length of the staminate and pistillate glumes on the individuals inside the half-tunicate group.

Of the 53 plants classed as full-tunicate by their tassels, all but two had either full-tunicate ears, or none at all, and the two exceptions had pistillate glumes only slightly below 45 mm., the minimum length set for the glumes of full-tunicate ears. There is then almost a perfect correlation between the type of tassel and ear, but here again there is no correlation inside the group. That is, long glumes in the tassel were not correlated with long glumes on the ear. Even the extreme form of full-tunicate plants that produced no ears did not differ, with respect to the length of the staminate glumes, from the plants bearing ears.

When both tunicate and ramosa characters were present in the ears, the tassels were always ramosa but the tunicate characters were not always apparent in the tassel.

Conversely, the 53 plants with ramosa tassels all exhibited ramosa characters in the ears, either pure or combined with the tunicate character.

Perhaps the most interesting class of plants were those with cauli-

flower inflorescences. This peculiar type of pistillate inflorescence seems definitely confined to the class of plants combining the tunicate and ramosa characters.

The cauliflower character, which in itself would seem more nearly related to ramosa than to tunicate, is somewhat more definitely associated with the ramosa than with the tunicate character of the tassels.

There were 22 plants with pure cauliflower ears. Twenty of these had tassels in which both tunicate and ramosa characters were obvious. The other two were classed as having ramosa tassels. Of the 13 plants with partial cauliflower ears 11 had ramosa tassels in which no tunicate characters were observed, and two showed evidence of both tunicate and ramosa in their tassels.

If the second-generation plants are examined for each of the parental types separately, there is seen to have been a simple 1 to 3 segregation in both instances. One-fourth of the total number of plants are ramosa and three-fourths non-ramosa (observed 79 to 247, expected 81.5 to 244.5). One-fourth are non-tunicate and three-fourths tunicate (observed 80 to 246, expected 81.5 to 244.5). The distinction between half- and full-tunicate could not be made when these characters were combined with the ramosa character. The various combinations of parental characters, occurring as they do in the normal di-hybrid ratios, show that the tunicate and ramosa characters are not genetically correlated.

The extended publication will appear in the *Journal of Agricultural Research.*

[1] Gernert, W. B., A new subspecies of zea mays L., *Amer. Nat., Lancaster, Pa.*, 1912, 46 (616-622).

[2] Blaringhem, Louis, *Mutation et traumatismes*, Paris, 1907, (121-122).

DISTRIBUTION OF GALL MIDGES

By E. P. Felt

NEW YORK STATE MUSEUM, ALBANY, NEW YORK

Communicated by J. M. Clarke, March 12, 1917

The intimate relation existing between many species of these tiny, fragile flies and their food plants and the relatively limited migratory ability of either adults (owing to their weak powers of flight), or the larvae (due to their apodous or nearly apodous condition), led to a study of the distribution of these highly variable forms. The immensity of the complex may be appreciated by remembering that approximately three hundred genera and probably nearly three thousand species are known—the largest about 6 mm. in length and the smallest less than 0.5 mm. long. Some live in decaying or dead organic matter, others

prey upon mites and certain insects, while a very large majority produce characteristic galls upon the different parts of a great variety of plants. Not a few are inhabitants of blossom buds, with a very limited period for feeding and growth and an extended estivation or hibernation, while many others pursue these vital activities through most of the growing season. Summarizing, we may state that great structural variations and equally marked physiological and ecological adaptations are accompanied by a puny organization and a world-wide distribution. What are the most important factors which have brought this about?

It is impossible, owing to the limited knowledge of this group from important faunal regions, to present a complete story. Fortunately there are a number of genera with sufficiently well marked characters as to render their identification reasonably certain, and for a number of these there are records from such widely separated localities as to justify the characterization of their distribution as world-wide. The presumption is against their occurring in all faunal regions, though they probably exist in most places where climate and flora combine to make this possible. There are in addition to the above, a number of genera which have been recorded from such distant countries as to justify the suspicion that they likewise may have a world-wide or nearly world-wide distribution. A tabulation of these genera is given below.

Itonid Genera of Presumably World-Wide Distribution

Tribe and Genus	North America	South America	Europe	Asia	Africa	Australia	New Zealand
Lestremiinariae							
Catocha Hald.	+		+				
*Lestremia Macq.	+	+	+	+		+	+
Campylomyzariae							
*Campylomyza Meign.	+		+			+	+
*Joannisia Kieff.	+		+		+†		
Heteropezinae							
Oligarces Mein.	+		+				
*Miastor Mein.	+		+			+	+
Leptosyna Kieff.	+		+				
Brachyneura Rond.	+		+				
Spaniocera Winn.			+			+	
Porricondyliariae							
Didactylomyia Felt	+			+			
Holoneurus Kieff.	+		+				
*Colpodia Winn.	+		+	+		+	
Dirhiza H. Lw.	+		+				
Porricondyla Rond.	+		+		+	+	+
Camptomyia Kieff.	+		+				
Rubsaamenia Kieff.	+	+	+				
*Asynapta H. Lw.	+		+	+		+	
Winnertzia Rond.	+		+			+	

ITONID GENERA OF PRESUMABLY WORLD-WIDE DISTRIBUTION—*Continued*

TRIBE AND GENUS	NORTH AMERICA	SOUTH AMERICA	EUROPE	ASIA	AFRICA	AUSTRALIA	NEW ZEALAND
Dasyneuriariae							
Cystiphora Kieff.	+		+				
Rhabdophaga Westw.	+		+				
Dasyneura Rond.	+	+	+	+	+	+	
Diarthronomyia Felt	+		+				
Lasiopteryx Westw.	+		+				
Oligotrophiariae							
Rhopalomyia Rubs.	+	+	+	+	+		
Oligotrophus Latr.	+	+	+	+	+	+	
Janetiella Kieff.	+	+	+				
Phytophaga Rond.	+		+			+	
Asphondyliariae							
Schizomyia Kieff.	+		+	+	+		
§Cincticornia Felt	+		+			+	
Asphondylia H. Lw.	+	+	+	+	+	+	
Lasiopterariae							
Trotteria Kieff.	+		+				
*Lasioptera Meign.	+	+	+	+	+	+	
Aplonyx De Stef.	+		+				
Clinorhyncha H. Lw.	+		+				
Itonididinariae							
Contarinia Rond.	+		+			+	
Thecodiplosis Kieff.	+		+				
Dentifibula Felt	+			+			
Dicrodiplosis Kieff.	+		+				
Bremia Rond.	+		+				
Aphidoletes Kieff.	+		+			+	
Karschomyia Felt	+	+					
Mycodiplosis Rubs.	+	+‡	+	+			
Clinodiplosis Kieff.	+	+	+	+			
Diadiplosis Felt		+		+			
Arthrocnodax Rubs.	+	+	+	+			
Hormomyia H. Lw.	+		+				
Pseudhormomyia Kieff.	+		+	+			
Monarthropalpus Lab.	+		+				
Hyperdiplosis Felt	+	+‡					
Lestodiplosis Kieff.	+	+	+	+	+	+	
Itonida Meign.	+	+	+	+		+	+
Parallelodiplosis Rubs.	+		+				
Cystodiplosis Kieff and Jorg.	+	+					

* Recorded from the Baltic amber of the upper Eocene.

† Seychelles.

‡ St. Vincent.

§ Polystepha Kieff. and Eocincticornia Felt are included because of obvious affinities.

An examination of the above tabulation shows that representatives of all the tribes are known from most of the continental areas and probably occur on all, since allowance should be made for our scanty

knowledge of the fauna in the more remote regions. Furthermore, the more primitive genera, such as Lestremia, Campylomyza, Porricondyla, Dasyneura, Oligotrophus, Asphondylia, Lasioptera, Contarinia and Lestodiplosis, to take representatives from the various tribes, are found on most of the continental areas. They are represented among living forms by numerous species, many of them capable of subsisting under comparatively simple conditions and therefore not easily exterminated or isolated by adverse climatic changes.

It is noteworthy that of the seven genera known from the Baltic Amber, six are recorded also from Australia, three from New Zealand and two from Africa, and in all probability more of these genera occur in the land areas of the Southern Hemisphere. The African, Australian, New Zealand and South American faunae appear to contain a considerable number of ancient, highly specialized offshoots from the somewhat primitive world-wide genera (likewise represented) mentioned above and their occurrence suggests a zoological isolation where only the more vigorous or adaptable would establish themselves and where the natural tendency to variation would consequently be less subject to interference on the part of closely related strains or species in the process of formation.

One of the striking peculiarities in distribution is the occurrence of the genus Aplonyx in the Northern Hemisphere, where it is known only from the Mediterranean region and the vicinity of Salt Lake, Utah—localities favored by both host plants and insects. Species of Didactylomyia, a very peculiar form, occur in both North America and India, while the closely related Erosomyia and Indodiplosis are occidental and oriental, respectively.

The above and other data which need not be discussed in detail here, suggest no connection between food habits and a general distribution, since there does not appear to be a better representation of midges, such as Lestremia and Porricondyla breeding in decaying vegetable or woody tissues, materials which might easily drift to distant shores, than there is of bud or leaf-inhabiting forms which find only temporary shelter in plants and transform in the soil, such as species of Dasyneura and Contarinia. The same is true of species passing most of their existence within plant tissues, such as Lasioptera and Rhabdophaga, forms most likely to be carried great distances with the host plant. An equally wide distribution is true of predaceous midges with only a secondary relation to plants, such as Aphidoletes, Mycodiplosis and Arthrocnodax.

It is possible that some species may be widely disseminated through

the agency of birds, since we have records of birds feeding upon the galls of Cincticornia and there would appear to be no reason why those produced by the genus Caryomyia might not be equally acceptable. The same would be true to a certain extent of galls inhabited by Asteromyia and some species of Oligotrophus. It would be possible for midge larvae in firm, moderately thick-walled galls, to pass through the digestive tract of the bird without material injury, but this would be true of relatively few and owing to obvious limitations we can hardly consider this method of dissemination as one of great importance so far as the group as a whole is concerned.

The flight of gall midges, especially such a one as Miastor, is so feeble as to render it improbable that the wings of themselves are important in distributing the species over extended areas, and yet these midges are surprisingly prevalent where breeding conditions are favorable. The adults issue in swarms from decaying logs and apparently drift with the wind. The probability of this method of spread being an important one is supported by the recent investigations of McCulloch,[1] which show that Hessian flies may drift at least 2 miles with the wind. It is probable that this method of dissemination is very effective in the case of a number of gall midges.

Extended drifting, some possibly may prefer the word flight, with the wind, is not impossible when we recall that the migratory locust, Schistocerca peregrina has been found five hundred miles east of South America and is credited with probably having crossed from South America to Africa. Such extended flights are rendered more likely by the record of Sphinx convolvulus being found 420 miles from land, while other moths and longicorn beetles have been reported out at sea at a distance of 230 miles. The extensive northern flight of the cotton moth is another case of the same kind. Recent investigations by Collins[2] have shown that young gipsy moth caterpillars may be carried 20 to 25 miles by the wind—a distance which presumably could be greatly increased without endangering larval existence.

With the above data in mind it seems probable that the carriage of gall midges by floating vegetation and drifting islands, or by the activities of birds, has occurred to only a very limited extent and can not begin to explain the general distribution of these fragile forms. Winds undoubtedly carry these insects for short distances and might even, under exceptional conditions, transport them over seas, though this latter is by no means established and the present known distribution appears to be most easily explained by migrations (probably drifting with the wind in this group) over land areas about the same way as other mem-

bers of the fauna and the flora. There has been very probably a distribution from the north and there may have been an antarctic center of dispersion, though there appears to be no evidence within the group itself which would justify a definite opinion as to this.

[1] McCulloch, *Econ. Ent. J.*, 10, 1917, (162–168).
[2] *Ibid.*, 10, 1917, (170–176).

FERTILITY AND AGE IN THE DOMESTIC FOWL

By Raymond Pearl

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Communicated March 22, 1917

It has been shown by Marshall,[1] Pearl,[2] and King,[3] that in a variety of different mammals fertility changes with the age of the animal in a definite way. The nature of this change is that, starting at a low point at the beginning of the sexual life, the rate of fertility rises with advancing age to a maximum, and then declines with further increase in age, until total sterility is reached. Marshall inclined to the view, on the basis of statistics which I have elsewhere[4] shown to be wholly inadequate, that the domestic fowl exhibited the same sort of change in fertility with age. There has been no thorough or careful investigation of this matter in the fowl, based on adequate statistical material.

The writer has lately studied[5] the change in fertility with age in poultry, on the basis of 1114 matings of Barred Plymouth Rock fowls, covering in point of time a period of nine years. For the purposes of this investigation, and generally, the writer has defined *fertility* as the total net reproductive capacity of pairs of organisms, male and female, as indicated by their ability to produce *viable* individual offspring. As a working measure of fertility may be taken a reproductive or fertility index which expresses the percentage which the number of viable offspring actually produced from a particular mating or pair of parents is of the maximum number which would be physiologically possible within the time limits during which the mating endures. This states, in most general terms, the form of index developed for the special case of poultry breeding in the present investigation. The same idea can be adapted to the measurement and biometrical study of fertility in other sorts of animals, and probably in plants as well.

Specifically, the reproductive, or fertility index used in the work for poultry has the following form:

$$RI = 100C/E_m$$

where RI denotes the index for any particular mating, C the number of chickens produced from that mating and alive at the end of the third week after hatching, and E_m the total number of days from the day when this mating began to the day when the last egg from this mating began its incubation.

The reasoning on which this index is based is as follows: Maximum reproductive capacity, as represented by 100%, would be attained if during the period of the mating the hen laid one egg every day (maximum fecundity), and if further every one of such eggs were fertile, and if each embryo hatched, and the hatched chick lived to be three weeks of age. There would then be one living chick three weeks of age for each day during which the mating existed. If the hen does *not* lay every day during the mating season this will cause some reduction in the reproductive performance as measured by the index. Similarly a reduction in any of the other factors involved, prenatal or postnatal mortality, will have the same sort of result. The final percentage value which one obtains by calculating the index will be a true measure of the reproductive capacity of that mating, including within its view all of the primary factors of reproduction in poultry.

Applying this index to the problem of changing fertility with advancing age we have the following results:

Weighted mean reproductive indices for males of specified ages mated with females of all ages

Ages	*Weighted mean RI*
Male = 1, mated with ♀♀ of all ages	12.868
Male = 2, mated with ♀♀ of all ages	10.214
Male = 3, mated with ♀♀ of all ages	9.625

Weighted mean reproductive indices for females of specified ages mated with males of all ages

Ages	*Weighted mean RI*
Female = 1, mated with ♂♂ of all ages	12.765
Female = 2, mated with ♂♂ of all ages	11.660
Female = 3, mated with ♂♂ of all ages	10.722

From these figures it appears that there is a decline in net reproductive ability or fertility, as measured by the reproductive index, with advancing age in both sexes. The rate of the decline is, however, more rapid in the male than in the female.

It is desirable also to look at the matter from the standpoint of the mating. This may be done by taking means (weighted in proportion to the frequencies involved) of the reproductive indices for the advancing combined ages of the two animals entering into each class of matings. If this is done we get the following results:

Weighted mean reproductive indices for matings of individuals of the specified combined ages

Combined ages of mated individuals when mated	Cases	Weighted mean RI
2 years	796	13.083
3 years	190	11.121
4 years	113	11.119
5 years	12	7.458

The cases are too few to give reliable results after a combined age of four years. Up to that point, however, what occurs is this: There is a significant drop in reproductive ability as we pass from a combined age of two years for the mated birds to three years. In passing from three years to four there is no significant change in reproductive ability. In passing from a combined age of four years to that of five years there is a large drop in the net reproductive ability of the mating.

All of these figures agree in indicating that in the strain of the domestic fowl with which this work was done there is nothing approaching that law of fertility which has been found to hold for mammals, as pointed out at the beginning. Instead we find a steady and progressive decline in fertility after the first breeding season.

[1] Marshall, F. N. A., *The Physiology of Reproduction*, London, 1910, xvii + 706 pp.
[2] Pearl, R., *Science, New York*, N. S., 37, 1913 (226–228).
[3] King, H. D., *Anat. Rec., Philadelphia*, 11, 1916 (269–289).
[4] Pearl, R., *J. Exp. Zool., Philadelphia*, 13, 1912 (155–268).
[5] A complete and detailed report of this work will appear presently in *Genetics*.

A KINETIC HYPOTHESIS TO EXPLAIN THE FUNCTION OF ELECTRONS IN THE CHEMICAL COMBINATION OF ATOMS

By William A. Noyes

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Read before the Academy, April 16, 1917

Beginning with Davy[1] and Berzelius, during the first part of the nineteenth century chemists generally accepted the theory that chemical combination is due to electrical forces, but when Dumas discovered the chloroacetic acids in which chlorine atoms, supposedly negative, replace positive hydrogen atoms it was believed that the theory had been shown to be false and it was practically abandoned. Following this, for fifty years or more, a theory of valence which took no account of electrical forces was developed and while occasional reference was made to positive and negative atoms and groups, no definite meaning in an electrical sense was attached to these expressions. Helmholtz in his Faraday lecture in 1881[2] drew the attention of chemists once more to the very

close connection between chemical forces and electrical phenomena and spoke for the first time of "atoms of electricity." He also pointed out that the "sulphur of sulphuric acid must be charged with positive equivalents of electricity." In 1887 Arrhenius proposed his theory of electrolytic dissociation and with the help of Ostwald and van't Hoff the belief in a separation of molecules into electrically charged parts in solutions was rapidly accepted. J. J. Thompson[3] gave precision to the atomic character of electricity in 1897 when he demonstrated the material character of cathode rays and the very minute mass of the corpuscles carrying negative charges. Van't Hoff[4] seems to have suggested for the first time that electrically charged atoms may play a part in reactions not usually considered as ionic. The same idea was proposed by the author[5] and by Stieglitz,[6] a little later. J. J. Thompson[7] seems to have been the first to suggest that two atoms may be held together by the electrical foreces resulting from the transfer of an electron from one to the other. He assumed a shell of electrically positive matter within which there was a static arrangement of electrons. Abegg[8] in an entirely independent paper published the same year, discussed the relation between electrons and ionization and the connection with older theories of Helmholtz and others. He also raises, I think for the first time, the question of polar and non-polar volences but seems to have decided that the former are more probable.[9] Rutherford[10] has advanced strong reasons for considering that atoms contain a positive nucleus around which electrons are rotating and this hypotheses has been further developed by Bohr,[11] Nicholson,[12] Moseley[13] and others,

Physicists in general have directed their attention to rotating or rapidly moving electrons and to the relation between these and spectral lines, the disintegration of atoms and other phenomena involving individual atoms. Chemists, on the other hand, following the suggestion of J. J. Thomspson, have considered chiefly the rôle which the valence electrons probably play in the combination of atoms. Sir William Ramsay[14] in his address on "The Electron as an Element" considered that the electron takes a position between the two atoms which are held in combination. In a very recent paper, probably the last which he wrote,[15] he elaborates this thought further and describes models to illustrate the magnetic attractions which would result from electrons rotating in contiguous parts of two molecules. The magneton theory of the structure of the atom has also been developed elaborately by Parson.[16] It cannot account for ionization, where, if we accept the electron theory at all, electrons must be transferred completely from the positive atom or group to the negative.

Falk and Nelson,[17] Fry,[18] L. W. Jones,[19] Stieglitz[20] Bray and Branch,[21] G. N. Lewis,[22] and others have discussed the phenomena connected with the transfer of valence electrons from one atom to another but, with the exception of the magneton theories referred to above, no one, so far as I can discover, has suggested a possible connection between the motion of the valence electrons and chemical combination between atoms.

In the hypotheses here proposed the following assumptions, now more or less current among physicists and chemists, are made:

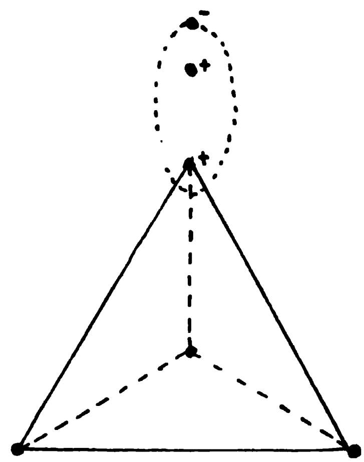
1. The atoms are of a complex structure made up of positive nuclei and electrons, of which the latter, at least, are in very rapid motion. If we assume that the electrons are 1/1800 the mass of hydrogen atoms and that they obey the same laws of motion as other atoms, their average velocity would be about sixty times the velocity of molecules of hydrogen (H_2). I will not attempt to discuss here the question whether the law of equipartition of energy actually holds for electrons.

2. That the electrons are of two kinds in their relation to the structure of the atom. Some of them are so involved in their orbits or motions among the positive nuclei that they can never escape from the atom. Others, called valence electrons, may be transferred to other atoms.

Let us suppose that two atoms, which have an affinity for each other are brought close together. A valence electron which is rotating around a positive nucleus in the first atom may find a positive nucleus in the second atom sufficiently close so that it will include the latter in its orbit and it may then continue to describe an orbit about the positive nuclei of the two atoms. During that portion of the orbit within the second atom that atom would become, on the whole, negative while the first atom would be positive. During the other part of the orbit each atom would be electrically neutral, and the atoms might fall apart. When we remember, however, the tremendous velocity of the electrons and the relatively sluggish motions of the atoms it seems evident that the motion of an electron in such an orbit might hold two atoms together. In ionization the electron would, of course, revolve about the nucleus of the negative atom leaving the other atom positive. It seems impossible to explain ionization otherwise than on the supposition of the complete transfer of the electron. This complete transfer in ionization is one of the strongest arguments against the magneton theory as the only explanation of chemical combination.

An interesting feature of the hypothesis proposed is that it may be used to account for that localization of the affinities in particular parts

of atoms which is indicated by many of the properties of organic compounds. Thus if we suppose that there are four (or eight) positive nuclei in a carbon atom around which valence electrons may rotate, an atom of hydrogen may be held to the neighborhood of one of these nuclei as indicated in the figure.



I wish to acknowledge my indebtedness to Julius Stieglitz, R. D. Carmichael, J. B. Shaw, Jacob Kunz, A. P. Carman, A. A. Noyes and R. C. Tolman, who have read the first draft of this paper and of whom several have made helpful suggestions.

[1] Davy, *London, Phil. Trans. R. Soc.*, 1807, p. 1.

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[3] Thomson, J. J., *Phil. Mag., London*, (Ser. 5), **44**, 1897, (291).

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[9] *Loc. cit.*, Note 8, p. 347.

[10] Rutherford, E., *Phil. Mag., London*, (Ser. 6), **21**, 1911, (669).

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[14] Ramsay, Sir W., *London, J. Chem. Soc.*, **93**, 1908, (774).

[15] Ramsay, Sir W., *London, Proc. R. Soc.*, (A), **92**, 1916, (451.)

[16] Parson, A magneton theory of the structure of the atom, *Washington, Smithsonian Inst., Misc. Collect.*, **65**, 1915, No. 11.

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[18] Fry, *J. Amer. Chem., Soc., Easton, Pa.*, **34**, 1912, (1268), *Zs. physik. Chem., Leipzig*, **76**, 1911, (385, 398, 591).
[19] Jones, L. W., *J. Amer. Chem., Easton, Pa.*, **36**, 1914, (1268).
[20] Stieglitz, *Ibid.*, **36**, 1914, (272); **38**, 1916, (2046).
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TRANSVERSE DISPLACEMENT INTERFEROMETRY

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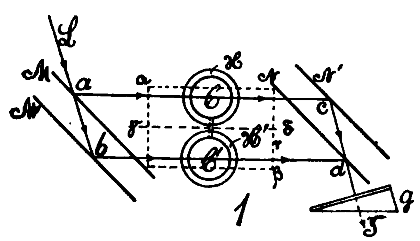
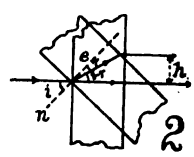
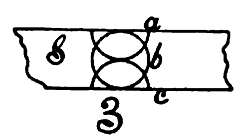
Communicated March 26, 1917

1. *Vertical Displacement of Ellipses.*—In the diagram figure 1, *M* and *N* are half silvered, *M′* and *N′* opaque mirrors; *C* and *C′* the U tube referred to in the preceding note and here to be removed. *L* is the collimated beam of white light and *G T* the direct vision spectroscope.

If the fringes are too small, when horizontally centered by the micrometer, the center of ellipses may be brought into the middle of the field of the telescope by sliding one component beam vertically over the other without appreciably changing the direction of the rays. In other words one illuminated spot at *d*, figure 1, is to move vertically relative







to the other by a small amount. This may be done by placing a thick plate glass compensator such as is seen in figure 2 in each of the component beams *abd* and *acd* and suitably rotating one plate relative to the other, each on a horizontal axis. Very little rotation is required. In the same way elliptical fringes may be changed to nearly linear horizontal fringes when desirable. If the fringes are to be sharp the slit must be very fine. When sunlight is used with a slit not too fine, each of the coincident sodium lines (D_1D_2) frequently shows a sharply defined helical or ropelike structure. This is a special phenomenon, which will be given further consideration presently.

The first result is interesting inasmuch as it is thus possible to displace the centers of ellipses not only horizontally as usual relative to the fixed sodium lines in the spectrum, but also *vertically* relatively to

the fixed horizontal shadow in the spectrum due to the fine wire across the slit. It does not occur when rays retrace their path. The following experiment was made to coordinate the vertical displacement of the component rays and centers of elliptic fringes. A glass plate $d = 0.705$ cm. thick was placed nearly normally in the beam *bd*, figure 1, and provided with a horizontal axis and graduated arc. The amount (i) of rotation of the plate, corresponding to the vertical displacement of one central fringe in the telescope (i. e., the passage of the ring *a*, into *b* and then into *c*, in the spectrum, S, figure 3) was then found to be, if i is the angle of incidence,

	i	h	Δh
No fringes, *c* fig. 3	3.5°	0.0149 cm.	....
One fringe, *b* fig. 3	5.0°	0.0214	0.0065 cm.
Two fringes, *a* fig. 3	6.5°	0.0281	0.0067

where h is the corresponding vertical displacement of the rays *bd* (see figure 2) and computed from (μ index, r angle of refraction)

$$h = d\,(\sin i - \cos i \tan r)$$

Thus the vertical displacement of rays corresponding to the vertical semiaxes of the central ellipse or one fringe is between 0.0065 to 0.0067 cm., i. e., a superior limit would be about 7×10^{-3} cm. Hence $h = N\ 0.007$ for N such central fringes is an excessive estimate.

The question is now suggested, in how far such an arrangement would fall short of being able to exhibit the drag of the ether in a rapidly rotating body, should such drag occur. In figure 1, let $\alpha\ \beta$ be a cylinder of glass with plane parallel ends, capable of rotating on the axle $\gamma\ \delta$. If l is the length of the cylinder μ its index of refraction, and and r the distance of either component ray (*ac*, *bd*) from the axis $\gamma\ \delta$, n the number of turns per second and V the velocity of light, we may write using the above estimate, N being the number of fringes displaced

$$h = \tfrac{1}{2}\, N \times 0.007 = 2\,\pi\, n\, r\, l\, \mu/V$$

since *ac* rises while *bd* falls. If $n = 200$, $r = 10$ cm., $l = 100$ cm, $V = 3 \times 10^{10}$, $\mu = 1.5$

$$N = 0.018, \text{ nearly.}$$

It would thus be necessary to estimate about 1/60 of a fringe, which is just beyond the limit of certainty even if n, r can be increased, and l multiplied by reflection. The device suggested is nevertheless of interest and deserves further consideration. In fact I have succeeded in increasing the sensitiveness about four times.

2. Displacement Interferometer. Jamin Type.—These considerations induced me to devote further study to the Jamin type of interferometer, figure 1. The mirrors M, N' were put on one pair of long slides (1.5 meters long) parallel to *ac* and the mirrors M' N on similar slides parallel to the former. In this way any distance *ac* or *bd* was available. The beams were about 16 cm. apart corresponding to a normal distance between the end mirrors (N N', M M') of about 12 cm. But these distances could also be increased from nearly zero (M and M' nearly contiguous) to about 20 cm. in view of the width of mirrors used. The angles at a, b, c, d were each about 45°, so that a rectangle of rays is in question.

The adjustment proved eventually to be quite simple by aid of a horizontal beam of sunlight with *weak* condenser lens and collimator. A thin wire is to be drawn across the slit. M and M' are first set for parallelism in the absence of N and N' by adjusting the images of the slit at the same level (horizontal) and equidistance on a distant wall. The mirrors N and N' are next put in place with the distances *acd* and *abd* about equal. The two images seen in the telescope at T (g removed) are then made to coincide both horizontally and vertically by adjusting N and N' and these are then slid by a small amount on their slides (direction *ac*) until the rays are coincident at d to the eye (light strips on the mirror coincide).

If now the grating g is inserted very, fine oblique fringes will usually be seen. These may be enlarged to a maximum by moving the micrometer controlling the displacement M' normal to itself. Somewhat coarser *horizontal* lines are thus obtained.

Finally the distant centers of the ellipses are brought into the center of the telescope by aid of the thick glass compensator (figure 2), (the equivalent air path of the other ray being correspondingly lengthened), by rotating the glass plate on a horizontal axis. The same result may be obtained by rotating N and N' on a horizontal axis, successively by small amounts, into parallelism with M and M'. But the compensator is more convenient.

The ellipses so obtained with common plate glass and a film grating at g were magnificent. A rough test of the diplacement interferometer was made by using the above plate glass of thickness $E = 0.434$ cm. where $z = E(\mu - 1) + 2B/\lambda^2 = 0.2428$ cm. In two experiments agreeing to within 10^{-4} cm., $2e = 0.3448$ cm. were the displacement obtained. Assuming that $\theta = 45°$, $2e \cos\theta = 0.2438$ cm. This agrees with z as nearly as may be expected unless θ is specifically measured.

3. Broad Slit Interferences. White Light. Residual Fringes.—Some

allusion has been made above to a type of interferences, totally different in size from the regular fringes, and seen in the broadened slit. These were finally isolated. They appear to best advantage in the absence of the spectroscope, in the broad *white* field of a very wide slit. The latter may be removed. They have the appearance when vertical of regular Young or Fresnellian fringes, very sharp and fine, achromatically black and white at the middle of the grid, colored and fainter outward. They are vertical when the enormously larger spectrum ellipses discussed above are centered. Like these they partake of displacement, here through the broad white slit image and this displacement is extremely sensitive in relation to the displacement of the opaque mirror M' (fig. 1) to which it is due. Thus a displacement of $\Delta N = 10^{-4}$ cm. of the latter, corresponded to a march of fringes through about .017 of the telescopic field of 3°, i.e. to 0.05°. This comprises two fringes or $\Delta N = 5 \times 10^{-5}$ cm. per fringe. Now these fringes are so fine, sharp and luminous that it should be possible on proper magnification to measure a few hundredths of this with an ocular micrometer. They supply the fine fiducial mark in displacement interferometry for which I have long been seeking. They appear in a white field, thus requiring no spectrum resolution, nor monochromiatc light. Moreover the source of light need not be intense.

I shall in referring to the new fringes use the term residual or achromatic fringes. Their theoretical breadth should be $\Delta N = \lambda / 2 \cos \theta$ agreeing substantially with the given estimate.

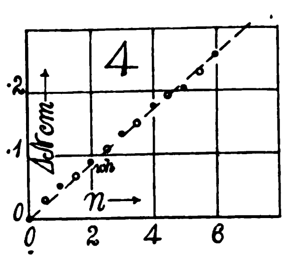
The displacement of fringes with ΔN at the mirror (when $n \lambda = \Delta N \cos \theta$) is so rapid that if they are lost it is difficult to find them, unless the centered large fringes in the spectroscope are first reëstablished. The latter are easily found. A removal of the prism grating gT, figure 1, and widening of the slit shows the achromatic fringes. The white pattern usually appears but once and is rarely present rhythmically as in the case of the next section for homogeneous light.

4. *Wide Slit. Homogenous Light. Sodium Flame.*—A clue to the nature of the residual fringes will be obtained when white light is replaced by homogeneous light. A strong large sodium flame near the mirror M, figure 1, suffices. The fringes now appear of the same size in yellow light and naturally spread over a much larger area of field. But on moving the mirror (ΔN increasing continually) forward very gradually, the homogeneous fringes alternately vanish and reappear, each time however enlarged in size but still straight, until at an intermediate position of symmetry enormous round ovals cover the yellow field. The fringes then diminish symmetrically in the same way. The

following data for the micrometer position corresponding to the clearest demarcations of yellow fringes are illustrative. At least six periods (n) are easily detected in each side of the ovals ($n = 0$) Thus,

n	6	5	4	3	2	1	0 etc.
$\Delta N \times 10^3$	0	49	90	127	171	214	243 cm. etc.

These intervals, since it is impossible to establish the maximums states of presence or absence of fringes quite sharply, are practically equidistant as the similar series in figure 4 indicates. Fringes in presence are here shown by black dots, the circles denote clear fields. Thus the mean period of reappearance is $\Delta N = 0.042$ cm.; or a path difference of $2\,\Delta N \cos\theta = 0.059$ cm; or a shift of ray parallel to itself ($2\,\Delta N \sin\theta = 0.059$ cm.) of the same amount.



The reason for this rhythm can only be the two wave lengths of the D_1 and D_2 lines of the sodium flame, originally detected in the colors of thin plates by Fizeau. Hence a relatively enormous shift of micrometer of nearly half a millimeter is equivalent to the wave length interval $\Delta\lambda = 6 \times 10^{-8}$ cm.; or $\Delta\lambda / \Delta N = 6 \times 10^{-8} / 42 \times 10^{-3} = 1.4 \times 10^{-6}$. Treating the case in terms of the interferences of thin plates and two wavelengths λ and $\lambda + d\lambda$

$$n\lambda = \text{constant, or } \frac{\Delta\lambda}{\lambda} = \frac{\Delta n}{n} = \frac{1}{n} \text{ for each period.}$$

Hence $$n\lambda = \frac{\lambda^2}{\Delta\lambda} = 2\Delta N \cos\theta \quad \text{or} \quad \Delta N = \lambda^2/2\Delta\lambda \cos\theta$$

since ($\theta = 45°$) approximately $\lambda = 60 \times 10^{-6}$ cm. $\Delta\lambda = 6 \times 10^{-8}$ cm.
$\cos\theta = 0.71$ $\Delta N = 0.041$ cm.,

agreeing as nearly as may be expected with the experimental datum. The apparatus thus serves incidentally for investigating such properties of spectrum lines as Michelson in particular has detected.

With white light the interference grid rarely reappears rhythmically nor does it correspond to the zero'th period of figure 4, i.e., to the ovals for sodium light. It is usually exactly of the size of some higher order, in yellow light, as at "*wh*" in fig. 4. It does not require such sharp horizontal and vertical coincidence of the superposed images, as the spectrum fringes.

In a flash of the arc, showing many sharp spectrum lines in all colors,

each of the lines gives evidence of the phenomenon; i.e., if the residual fringes are oblique, each such line is strongly helical in appearance.

Hence the equation for the residual fringes may be assumed to be

$$n\,\lambda = (e-e')\,[\mu \cos\,(\,r - \alpha) - \cos\,i]$$

when e and e' are the thicknesses of the two half silver plates, μ their index of refraction, i the angle of incidence, r the angle of refraction of an incident ray, and where α is the outstanding angle between the faces of the differential glass wedge, $e-e'$ thick at the ray in question. The possibility of throwing these fringes into any order of size, their small extent, sharpness and great abundance of light constitute their value for measurement.

THE PROTEINS OF THE PEANUT, ARACHIS HYPOGAEA

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Communicated by R. Pearl, March 27, 1917

During the last ten years the culture of peanuts has increased rapidly in the United States. This increase is partly due to the fact that the boll weevil has made the growing of cotton unprofitable in various sections of the South and peanuts are now grown as a supplementary crop. Most of the peanuts thus produced are sold to the oil mills to be pressed for the oil which they contain. Shelled peanuts yield from 40 to 50% of oil. This edible oil compares favorably with olive oil and is used for culinary purposes and for making olemargarin. Press cake from shelled peanuts contains about 45% of protein (N x 6.25). This cake is ground to a meal which is rapidly finding favor as a cattle food. Peanut meal is now quoted at $35 per ton and peanut oil at $1.05 per gallon.

Previous to the present work, the only published experiments on the protein of the peanut seem to be those described by Ritthausen[2] in a paper which appeared in 1880. This author found that the proteins of the peanut consist chiefly of globulin and his analytical data led him to believe that only one globulin is present.

We have made fractional precipitations of the proteins extracted by sodium chloride solutions from oil-free peanut meal and have isolated two globulins, one of which is present only to a small extent. The globulin composing the greater part of the protein in the peanut is precipitated, when in 10% sodium chloride solution, by adding ammonium sulphate to 0.2 of saturation. To this globulin we have given the name *arachin*. After filtering off the arachin, the second globulin may be ob-

tained by dialysis or by saturating the filtrate with ammonium sulfate. This second globulin we have named *conarachin.*

Isolation of Arachin.—An extract was made by mixing 500 grams of peanut meal with 2.5 liters of a 10% aqueous solution of sodium chloride. This mixture was then run through a mill and the resulting thin liquid was filtered clear through paper pulp. To the filtrate, solid ammonium sulphate was added gradually until the solution became 0.2 saturated with this salt. A precipitate began to appear at 0.15 of saturation and increased rapidly until 0.2 of saturation was reached, when precipitation practically ceased and did not occur again until much more ammonium sulfate was added. On allowing the 0.2 saturated solution to stand overnight, the precipitate settled in a very compact form so that the supernatant liquid could be easily decanted. The precipitate was washed with a 10% sodium chloride solution containing ammonium sulphate to 0.2 of saturation. The residue was then redissolved in a small volume of 10% sodium chloride, and the resulting solution was filtered and dialyzed until chlorides were removed. In this manner the greater part of the globulin present in the meal was obtained as a white powder. This was suspended in alcohol for twenty-four hours and in anhydrous ether for another twenty-four hours. It was then filtered off and freed from ether in a vacuum desiccator and finally dried to constant weight at 110°C.

Isolation of Conarachin.—The filtrate from which arachin had been removed, and which was now 0.2 saturated with ammonium sulphate, was completely saturated by adding more ammonium sulphate. This treatment produced a small quantity of precipitate. This was redissolved in 10% sodium chloride and the filtered solution dialyzed until free from chlorides. The conarachin was then dried in the same manner as the arachin.

Isolation of an Albumin.—Beside arachin and conarachin sodium chloride solutions also extract from peanut meal a trace of albumin which coagulates at 65 to 70°C. Carbon and hydrogen determinations made on a small quantity of this albumin gave results which agree closely with the values obtained by Osborne[3] and his co-workers for the legumelins frequently found in the seeds of leguminous plants.

The difference in the composition of arachin and conarachin is shown by the following analytical results:

	ARACHIN Per cent	CONARACHIN Per cent
C	52.15	51.17
H	6.93	6.87
N	18.29	18.29
S	0.40	1.09
O	22.23	22.58

It will be seen that the greatest difference between these two globulins is in the percentage of sulphur, which is nearly three times as great in conarachin as in arachin. Another marked difference between these two proteins is shown by the figures representing the distribution of nitrogen as determined by the Hausmann method. The figures are as follows:

N	ARACHIN	CONARACHIN
Amide	2.03	2.07
Humin	0.22	0.22
Basic	4.96	6.55
Non-basic	11.07	9.40
Total	18.28	18.24

The high percentage of basic nitrogen in these proteins is most striking and, as far as we know, conarachin contains more basic nitrogen than any other seed globulin yet examined. For the sake of comparison, the percentages of basic nitrogen in proteins from some seeds commonly used for foods are also given below.[4]

PROTEIN	SOURCE	BASIC NITROGEN *Per cent*
Zein	Maize	0.49
Gliadin	Rye	0.91
Gliadin	Wheat	1.09
Phaseolin	Kidney bean	3.62
Vicilin	Pea	4.92
Arachin	Peanut	4.96
Legumin	Pea	5.11
Excelsin	Brazil nut	5.76
Edestin	Hemp seed	5.97
Globulin	Cocoanut	6.06
Conarachin	Peanut	6.55

Percentage of Basic Amino Acids in the Globulins of the Peanut.—Arachin and conarachin were hydrolyzed by boiling with 20% hydrochloric acid for twenty-four hours. The hydrolyzates were then analyzed by the Van Slyke method. The following results are corrected for the solubilities of the phosphotungstates of the bases:

	ARACHIN *Per cent*	CONARACHIN *Per cent*
Arginine	13.51	14.60
Histidine	1.88	1.83
Lysine	4.98	6.04
Cystine	0.85	1.07

The figures for cystine are undoubtedly too low as they represent only the cystine which escaped destruction during the hydrolysis of the proteins. Both proteins also gave a strong qualitative test for trypto-

phane. For the sake of comparison the percentages of lysine in the proteins of some common seeds and in muscle from some animals are also given in the following tables:

Lysine content of some vegetable proteins

PROTEIN	SOURCE	PER CENT
Zein	Maize	0.00[5]
Gliadin	Wheat	*1.21[6]
Legumin	Pea	4.29[7]
Phaseolin	Kidney bean	4.58[8]
Arachin	Peanut	*4.98
Conarachin	Peanut	*6.04

Lysine in muscle substance of different animals[9]

Scallops (*Pecten irradians*)	5.77
Halibut (*Hippoglossus vulgaris*)	7.45
Chicken	7.24
Ox	7.59

The figures marked by an asterisk were obtained by the Van Slyke method and, therefore, probably represent the maximum percentage of lysine obtainable. Those not so marked were obtained by Kossel's absolute method and may be somewhat too low. It is seen, however, that the percentages of lysine in arachin and conarachin of the peanut are relatively high and, indeed, approach the lysine content of muscle substance of different animals. It will also be noted that no lysine has been found in zein and that the maximum percentage of lysine obtained from gliadin is only 1.21%.

Osborne and Mendel[10] and other workers have shown that lysine is essential to the growth of animals. Nutrition experiments indicate[11] that the animal organism cannot synthesize lysine which must, therefore, be provided in suitable quantity in the food to insure normal growth. Since the muscle substance of animals contains about seven per cent of lysine, foods deficient in this essential amino acid should be supplemented by the addition of other foods which contain a high percentage of lysine. Diets of wheat and corn, both of which contain but little lysine, should therefore prove more efficient if supplemented by some food of high lysine content. Peanut meal appears to be well adapted to this purpose. From a nutritive standpoint it is one of our cheapest foods and seems to possess no objectionable properties. Animals fed on it thrive well and increase rapidly in weight.[12] It, therefore, seems probable that corn and wheat could be much better utilized and a considerable saving in the cost of feeding effected by supplementing these cereals with peanut meal.

[1] This article is published by permission of the Secretary of Agriculture. A part of the work of which this is a summary appeared in *J. Biol. Chem.*, **28**, 1916, (77). The part relating to the determination of the basic amino acids in the proteins will appear in the May number of the same journal.

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[11] Hopkins F. G., *London, J. Chem. Soc.*. **109**, 1916, (629).

[12] *U. S. Dept. Agric., Washington, Weekly News Letter*, **4**, No. 22, 1916.

A DESIGN-SEQUENCE FROM NEW MEXICO

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Communicated by W. H. Holmes, April 2, 1917

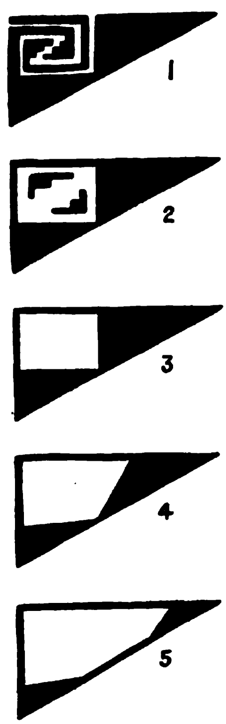
Much has been written on the development of geometrical decoration among primitive people, and many design-sequences have been arranged; the latter, however, have almost always been based on preconceived theoretical ideas, and the material for them has usually been selected from specimens whose relative ages have not been known. Such sequences cannot, therefore, be regarded as indicating surely the tendencies of design growth, for the specimens regarded as early may in fact have been late, and the development may thus have taken place in the opposite direction to the one postulated; or, again, the specimens may all have been of one period and may represent either contemporary variants of a single design-phase, or entirely unrelated parts of other unsuspected sequences. It has accordingly been impossible in most cases to do more than guess as to whether any given change in design has been from the natural to the conventional or vice versa; whether toward simplification or toward elaboration.

The only safe method for the working out of developments in decorative art is to build up one's sequences from chronologically sequent material, and so let one's theories form themselves from the sequences. In the case of aboriginal American art this ideal has been very hard to attain because of the scarcity of stratified sites and the corresponding difficulty of obtaining relatively datable specimens.

In the Rio Grande district of New Mexico, however, students have recently been recovering stratigraphical data which establish an orderly

succession of several pottery styles; so that almost any vessel may be placed in its proper chronological relation to any other. Close studies of the decoration of these vessels should enable us to recognize and tabulate enough true design-sequences to form the basis for a correct appreciation of the art tendencies in that area. Several such sequences are already becoming apparent; the accompanying incomplete example is given as an illustration.

While the five units in the series are from vessels from various sites, stratigraphical studies by Mr. Nelson at San Cristobal and by me at Pecos allow it to be stated positively that they are arranged in their proper chronological order. A description follows.



In the early black-on-white pottery a common design consists of a large triangle with two of its corners filled in with black; a pair of opposed stepped figures mounted on interlocking 'stalks' occupies the remaining rectangular space (fig. 1). In a primitive type of biscuitware which succeeds the black-on-white the same triangular element is often seen, and the two opposed stepped figures are also present but have lost their interlocking 'stalks' and hang suspended in the open space (fig. 2). In the biscuitware of a slightly later period the stepped elements drop out altogether, but the triangle holds to its original shape (fig. 3). In still later examples a progressive modification takes place in the cut-off and filled-in corners of the triangle; they become smaller and their two contiguous sides are no longer at right angles to each other (fig. 4). A final step is shown in figure 5; it is characteristic of the last type of biscuitware with which we are familiar.

This series represents, of course, only a short period in the life of this particular design; what phases it passed through in reaching the complicated form in which we first encounter it are as yet unknown; nor can we tell whether or not it had any later developments. In this short sequence we see: first, a progressive simplification due to the dropping out of elements (figs. 1, 2, 3); second, a modification in the shape of the remaining elements (figs. 4, 5). These data are, of course, too scanty for general conclusions, as they illustrate only one of many designs; they show, however, what interesting results may confidently be expected.

THE EQUILIBRIUM BETWEEN CARBON MONOXIDE, CARBON DIOXIDE, SULPHUR DIOXIDE, AND FREE SULPHUR

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Communicated by A. L. Day, April 4, 1917

The value of a study of the equilibrium between CO, Co_2, SO_2, and S_2 lies not in the reaction itself, important as this undoubtedly is both to the scientific and technical worlds, but rather in the fact that from such a study the free energy of sulphur dioxide, a quantity indispensable to a proper study of the chemistry of the sulphur compounds, may be directly obtained. That such an investigation has not heretofore been completed may be ascribed to the many difficulties which appear at the outset to block the way. The complex subsidiary reactions taking place at comparatively low temperatures are the source of many of these and would seem to have deterred Lewis and Lacey[1] from continuing their investigations in this direction. These reactions may be thus summarized:

$$\begin{aligned} &CO + S \rightleftharpoons COS \\ &2\,COS \rightleftharpoons CO_2 + CS_2 \\ &CS_2 \rightleftharpoons CS + S \end{aligned}$$

They rendered the ordinary stream methods of investigation useless since by these methods samples could not be obtained which had not reacted after leaving the equilibrium chamber. A semi-stream method devised by the writer obviated this difficulty and samples so obtained from equilibrium mixtures at 1263° abs. showed no evidence of any such reaction taking place. Since the velocities of these reactions greatly exceed that of the main reaction, which goes according to the equation:

$$CO + \tfrac{1}{2}\,SO_2 \rightleftharpoons CO_2 + \tfrac{1}{4}\,S_2,$$

the absence of the products of the former clearly indicated that by this method cooling effects had been eliminated. Details of the method will be given in the final paper. By it a given gas mixture may be heated for a given time at a given temperature and a correct sample of the same then obtained for analysis. It has also the added merit that if the temperature be properly varied the equilibrium can be approached from either side.

A few of the results obtained in this way are presented in the following tables, K being defined by the equation:

$$K = \frac{P_{CO_2}^{\frac{1}{2}} \cdot P_{S_2}^{\frac{1}{4}}}{P_{CO} \cdot P_{SO_2}^{\frac{1}{2}}}$$

All of these with the exception of the measurements at 1458 were obtained from experiments in which platinum was present as a contact mass and the variations are probably due to analytical errors. The two measurements at 1458 were obtained from experiments in which a bare porcelain tube was used as a container and it is unlikely that equilibrium was reached in either case. An average of these two probably will be very near the correct value.

From these results the thermodynamical constant I for this reaction may be calculated by means of the equation:[2]

$$-RT \ln K = \Delta H_0 - \Delta\Gamma T \ln T - \tfrac{1}{2}\Delta\Gamma_1 T^2 - \tfrac{1}{6}\Delta\Gamma_2 T^3 + IT \qquad (1)$$

The additional data necessary has been well summarized by Lewis[3] and his co-workers with the exception of the specific heat of S_2 and and the value of ΔH_0 for the reaction $S_R + O_2 = SO_2$. The assumption that the former is the same as that of oxygen leads to the equation for the increase in heat capacity for the main reaction:

$$\Delta\Gamma = -(1.375 - 0.0028\,T + 0.000\,000\,93\,T^2)$$

The further assumption that Berthelot's[4,5] value of 69400 cal. for the heat of formation of sulphur dioxide from its elements at room temperatures gives for the reaction

$$\tfrac{1}{2}(S_R + O_2 = SO_2) \qquad \Delta H_0 = -34\,195 \text{ cal.}$$

and for the main reaction

$$CO + \tfrac{1}{2}\,SO_2 = CO_2 + \tfrac{1}{4}\,S_2 \qquad \Delta H_0 = -25\,915 \text{ cal.}$$

Substituting these values in equation (1) we have

$$-RT \ln K = -25\,915 + 1.375\,T \ln T - 0.0014\,T^2 + 0.000\,000\,155\,T^3 + IT$$

In table 3 will be found the values of I calculated by means of this equation from the results found in tables 1 and 2.

TABLE 1

EQUILIBRIUM APPROACHED FROM THE $CO\text{-}SO_2$ SIDE

TEMPERATURES		
Absolute	Centigrade	Log K
1271	998	1.79
1273	1000	1.73
1273	1000	1.77
1458	1185	1.13
1463	1190	1.19

TABLE 2

EQUILIBRIUM APPROACHED FROM THE CO_2-S_2 SIDE

TEMPERATURES Absolute	Centigrade	Log K
1277	1004	1.78
1458	1185	1.27
1463	1190	1.18

TABLE 3

TEMPERATURE Absolute	I CALCULATED
1271	3.88
1273	4.12
1273	3.94
1277	3.85
1458	3.97
1463	3.96
1463	4.00
Average value =	3.96

The agreement speaks not only for the results themselves, but justifies the assumption of $-$ 34195 cal. for ΔH_0 for the reaction, $\frac{1}{2}$ ($S_R + O_2 = SO_2$) and indicates the correctness of Berthelot's value for the heat of formation of sulphur dioxide. The free energy equations for the two reactions

$$CO + \tfrac{1}{2} SO_2 === CO_2 + \tfrac{1}{4} S_2 \text{ and}$$
$$S_R + O_2 === SO_2$$

may now safely be written.

The former will be

$$\Delta F_0 = -25\,915 + 1.375\,T \ln T - 0.0014T^2 + 0.0000001\,55T^3 + 3.96T$$

and the latter

$$\Delta F_0 = -68\,391 + 3.62T \ln T - 0.0007\,T^2 + 0.000\,000\,317\,T^3 - 25.03T$$

Under standard conditions, therefore,

$$CO + \tfrac{1}{2} SO_2 = CO_2 + \tfrac{1}{4} S_2 \qquad \Delta F_{298} = -22\,529 \text{ cal.}$$
$$S_R + O_2 = SO_2 \qquad \Delta F_{298} = -69\,757 \text{ cal.}^{7}$$

The final paper will appear in the *Journal of the American Chemical Society*.

[1] Lewis, G. N. and Lacey N. *J. Amer. Chem. Soc., Easton, Pa.*, **37**, 1915, (1976)

[2] Lewis, G. N. *Ibid.*, **35**, 1913, (1).

[3] Lewis, G. N. and Randall, M. *Ibid.*, **34**, 1912; (1128), **36**, 1914, (2468); **37**, 1915, (465).

[4] *Ann. chim. phys., Paris*, (Ser. 5), 22, 1881, (428).

[5] Berthelot's average value of 69260 includes results obtained from experiments in which the sulfur dioxide was determined by means of an alkaline solution. This procedure prob-

ably led to too low results and it is significant that those so obtained were lower than those obtained when iodometric methods were used. See 'The Iodometric Determination of Sulfur Dioxide and the Sulfites,' J. B. Ferguson, *J. Amer. Chem. Soc., Easton, Pa.*, 39, 1917, (364).

[6] Thomsen's value is 71080. (Thomsen, *Thermische Untersuchungen*, 2, p. 251)

[7] This value agrees with that derived from some preliminary investigations by M. Randall on sulfur and water (*Thesis, Mass. Inst. Tech., Boston*, 1912) and also with that obtained by Lewis and Bichowsky in a more complete investigation carried out at higher temperatures (private communication).

PHYSIOLOGICAL EFFECT ON GROWTH AND REPRODUCTION OF RATIONS BALANCED FROM RESTRICTED SOURCES

By E. B. Hart, E. V. McCollum, H. Steenbock, and G. C. Humphrey

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Communicated by R. Pearl, March 29, 1917

Our early work[1] on the nutrition of herbivora with restricted rations demonstrated clearly the inadequacy of the accepted theory as to what constitutes a balanced or complete ration. Up to that time total protein—without reference to quality—energy, and ash materials were considered the essentials of a ration. The latter, however, occupied no position in the mathematical expression of the standards developed. The standards have been stated only in terms of total digestible protein and energy. It is, however, probably true that in a practical sense, and with the generally accepted knowledge of the quality of materials accumulated from a long and varied experience, that such standards have had and will continue to have very great value; but their limitations are also made evident by this earlier work and are emphasized by what we have since done. Within the past few years our knowledge[2] of the essentials of a ration have expanded and today we would consider a ration complete and efficient only when it contained protein of adequate quantity and quality, adequate energy, ash materials in proper quantity and proportion and two factors of unknown constitution (vitamines), designated from this laboratory[3] fat soluble A and water soluble B.

In addition to the above normal factors there may be introduced with natural food-stuffs the important factor of toxicity.[4] This can be wholly absent or so mild in its effects as to be entirely obscured when the other essentials of a ration are at an optimum adjustment; or with fair adjustment it may only reveal its effects when the ration is continued over a very long time and the animal involved in the extra strains of reproduction and milk secretion. This resistance to toxicity

is very materially increased through a proper adjustment of the normal factors of nutrition.

With this recognition of all the normal factors for adequate nutrition there must not simultaneously arise a desire for a mathematical expression of these factors in feeding standards. It is doubtful if this can ever be done, at least for certain of them. For example, the rôle of the mineral nutrients is so varied, including such widely separate functions as construction and control through antagonism as to make it seem futile to attempt an expression of absolute requirements when natural foods, with their diversity of mineral content, are involved. Even the recognition of differences in the quality of proteins and their relation to nutrition[5] will make it more difficult to continue expressing protein requirements in exact quantities than before the development of such knowledge; and what can be said of the quantitative requirements of fat soluble A and water soluble B and their supply in feeding materials?

All these developments of the last few years emphasize the need of a thorough study of the contributing nutritive factors of a *single* food stuff, and in the state of our present knowledge such information will be secured only by physiological tests, involving the animal in reproduction and milk secretion. A contributing factor by a natural food may at times be in the nature of toxicity and this may serve as a harmful and abnormal factor. As such knowledge develops and it becomes clear that this or that single food material will supply adequately the normal nutritive factors, not measurable by any quantitative chemical method, such as fat soluble A, water soluble B, or mineral nutrients, we will return with more confidence to the mathematical standard that only involves the energy and protein supply of that single food material. This confidence in the expressed quantities of energy and protein available in a food-stuff will rest upon the definite information that they become physiological effective only when they form part of a ration which carries one or a number of food-stuffs supplying adequately the other nutritive factors. With such an understanding the feeding standards developed on the energy-protein basis would continue to be theoretically sound and of very great practical value. As illustrative of our position, and taken from our own experience with wheat grain feeding we would feel reasonably safe if a wheat grain ration, based on protein and energy and to be fed continuously to a growing herbivorous animal, was built around alfalfa hay; less safe if built around corn stover, and fearful of disaster should the roughage used be wheat straw.

In our earlier experiments a 'balanced' ration from the wheat plant

gave fair growth, but complete failure in reproduction with heifers, while a 'balanced' ration from the corn plant was successful.

In our attempts to locate the trouble in the all wheat ration, wheat grain—wheat straw—we have fed rations made up of corn grain and wheat straw. Here the offspring were weak and often born dead. When to that same ration, however, a suitable salt mixture was added so that the ash content of the ration was like that of the all corn ration perfect offspring resulted. This would clearly indicate that one of the deficiencies of an all wheat plant ration was a proper salt mixture. When, however, the corn grain in the above ration was displaced by the wheat grain and the ration consisted of wheat grain—wheat straw and salts, disaster again resulted, which showed the presence of another disturbing factor in the wheat grain. Calves born by mothers upon this ration showed peculiar deflections of the head, inability to get up and suckle the mother, and in most cases have died within a few hours after birth.

These experiments indicate that in the all wheat plant ration there were two factors operative against normal nutrition, namely, a poor salt mixture and inherent toxicity of the wheat grain. When the wheat grain was coupled with corn stover we have sometimes met with success and sometimes with failure in the character of the offspring. With strong mothers it appears that the corn stover may become an 'antidote' and thereby furnish sufficient of all the normal factors of nutrition so as to enable the animal to reproduce normally.

The possibility of the toxicity being destroyed by heat was also investigated and baked wheat was fed with corn stover. This had no effect whatever in improving the wheat kernel.

In other cases the wheat grain-corn stover ration had butter fat added to it for the purpose of supplying plentifully the growth promoting—factor-fat soluble A—now known to be necessary for growth and supplied abundantly in butter fat. It was thought possible that the wheat grain-wheat straw ration was somewhat deficient in this material. Butter fat additions, however, did not uniformly improve the ration. We had a number of failures in reproduction, and also a number of successes with its use. This would again emphasize the probability of the presence of a toxic substance in the wheat grain.

When, however, the wheat grain was mixed with a legume hay, such as alfalfa, so that the latter formed but 20% of the ration, we have had perfect success in all cases in the production of normal offspring, at least for the first gestation. The improvement resulting from the use of the alfalfa must lie in introducing into the ration a better salt mix-

ture, perhaps a better protein mixture, and an abundance of growth promoting substances, all of which may contribute toward making it possible for the cell to destroy or resist the action of the toxic substance introduced. However, in the second gestation period on the same ration—wheat grain, wheat straw, alfalfa hay, the calves were weak, and in one case blind, but lived. This is extremely interesting as illustrating the cumulative effect of this toxicity.

Where corn stover was wholly substituted for the wheat straw we had a number of successes and also a number of failures in the first gestation period. Apparently this roughage was not as effective as an 'antidote' to the toxicity as the legume hay.

We had thought it possible in our earlier work that the acidity of the wheat ration was an important factor in the results recorded. It was true that the urine of the all-wheat plant fed animals showed a slight acidity to litmus due to a low intake of bases in the ration. If this were an important factor in our results then the successful corn ration might be disturbed with acids and give us results similar to the wheat ration. This however, we found not to be the case; for when to an all corn ration there were added mineral acids, such as sulphuric and phosphoric acids, in such proportions as to make the acidity of the urine of a degree similar to that of a wheat-wheat straw fed animal, the offspring were strong and normal in every respect. Even the addition of a high proportion of magnesium salts to a corn ration did not disturb in any way its power of producing normal offspring.

The results detailed above indicate clearly that wheat grain contains a toxic material, and later work has shown that this is very prominent in the embryo of the seed. When wheat embryo is imposed on corn stover so as to bring into the ration seven to eight times the amount of embryo that would be introduced when feeding whole wheat, the result is likely to be an early abortion. The calf is now dropped at six to eight months; this demonstrates that the increased mass of the toxic material produces this disturbance at a somewhat more rapid rate. This result was particularly apt to occur where no other grain was used with the embryo. With both corn meal and corn stover in the ration the detrimental effect of the wheat embryo was nullified, at least for a single gestation period.

It is an interesting fact that in the wheat milling industry the embryo passes into wheat bran in small amounts but in much greater quantities in wheat middlings. The wheat flour that is used for bread making has the least content of embryo of any of the wheat by-products.

In an attempt to obtain an anatomical picture of the condition re-

FIG. 1. CALF FROM COW 562

Wheat embryo 3 parts, corn starch 4 parts, corn stover 7 parts. The embryo of the wheat grain carries a considerable mass of toxicity. Massing this in the ration brought on early abortion with a gestation period of six to seven months. This ration was a so-called 'balanced' ration

FIG. 2. CALF FROM COW 648

Second gestation on a ration of wheat grain 8 parts, wheat gluten 0.3 part, wheat straw 2.9 parts, alfalfa hay 2.9 parts. In the first gestation this cow produced a strong calf on this ration. In this second gestation period the calf was carried to full time, but was weak and was fed from a bottle; grew strong, but the front legs were weak and it stood for the first few days of its life on the first joints. *This calf was blind.* The mother remained in apparently good condition. This again illustrates the cumulative effect of the wheat toxicity.

sponsible for the physiological disturbances as already described, Dr. Bunting of the Medical School of the University, kindly consented to make a histological study of the tissues from a number of the abnormal calves. In general no striking lesions were revealed. Livers and kidneys showed some degeneration (hydropic) changes, but the nervous tissues gave the most evidence of the presence of an excessive amount of fluid—a condition of oedema. This histological picture was anal-

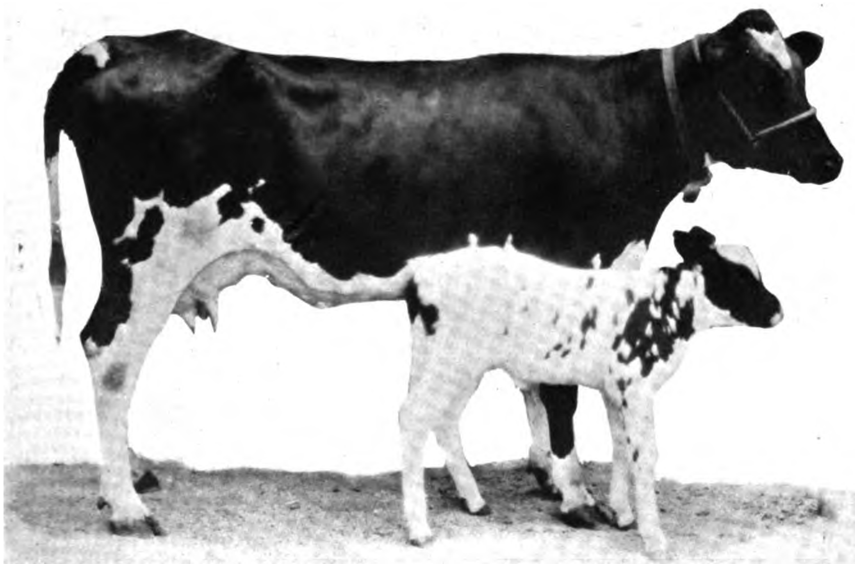

FIG. 3. COW 662 AND CALF

Successful reproduction in the presence of the embryo. A ration of corn meal 4 parts, corn starch 1 part, wheat embryo 2 parts, corn stover 7 parts. At least for the first gestation the 'antidotal' properties of corn meal and corn stover were sufficient to overcome the toxic effect of the wheat embryo. Without the corn meal and with only wheat embryo, starch, and corn stover in the ration reproduction would have been premature and the calf either dead or markedly undersize (see figure 1).

ogous to that of beri beri, the result of feeding polished rice, and it also simulated, if it was not identical, with that obtained from the spinal cord of pigs on certain rations as described in a previous publication.[4] The oedema was observed between the membranes covering the cord, around the blood vessels and around the nerve cells. In these instances the nerve cell and their nuclei were shrunken, the latter staining more intensely than normally. No abnomalities in medullation of the fibers of the cord as demonstrable by the Weigert stain were observed. While the observations did not point to anything especially characteristic it

is probable that the motor disturbances observed in the animals can be referred to the oedematous condition of the nervous tissues. The cause of beri beri is ascribed to the absence or deficiency of certain essential factors in the diet, particularly to water soluble B. In the case of excessive wheat feeding it would appear that the essential causal factor for disaster to growth and reproduction is a toxic substance which either interferes with the utilization of materials necessary for the full development of the nervous system of the animal or directly with the normal functioning of this tissue. This would account for the blindness observed in some of the heifers and also for the failure of muscular coordination apparent in the new born calves produced on rations of large whole wheat content.

It was also apparent that rations producing an early delivery of offspring would usually lead to a failure of the animal to remove properly the afterbirth, with its attending dangers of infection; and an overabundance of a material like wheat straw in a ration, owing to its low salt content, becomes an important factor in premature birth.

An observation in our experimental work of interest to veterinarians was the low resistance to other diseases of the mothers fed the wheat ration. In an outbreak of anthrax in the university herd the only losses to occur from this disease in our experimental herd were among the wheat grain fed animals.

The principle[5] laid down as to what factors must be present in a ration of natural origin in order that it becomes efficient for both growth and reproduction is well supported by these data. This principle postulates that there must be present efficient proteins, adequate energy, proper salt mixture, fat soluble A and water soluble B (vitamines) and an absence of toxicity or a toxicity of such mildness as to become innocuous in the presence of the other normal factors of nutrition. The presence of toxicity in the wheat kernel as the explanatory factor for these records rests not only upon the evidence secured with swine and rats but also on that presented here. *It is not a deficiency phenomenon.* A wheat grain, corn stover ration often failed, not only when used alone but when there was added to it the most likely limiting factor, fat soluble A, as butter fat.

The recognition of these normal factors of nutrition and the further recognition of the occurrence in apparently normal food-stuffs of substances of mild toxicity will be of immense advantage in arriving at an understanding of the oft reported troubles with farm animals which today are either not understood or their etiology wrongly assigned; and in the field of human nutrition the same principles will apply.

When a few years ago the corn crop of Nebraska failed to mature because of drought, but early rains had produced a bumper wheat crop it left many farmers with little to feed their breeding stock but wheat grain and certain roughages. In many cases where this was done the calves were born either dead or weak, with great financial losses to many breeders. No one would have suspected that the ration was a factor in these disasters, but it undoubtedly was the direct cause of the trouble.

When Dakota farmers, with their only roughage as wheat straw, try to build up an animal husbandry industry there is likely to arise trouble in reproduction with this class of animals unless other roughages with better salt mixtures are brought into the ration. We are informed that there is already much trouble with reproduction by cows in the Dakotas wherever much wheat straw is fed. Such facts as these must emphasize the importance of an understanding of all the factors of animal nutrition and in addition an understanding of all the factors contributed by any particular food-stuff. It should further emphasize how such studies can furnish the facts which will aid the animal feeder in avoiding the danger zones of his art. *We need more effort placed on the accumulation of information on the physiological behavior of feeding stuffs than on the attempts to bring out new mathematical expressions of feeding standards.*

These experiments further show the limitations of the theory of a 'balanced' ration as now expressed and indicate the very great importance of other factors besides protein and energy in the successful diet. It was indeed surprising to find that the common wheat kernel had a low toxicity; but such factors as toxicity, growth promoting substances of unknown nature, proper balance of salts, indicate how complex the problems of animal nutrition really are and how necessary it is that these factors be clearly exposed in order that we may place the various feeds in their proper category. We have pointed out how a material of low toxicity, such as the wheat kernel, may be used with success. A good roughage like a legume hay was an admirable 'antidote.' Even corn meal and a poorer roughage like corn stover served to offset the detrimental effects of a large mass of wheat embryo. This also illustrates how an adjustment of the normal factors of nutrition may conceal the presence of the detrimental factors.

It is important to keep constantly in mind that the disclosure of either a nutritive deficiency or the presence of an abnormal factor in a common natural food stuff should not necessarily condemn its use. It should, however, emphasize the need of combining it in the ration with

those other natural products which will either supply abundantly the deficiencies or act as an 'antidote' to any inherent toxicity.

[1] Hart, E. B., McCollum, E. V., Steenbock, H., and Humphrey, G. C., *Wisconsin Exp. Sta. Research Bull.* No. 17, 1911.

[2] Stepp, W., *Zs. Biol., München*, **57**, 1912, (135) 62, 1913, (405); Hopkins, F. G., *J. Physiol.* **44**, 425, 1912; Funk, C., *Zs. Physiol. Chem.*, **88**, 1913; (352), **92**, 1914, (13); McCollum, E. V., and Davis, M., *J. Biol. Chem.*, **15**, 1913; (167), **23**, 1915, (231); Osborne, T. B. and Mendel, L. B. *Ibid.*, **15**, 1913, (311).

[3] McCollum, E. V., and Kennedy, C., *J. Biol. Chem.*, **24**, 1916, (491).

[4] Hart, E. B., and McCollum, E. V., *Ibid.*, **19**, 1914, (373); McCollum, E. V., and Simmonds, N., and Pitz, W., *Ibid.*, **25**, 1916, (105); Hart, E. B., Miller, W. C., and McCollum, E. V., *Ibid.*, **25**, 1916, (239).

[5] Hart, E. B., Miller, W. C., and McCollum, E. V., *Ibid.*, **25**, 1916, (239); McCollum E. V., Simmonds, N., and Pitz, W., *Ibid.*, **25**, 1916, (105).

WHAT DETERMINES THE DURATION OF LIFE IN METAZOA?

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Read before the Academy, April 16, 1917

1. It can be stated as a fact that most if not all organisms have a characteristic duration of life. To give extreme examples, many insects have a duration of life measured in weeks only, while the Californian sequoia has a duration of life of thousands of years, and in the human being the duration of life is proverbially three score and ten. The question arises: What determines this characteristic duration of life?

Bütschli was the first to point out that unicellular organisms have an unlimited duration of life and this idea has become very popular through Weismann. All recent researches support the correctness of this idea. As a consequence we are forced to the conclusion that natural death is a phenomenon found almost exclusively in organisms which are composed of different organs. The idea that natural death is connected with the compound character of organisms is supported by two facts; namely, first, the observation that a cutting will survive the whole plant, while the cutting if not separated would have died with the whole plant. By the method of cuttings the life of the individual plant can be prolonged apparently indefinitely.

The second fact is that if we take pains to transplant certain cells from an old organism successively to young organisms, these cells will outlive the original individual indefinitely—they are, in other words, immortal. The proof can only be furnished with the aid of marked cells and Leo Loeb selected for this purpose the cancer cell which is easily distinguishable from other cells by its rapid growth. He thus

showed that the cancer cell is apparently immortal and the same could possibly be shown for many other tissue cells if it were only possible to mark them and thus follow them individually through a series of successive graftings.

The experiments on hybridization also support the idea that natural death is due to some maladjustment in a compound organism. By fertilizing an egg with the sperm of another species we can bring about a shortening of the duration of life to an almost infinitely small fraction of the normal duration; and in some cases the opposite result may also be obtained. It depends whether in the hybrid the maladaptation of the parts is greater or smaller than in the pure breed. The problem then before the biologist is to find out the physicochemical character of this maladaptation which results in old age and death.

2. In certain organisms life is divided into two or more well defined morphological periods separated by an equally well defined process, the metamorphosis. Thus in the frog the metamorphosis from the tadpole stage consists in the growth of the legs and the absorption of the tail and gills. We know now through Gudernatsch that this metamorphosis can be brought about at any time by feeding the tadpole with thyroid, and it is possible that the natural duration of the tadpole stage is determined by the production of a certain quantity of a substance, possibly a specific constituent or product of the thyroid gland in the tadpole. This substance acts on many different organs in the body in a different way, causing the buds of the legs to grow and causing the tail and the gills to die by autolysis and by phagocytosis.

This suggests the possibility that the termination of the second stage in the frog's life, namely natural death, is also determined by the production in the body of one (or more) substances which bring about the termination of the second stage of life as definitely as the thyroid substance terminates the tadpole stage. It matters little whether we call such substances terminating one of the stages in life hormones or poisons.

3. In order to test the validity of such reasoning we have approached the problem by a method which one of us suggested in 1908[1] for this purpose, namely by ascertaining whether there is a temperature coefficient for the duration of life.

It is definitely established that a number of life phenomena have a temperature coefficient characteristic of chemical reactions, namely of about 2 for a difference of temperature of about 10°C.; although at the lower and upper temperature limit this coefficient changes more rapidly than it is usually found to change in purely chemical processes. This might be interpreted to mean that the order of magnitude of the

rate of such life phenomena and of chemical reactions is the same and that such life phenomena are due to definite chemical reactions, e.g., a reaction resulting in the formation of a hormone. Such a temperature coefficient was first observed by F. Lillie and Knowlton for the development of the egg of the frog and has since been found to exist generally.

If we could show that in a species with a definite metamorphosis there exists not only a definite temperature coefficient for the duration of life of the order of that of a chemical reaction, but that this coefficient is approximately identical with the temperature coefficient of the duration of the larval stage or stages, we should not have certainty but at least some probability that the duration of life is determined by causes similar to those which determine the duration of the larval stages. Our experiments were made on the fruit fly which has three definite stages, larval, pupa, and imago. We have already reported on some experiments in this direction[2] but our new experiments were made on forms which had been freed from all microorganisms. This was necessary on account of Metchnikoff's suggestion that poisons formed by intestinal microorganisms play an important rôle in limiting the duration of life.

Moreover, in our previous experiments only water or sugar solutions were offered to the flies and it was necessary to keep them on adequate food. It was found that on 2% glucose-agar the flies lived as long as on their natural food, yeast or yeast-agar. We preferred glucose-agar since it does not allow the larvae to hatch and we had to guard against this possibility since otherwise it would have been impossible to obtain correct figures for the death rate of the old flies. About 20 flies were kept in a large flask and about 8 to 10 different flasks with flies were used for the determination of life for any one temperature. The number of flies which died were counted each day and the duration of life for each temperature is the average for all the cultures kept at this temperature. The experiments were carried on in thermostats whose temperature was constant within one-tenth of a degree. The temperatures selected were 30°, 25°, 20°, 15°, and 10°. Table 1 gives the duration of the three stages for the different temperatures with the probable error.

These figures show, first, that the temperature coefficient for the larval stage, the pupa stage, and the life of the imago is of the order of 2 or above for 10°C., which is characteristic for both chemical reactions and life phenomena in general.

They show in addition, however, that the ratio of the duration of

the three stages is approximately constant for all temperatures as far as our experiments go. The ratios are given in table 2.

The values for the ratios at 15°, 20°, and 25° agree very well, while at 30° the value for the imago falls off; one of the cultures at 30° gave, however, the value of 0.22.

We may, therefore, say that as far as our present experiments go the ratio of the duration of life of the insect to the duration of the larval stage is approximately constant for all temperatures and that the same is true for the ratio of the larval to the pupa stage.

If it is permissible to consider the production of a hormone as the limiting factor for the duration of the larval stage we face the possibility that the production of a hormone (or a poison) may be also the

TABLE 1

DURATION OF THE THREE STAGES OF THE LIFE OF *Drosophila* IN DAYS

STAGES	10°	15°	20°	25°	30°	31.5°
Larva....	56.8 ±1.1	18.0 ±0.2	8.38 ±0.04	6.32 ±0.04	4.62 ±0.04	4.9 ±0.05
Pupa....	Does not hatch	13.5 ±0.2	6.1 ±0.1	4.22 ±0.03	3.43 ±0.04	
Imago[3]...			40.0 ±0.9	28.5 ±0.2	13.7 ±0.4	
Total length of life................			54.5	39.0	21.7	

TABLE 2

RATIO OF DURATION OF THE THREE STAGES

STAGES	15°	20°	25°	30°
Larva: pupa....................	1.31 ±0.03	1.37 ±0.03	1.49 ±0.02	1.35 ±0.02
Larva: imago..................		0.21 ±0.2	0.22 ±0.1	0.33 ±0.1

limiting factor for the duration of the imago stage and the duration of a normal life in general.

On the basis of this assumption we can readily understand that by inadequate feeding of the larva the duration of the larval stage may be prolonged since the inadequate feeding may delay the formation of the hormone responsible for metamorphosis. It would be wrong to state that the duration of the larval stage is determined by the energy value of the food taken up by a normal larva since in the frog at least the larval period can apparently be prolonged indefinitely if certain hormones are lacking though the larva may grow considerably during this period. In the experiments on parthenogenetic frogs one of us has twice obtained a tadpole unable to metamorphose in a year or in a year and a half. One of these had grown beyond the normal tadpole

size. The latter was finally fed thyroid and began to metamorphose within a week. Both tadpoles were almost albinotic and from a recent paper by Allen it seems possible that in both larvae the hypophysis was abnormally developed or lacking. It is obvious that the production of a definite hormone and not the calory value of food is the limiting factor for the duration of the larval period.

4. *Conclusion.*—The experiments show that *Drosophila* has a temperature coefficient for the duration of life of the order of magnitude of that of a chemical reaction. Since the animals used in our experiments were absolutely free from microorganisms this influence of temperature on the duration of life cannot be attributed to bacterial poisons.

It was found, moreover, that the duration of the pupa stage is at each temperature proportional to the duration of the larval stage and that the same proportionality exists between the duration of the life of the insect to the larval stage, as far as our present experiments go. Since we know that the duration of the larval stage is determined by a specific hormone we are compelled to consider the possibility that the duration of life is also primarily determined by the formation of poisonous substances or a hormone in the body.

[1] Loeb, J., *Arch. ges. Physiol., Bonn,* 124, 1908, (411); Moore, A. R., *Arch. Entw.-Mech., Leipzig,* 29, 1910, (287). For a fuller discussion of the subject see Loeb, *The Organism as a Whole,* New York, 1916.

[2] Loeb, J., and Northrop, J. H., these PROCEEDINGS, 2, 1916, (456).

[3] The experiments on the duration of life of the imago at 10° and 15° are not yet completed. From present indications it seems that for 15° the duration of life is in the neighborhood of ninety days, which agrees with the value expected according to the theory. At 10° the flies have been alive for ninety days and have not yet begun to die.

THE INTERRELATION BETWEEN DIET AND BODY CONDITION AND THE ENERGY PRODUCTION DURING MECHANICAL WORK IN THE DOG

By R. J. Anderson and Graham Lusk

PHYSIOLOGICAL LABORATORY, CORNELL UNIVERSITY MEDICAL COLLEGE, NEW YORK CITY

Read before the Academy, April 16, 1917

A dog weighing 8 kgm., in poor nutritive condition, was received into the laboratory. His basal metabolism, as measured during a period of absolute rest eighteen hours after partaking of an adequate standard diet, was 17.6 calories per hour. Traveling at the rate of 2.5 miles (4.0 km.) per hour he required 11.0 calories above the basal to move his body 1000 meters during the period beginning about eighteen hours after food ingestion, whereas 11.2 calories were required if 70 grams of

glucose had been administered immediately before the experiment began. These facts appear in the following table:

The interrelation between diet and body condition and the energy production during mechanical work in the dog

CONDITIONS	NUMBER OF EXPERIMENTS	WEIGHT IN KILOGRAMS	CALORIES OF METABOLISM	RESPIRATORY QUOTIENT	WORK IN DISTANCE TRAVELED PER HOUR: Miles	WORK IN DISTANCE TRAVELED PER HOUR: Meters	CALORIES ABOVE THE BASAL PER 1000 METERS	ENERGY IN KILOGRAMS TO MOVE 1 KGM. 1 METER
Basal	1	8.0	17.6	0.84				
Work, no food	1	8.3	60.7	0.78	2.48	3925	11.0	0.585
Work, glucose 70 grams	2	8.4	63.4	1.00	2.51	4170	11.2	0.566
Basal	4	9.1	17.2	0.85				
Work, no food	4	9.2	76.8	0.80	2.95	4760	12.4	0.578
Work + glucose, 70 grams	2	9.6	76.6	0.94	2.95	4750	12.5	0.550
Work + glucose, 100 grams	1	9.7	76.4	0.95	2.94	4740	12.5	0.550
Rest + glucose, 70 grams	2	9.3	20.7	1.05				
Work + meat, 750 grams	1	9.6	92.4	0.80	2.91	4700	16.0	0.708
Rest + meat, 750 grams	1	9.6	29.7	0.83				
Work + alanin, 20 grams	1	9.3	82.0	0.78	2.96	4780	13.6	0.620
Rest + alanin, 20 grams	1	9.4	21.1	0.84				
Fasting								
Day 3 + work		8.75	72.4	0.72	2.97	4800	11.7	0.569
4		8.6	16.1	0.73				
5 + work		8.55	70.1	0.73	2.94	4750	11.4	0.567
7		8.35	15.2	0.74				
8		8.10	14.2	0.71				
8 + work		8.10	70.6	0.72	3.11	5020	11.2	0.588
13		7.60	12.4	0.74				
13 + work		7.60	62.5	0.72	2.92	4710	10.6	0.595
15		7.45	13.0	0.73				

In a second series of experiments begun after an interval of six weeks, during which time the dog had added a kilogram to his body weight, it was found that when he moved at the rate of 2.95 miles (4.75 km.) per hour 12.4 calories were required to propel his body 1000 meters if the dog had been given no food in the morning, and 12.5 calories were required when 70 or even 100 grams of glucose had been administered. The rise in the respiratory quotient demonstrates the influx of glucose molecules for use in the cells for the production of power, and yet this influx caused no increase whatever in the heat production.

On the eighth day of fasting, when the body weight had fallen to 8.1

kgm. (the original weight of the dog), it required 11.2 calories to move the dog 1000 meters. On the thirteenth day of fasting, the basal heat production having fallen to 14.2 calories, it required 10.6 calories to move the dog, now weighing 7.6 kgm., a distance of 1000 meters.

A loss of body weight of 19% accomplished through fasting was accompanied by a fall in the basal metabolism of 28%, and an economy in the fuel necessary to move the body 1000 meters amounting to 15%.

This method of calculation, however, does not give an accurate picture of the interrelation between the nutritive condition of the dog and the amount of energy of metabolism necessary to accomplish a given amount of mechanical work, for it is obvious that to move a body weight of 9.2 kgm. through 1000 meters of distance would require a greater amount of energy than to move a body weight of 7.6 kgm. over the same distance. When the computation is based upon the energy equivalent in kilogrammeters required to move 1 kgm. of body substance 1 meter through space, it is found that an average of 0.580 kilogrammeter is required for that purpose with a maximum variation of ±2.5% no matter what is the nutritive condition of the animal. (The reduction in this value after giving glucose in solution to the dog is dependent on the fact that the weight of the glucose solution (±280 grams) was reckoned as part of the weight of the dog. The carrying of this inert mass, however, did not appreciably increase the level of metabolism, as has already been set forth above.)

One may conclude, therefore, that *the accomplishment of a given amount of mechanical work is always at the expense of a given amount of energy, and that the amount of energy required for mechanical work is independent of the physical condition of the subject and independent of the quantity of carbohydrate food present in the gastrointestinal tract.*

No experiments have been made after giving fat but, since the authorities (Zuntz, Atwater and Benedict) are agreed upon the equal economy of iso-dynamic values of these two classes of food substances in the production of a given amount of mechanical work, it may be assumed that ingested fat acts like carbohydrate in metabolism when work is performed.

When meat is ingested the situation changes. Meat is a stimulant of the metabolism, as are several of its component amino-acids. The effect of work upon the heat production during the fifth and sixth hours after giving 750 grams of meat, and during the third and fourth hours after giving 20 grams of alanin, is summarized in the following table:

Chart indicating the influence of meat and of alanin upon the energy production during mechanical work in the dog

	Calories of metabolism per hour
Rest after meat, 750 grams	29.7
Basal	17.2
Difference	12.5
Work + meat, 750 grams	92.4
Work, no food	76.8
Difference	15.6
Rest + alanin, 20 grams	21.1
Basal	17.2
Difference	3.9
Work + alanin, 20 grams	82.0
Work, no food	76.8
Difference	5.2

It is apparent that meat acts as a stimulant to metabolism, raising not only the level of the basal metabolism but even increasing the quantity of additional energy required to move the dog, above the quantity to be expected were there a summation between the effect of protein stimulation and work to be accomplished. The same holds true of alanin, a simple cleavage product of meat which, in metabolism, is convertible into lactic or pyruvic acid, either of which may be transformed into glucose. Clear and sharp cut appears the distinction between the behavior of glucose itself and of alanin, which is convertible into glucose but whose intermediary acid products constitute a direct stimulus to metabolism, while the metabolites of glucose do not.

Summarizing, one may conclude:

1. Protein in the dietary is primarily for the repair of the tissues. It is not beneficial for the economical performance of work. In excess, it largely increases the heat production which a working organism is called on to eliminate.

2. One may reduce the basal requirement for energy by starvation, and this process may economize food in the case of those who do no mechanical work.

3. To accomplish a given amount of work a given amount of fuel energy is required, irrespective of the nutritive condition of the organism. This is of primary importance in the maintenance of armies or munition workers. Carbohydrate food fuel is utilized without loss.

4. Upon the capacity for heroism in the farmer will depend in the immediate future the security of the world.

REPORT OF THE ANNUAL MEETING

Prepared by the Home Secretary

The annual meeting of the Academy was held at the Smithsonian Institution in Washington, April 16, 17, and 18, 1917.

Seventy-three members were present as follows: Messrs. C. G. Abbot, Abel, Barnard, Becker, Bogert, Cannon, Cattell, Chittenden, W. B. Clark, F. W. Clarke, J. M. Clarke, Conklin, Coulter, Crew, Cross, Dall, Dana, Davenport, Davis, Day, Dewey, Donaldson, Fewkes, Flexner, Frost, Gomberg, Hague, Hale, E. H. Hall, Harper, Hayford, Hillebrand, Holmes, Howard, Howell, Iddings, Jennings, Lillie, Loeb, Lusk, Mall, Meltzer, Merriam, Merritt, Michelson, Millikan, Moore, Morley, E. L. Nichols, E. F. Nichols, A. A. Noyes, W. A. Noyes, H. F. Osborn, Pearl, Pickering, Pupin, Ransome, Remsen, Rosa, Schlesinger, Scott, Erwin F. Smith, Stieglitz, Van Vleck, Vaughan, Walcott, Webster, Welch, Wheeler, D. White, H. S. White, Wilson, R. W. Wood.

BUSINESS SESSIONS

The President announced the death since the Autumn Meeting of one foreign Associate: Gaston Darboux.

REPORTS FROM OFFICERS OF THE ACADEMY

The annual report of the President to Congress for 1916 was presented and distributed.

The President announced the appointment of the Local Committee as follows: Charles G. Abbot, Chairman; Whitman Cross, William H. Dall, William F. Hillebrand, Charles D. Walcott.

A Finance Committee for the Proceedings was also appointed, as follows: C. B. Davenport, Chairman; M. T. Bogert, W. B. Clark, F. R. Lillie, Raymond Pearl.

Other committee appointments, with date of expiration were as follows:

Program: C. G. Abbot, Chairman, 1920; Raymond Pearl, 1920.

Henry Draper Fund: A. A. Michelson, to succeed himself, 1920.

J. Lawrence Smith Fund: Whitman Cross, 1922.

Comstock Fund: A. G. Webster, to succeed himself, 1920.

Marsh Fund: E. S. Dana, to succeed himself, 1920.

Murray Fund: G. H. Parker, to succeed himself, 1920.

Marcellus Hartley Fund: Theobald Smith and A. G. Webster, to succeed themselves, 1920.

On Publication: E. B. Rosa.

The Home Secretary presented the following communication from Mr. Edwin B. Frost, resigning as a member of the Trustees of the Watson Fund:

Dear Mr. Day: April 12, 1917.

Please present to the Council my resignation as a member of the Trustees of the Watson Fund. It is necessary for me to give up further service on this committee, but I am willing to continue as Chairman of the Bache Committee for another year.

Very truly yours, Edwin B. Frost.

The resignation of Mr. Frost was accepted and Mr. A. O. LEUSCHNER was elected a member of the Trustees of the Watson Fund.

Mr. WILLIAM TRELEASE was appointed to represent the Academy at the inauguration of Walter Albert Jessup, as President of Iowa State University.

The report of the Home Secretary was presented as follows:

The President of the National Academy of Sciences.

SIR: I have the honor to present the following report on the publications and membership of the National Academy of Sciences for the year ending April 18, 1917.

The following publications have been issued and distributed: the *Memoirs of the National Academy of Sciences*, Volume 14, Memoir 1, entitled "Report on Researches on the Chemical and Mineralogical Composition of Meteorites, with Especial Reference to Their Minor Constituents," by George Perkins Merrill; Biographical Memoirs of Edward Singleton Holden, by W. W. Campbell; Theodore Nicholas Gill, by William Healey Dall; George William Hill, by Ernest W. Brown; and Alfred Marshall Mayer, by Alfred G. Mayer and Robert S. Woodward; also the Annual Report for 1915.

Two members have died since the last annual meeting: Cleveland Abbe, elected in 1879, died October 28, 1916; and Josiah Royce, elected in 1906, died September 14, 1916. Two foreign associates have also died: Sir William Ramsey, elected 1904, died July 24, 1916; and Gaston Darboux, elected in 1913, died March, 1917. There are 147 active members on the membership list, one honorary member and 37 foreign associates.

(Signed) ARTHUR L. DAY, *Home Secretary.*

The report of the Foreign Secretary was presented as follows:

On April 6, 1917, a cablegram was prepared by the Foreign Secretary of the Academy, with the approval of the five members of the Council:

"The entrance of the United States into the war unites our men of science with yours in a common cause. The National Academy of Sciences, acting through the National Research Council, which has been designated by President Wilson and the Council of National Defense to mobilize the research facilities of the country, would gladly co-operate in any scientific researches still underlying the solution of military or industrial problems.

HALE, *Foreign Secretary.*

The cablegram was sent to the Royal Society, London, England; to the Académie des Sciences, Paris, France; to the Accademia dei Lincei, Rome, Italy; and to the Académie des Sciences, Petrograd, Russia. (Signed) GEORGE E. HALE, *Foreign Secretary.*

The report of the Treasurer was presented in its printed form and approved:

REPORTS FROM COMMITTEES ON TRUST FUNDS

A report was received from the Directors of the Bache Fund, signed by Edwin B. Frost (Chairman), stating that since the annual meeting of the Academy in April, 1916, grants Nos. 196–201 (as announced in the PROCEEDINGS, vol. 2, p. 743) and Nos. 202–204 (as announced below, p. 398) had been made; and that reports on previous grants had been received as follows:

No. 186, J. VOÛTE, Royal Observatory, Cape of Good Hope. From a letter dated January 15, 1915, it appears that about 13,000 transits had then been observed for the purpose of determining stellar parallaxes. From a letter dated July 22, 1916: "The parallax work on the transit circle is nearly finished, but I have started a small series of about 125 stars on the astrographic refractor."

No. 187, H. H. LANE, University of Oklahoma. Preliminary report on "The Structure and Function in the Development of the Special Senses in Mammals," *Science, New York*, N. S., **43**, 1916, (179).

No. 188 and 196, H. W. NORRIS, Grinnell College. Research nearly ready for publication. Preliminary results on the cranial nerve of coecilians published in *Science, New York*, 43, 1916, (182).

No. 189, E. J. WERBER, Yale University. Experiments aiming at the origin of monsters. The greater part of the results are published in *J. Exp. Zoöl., Philadelphia*, 21, Nos. 3 and 4, 1916. Record of award closed.

No. 190, H. S. JENNINGS, Johns Hopkins University. For research in variation, heredity and evolution. Preliminary result published in *Genetics, Cambridge*, 1, 1916, (407–534).

No. 184 and 191, P. W. BRIDGMAN, Harvard University. To assist in the study of the effect of high pressures on the electrical resistance of metals. The results have been published in these PROCEEDINGS, 3, 1917, (10–12); *Boston, Proc. Amer. Acad. Arts, Sci.*, 52, 1917, (571–646); and in a theoretical article soon to appear in the *Physical Review*. Record of award closed.

No. 192, J. P. IDDINGS, Brinklow, Md. For a microscopic study of volcanic rocks from the Dutch Indies, French Oceania and the Philippines. Preliminary results are published in these PROCEEDINGS, 2, 1916, (531). Research being continued under Grant 203.

No. 194, REGINALD A. DALY, Harvard University. For the construction of a deep sea thermograph. The vital part of the instrument is practically ready for testing, and it is hoped that the whole will be ready for work by May 1. The clock-work has been tested and shows capacity for taking 250 readings with one cast of the machine overboard. The manuscript descriptive of the instrument, by Dr. Harry Clark, the expert designer, to be illustrated with several full-page plates, is nearly completed. The necessary expenditure has considerably exceeded the appropriation.

No. 195, R. W. HEGNER, University of Michigan. To assist a research of "Differential Mitoses in the Germ Cell Cycle of Dineutes Nigrior." Preliminary publication in these PROCEEDINGS, 2, 1916, (356–360).

No. 197, GREGORY P. BAXTER, Harvard University. "The Preparation of Pure Anhydrous Zinc Chloride and its Analysis by Electrolysis" are under way and will probably be completed before the end of the college year.

No. 198, LOUIS T. MORE, University of Cincinnati. Preliminary results are published in the *Physical Review, Ithaca*, Ser. 2, 9, 1917, (198–204), by J. S. Allen and L. M. Alexander, "On the Effect of Previous Filtering upon the Absorption of High Frequency X-Rays." The research continues.

No. 199, FRANKLIN P. REAGAN, Princeton University. The partial results are published in the *Anatomical Record*, Feb., 1917, under the title "Anterior Haematopoedis in Teleost Embryos Under Continual Observation," by F. P. Reagan, E. E. MacMorland, and Stuart Mudd.

During the year the Directors unanimously voted to cancel the rule heretofore effective for grants from the Bache Fund, which stated "No appropriation will be made to assist any investigation begun or conducted under any national or state government, either wholly or in part." The cash income balance of the fund on April 2, 1917, was $846.30; but there are outstanding grants, not yet paid, amounting to $2050. On three of these grants, amounting to $1600, the recipients of which live in foreign countries, it seems to be impossible to make payments until the end of the war.

A report was received from the Trustees of the Watson Fund, signed by Edwin B. Frost (Chairman), W. L. Elkins, and G. C. Comstock stating that grants Nos. 13–14 (as announced below, p. 398) were recommended, and that the available income of the Watson Fund in April 1, 1917, was $1213.65. Reports on previous grants were as follows:

No. 13, JOHN A. MILLER, Sproul Observatory, Swarthmore College. For measuring plates to determine stellar parallaxes, The money has been paid to an assistant, Miss Marie S. Bender, who has given all her energy to the measurement, and reduction of plates for determining parallaxes. She has measured 130 plates of 24 different series, and has computed the orientation factor and oriented 122 plates and has discussed eight different series by means of graphs. The continuance of the grant is requested.

No. 11, JOHN E. MELLISH. To undertake observations at the Yerkes Observatory. Used in 1915–16. Record of award closed.

No. 12, HERBERT C. WILSON, Carleton College, Northfield, Minn. For measuring the positions of asteroids on photographic plates. From a letter dated April 7, 1917: "We have measured all of our asteroid plates up to the beginning of this year, and all have been reduced. We are now going over the reductions thoroughly, checking them. . . . It will be a couple of months yet before we can get the matter ready for the press."

A report was received from the Committee on the Henry Draper Fund, signed by W. W. Campbell (Chairman), as follows:

The Committee recommends that a grant of $300 be made to Prof. Joel Stebbins, Director of the Observatory of the University of Illinois, in support of his investigations with sensitive photometers particularly the photo-electric cell photometer; the grant to be available on or before September 1, 1917. It is understood that this grant will be supplemented by the appropriation of a sum two or three times as great to be made by the University of Illinois, and that the total sum will constitute the salary of a research assistant for Professor Stebbins during the academic year 1917–18. It is expected that all of the assistant's time in the first year will be available for the researches in photometry and that after the first year the University authorities will assume the full responsibility for the assistant's salary. Professor Stebbins reports that improvements have been made in his photo-electric photometer. "A new quartz cell as compared with the cell used at Mount Hamilton (1915) gives twice the light effect, with the dark current five times smaller. It is therefore conservative to say that the sensibility has been doubled since 1915."

A report was received from the Committee on the J. Lawrence Smith Fund, signed by E. W. Morley (Chairman), stating that the income now available for grants is $677.13, and containing the following reports on previous grants:

No. 3, E. O. HOVEY, Curator in geology and paleontology, American Museum of Natural History, New York. For the study of certain meteorites. Dr. Hovey has been for some years out of the country and has not been reached by letters. He has suggested the repayment of the amount of this grant.

No. 4, C. C. TROWBRIDGE, Department of Physics, Columbia University, New York. To aid in study of luminous trains of meteors. During the last year good progress has been made in collecting photographs of drawings of meteor trains. One interesting result obtained is that the height of the zone of luminous trains agrees with the heights obtained in recent investigations on the auroral zone and is suggestive.

No. 6, S. A. MITCHELL, University of Virginia. To aid in securing observations of paths and radiants of meteors and in computing orbits where observations are sufficient. Good progress has been made in the work; and the committee are unanimous in recommending a further grant of $400.

Nos. 5 and 7, GEORGE P. MERRILL, of the U. S. National Museum. To aid in further studies of the occurrence of certain elements in meteorites. Upon the work for which he received grant No. 5 his final report has been made. This work is in progress.

A report was received from the Directors of the Wolcott Gibbs Fund, signed by C. L. Jackson (Chairman), Edgar F. Smith, and T. W. Richards, stating that the accumulated income of the Fund amounted to $356.11 on March

1, 1917, and that grants Nos. 7–8 (as announced below, p. 398) had been made. Reports from holders of previous grants are as follows:

Nos. 1 and 5, MARY E. HOLMES, Mt. Holyoke College. Her paper on the electrical deposition of copper is nearly ready for publication, and she has studied the electrical separation of cadmium from eight other metals.

No. 3, W. J. HALE, University of Michigan. Results published in a paper on "The formation of Cyclopentadienodihydropyridazines," *J. Amer. Chem. Soc.*, 1916, (2537–2545). Record of award closed.

No. 4, W. D. HARKINS, University of Chicago. Three papers are submitted: *a.* "The Free Energy of Dilution and the Freezing-Point Lowerings in Solutions of Some Salts of Various Types of Ionization and of Salt Mixtures." Harkins and R. E. Hall, *J. Amer. Chem. Soc.*, 38, (2658–2676). *b.* "The Freezing-Point Lowerings in Aqueous Solutions of Salts and Mixtures of Salts and of a Salt with a Non-electrolyte." Harkins and W. A. Roberts, *Ibid.*, (2676–2679). *c.* "Studies on the Cobalt-ammines. I. Various Ionization Types as determined by the Freezing-Point Lowerings in Aqueous Solution, Together with Conductance Measurements." Harkins, Hall and Roberts, *Ibid.*, (2643–2568).

No. 6. G. P. BAXTER, Harvard University. Studies, soon to be completed, on: *a.* Sources of error in the electrolytic determination of the atomic weight of cadmium, and analysis of cadmic sulphate. *b.* Gaseous impurities of silver and iodine. *c.* Relation between arsenious trioxide and iodine.

A report was received from the Committee on the Murray Fund, signed by W. H. Dall, stating that there was a cash income balance of $274.97 and that owing to conditions in Europe and the North Atlantic there was no recommendation and no report of progress to be made.

A report was received from the Committee on Biographical Memoirs, signed by C. B. Davenport, G. C. Comstock, A. G. Mayer, and A. L. Day, stating that each biographer had been circularized concerning the progress of the memoir in his charge. The biography of William Stimpson by A. G. Mayer is completed and that of F. A. P. Barnard is nearly ready. Biographies of all members who died more than fifteen years ago are thus accounted for. A circular letter was sent to all members of the Academy suggesting that each prepare at the end of each year an autobiographical record for the preceding year; a number of members have utilized this suggestion; and some have deposited biographical material with the Committee.

GENERAL BUSINESS

The Council reported the acceptance for the Academy of the gift of $8,000 made to the National Academy of Sciences by Miss Margaret Henderson Elliot in pursuance of her desire to carry out a testamentary provision in the will of her father, Daniel Giraud Elliot, which would have been operative under circumstances stated in the will and applied in accordance with the terms of the legacy as fully set forth in the will whereby a medal, to be known as the Daniel Giraud Elliot gold medal, together with an accompanying diploma and an unexpended balance of income for the year was to be awarded annually to the author of such paper, essay or other work upon some branch of zoology or paleontology published during the year as in the opinion of the judges, who shall be Henry Fairfield Osborn of New York, the Scientific Director of the American Museum of Natural History, and the Secretary of the Smithsonian Institution, shall be the most meritorious and worthy of honor.

Mr. William H. Welch presented the following letter, resigning the presidency of the Academy to take effect with the close of the present meeting, which was accepted with great regret:

National Academy of Sciences, April 16, 1917.

To the Members of the National Academy of Sciences.

GENTLEMEN: In accordance with the intention which I announced at our semi-annual meeting last November, I hereby tender my resignation of the office of President of the Academy to take effect at the close of the present annual meeting.

It is mainly the assumption of new and exacting duties and responsibilities connected with the organization and development of the School of Hygiene and Public Health recently established at the Johns Hopkins University which leads me to withdraw from a position which should receive much of the time and thought and energy of the incumbent.

It is a source of gratification that the Academy, while retaining its distinguished position as representative of those most eminent for their contributions to science in America, has widened in recent years its fields of activities and thereby become more useful and significant in the advancement of science in our country. It is to be hoped and expected that the Academy in its activities, influence and membership will be still further broadened and become increasingly serviceable and important in the promotion of knowledge and of the welfare of the nation.

I wish to renew the expression of my profound appreciation of the very high honor which was conferred upon me four years ago by election to the presidency of the Academy, as well as my appreciation of the continued loyalty and support and kindness of the members. I desire likewise to record my gratitude and indebtedness for the unfailing aid of the officers of the Academy, and especially of the Home Secretary and the Foreign Secretary, who have really carried the burden of the conduct of affairs.

Faithfully yours,

WILLIAM H. WELCH.

The invitation of the Provost and Trustees of the University of Pennsylvania, to hold the Autumn Meeting in Philadelphia, was accepted.

The following resolutions were adopted:

WHEREAS, there is a great shortage in the supplies of platinum available for scientific and industrial purposes, and

WHEREAS, the shortage is in very great measure due to the extensive use of platinum for jewelry and other articles of luxury, therefore be it

Resolved, that the National Academy of Sciences, in this critical situation where all the resources of the Nation should be put to the use of those immediately active in the constructive, productive and defensive work of the country, appeal to the women and the men of America to refuse to purchase or accept as gifts jewelry and other articles made in whole or in part of platinum and thus to allow all the available supplies of the metal to be used where they can do the greatest good.

The Foreign Secretary presented the following communication from the Académie des Sciences at Paris received through the Ambassador from France to the United States:

AMBASSADE
DE LA REPUBLIQUE FRANÇAISE
AUX ETATS-UNIS

Washington le April 16, 1917.

MY DEAR MR. SECRETARY:

I am informed by my Government that our "Académie des Sciences" has received, from "the National Academy of Sciences," a friendly telegram by which they have been deeply touched.

I am instructed to forward to you their answer, the text of which is here enclosed.
I have the honor to be, dear Mr. Secretary,

Sincerely yours, JUSSERAND.
(Signed)

Mr. Hale,
Foreign Secretary
National Academy of Sciences,
Washington, D. C.

National Academy of Sciences, Washington, D. C. Paris, 13 avril, 1917.

L'Académie des Sciences de l'Institut de France est heureuse de saluer l'aide que votre noble pays apporte au nôtre, et à ses alliés dans leur lutte pour le droit et la liberté.

Elle vous remercie de votre offre, et sera heureuse de collaborer avec l'Académie Nationale des Sciences pour la solution des problèmes scientifiques interessant la defense nationale et l'industrie.

A. LACROIX,
EMILE PICARD,
Secrétaires Perpétuels.

The Foreign Secretary presented the following communication from the Royal Society, London, received through the Ambassador from Great Britain to the United States.

DR. GEORGE E. HALE, *Washington, D. C.*

The Royal Society heartily welcomes the offer of the National Academy to co-operate in scientific researches connected with the war, and will communciate by letter proposals for carrying this into effect. THOMSON, *President.*

ELECTION OF OFFICERS AND MEMBERS

President.—Mr. CHARLES DOOLITTLE WALCOTT was unanimously elected President for a term of six years to expire in 1923.

Vice-President.—Mr. A. A. MICHELSON was unanimously elected Vice-President for a term of six years to expire in 1923.

Treasurer.—Mr. WHITMAN CROSS was unanimously elected to succeed himself for a term of six years to expire in 1923.

Two Members of the Council.—Mr. A. A. NOYES and Mr. E. G. CONKLIN were elected to succeed themselves for terms of three years each to expire in 1920.

The following persons were elected as new members of the Academy.

HARVEY CUSHING, Surgeon, Harvard University, Cambridge, Mass.
ROBERT RIDGWAY, Ornithologist, United States National Museum, Washington, D. C.
WILLIS RODNEY WHITNEY, Chemist, General Electric Company, Schenectady, N. Y.
LIBERTY HYDE BAILEY, Horticulturalist, Cornell University, Ithaca, N. Y.
JOHN J. CARTY, Electrical Engineer, American Telegraph and Telephone Company, New York, N. Y.
WALTER SYDNEY ADAMS, Astronomer, Mount Wilson Solar Observatory of Carnegie Institution, Pasadena, Cal.
HENRY MARION HOWE, Metallurgist, Professor Emeritus, Columbia University, New York, N. Y.
EDWARD KASNER, Mathematician, Columbia University, New York, N. Y.
WALLACE C. SABINE, Physicist, Harvard University, Cambridge Mass.
EDWARD OSCAR ULRICH, Paleontologist, U. S. Geological Survey, Washington, D. C.
WILLIAM FREDERICK DURAND, Mechanical Engineer, Leland Stanford Junior University, Stanford University, Cal.

SAMUEL WESLEY STRATTON, Physicist, Bureau of Standards, Washington, D. C.
THEODORE LYMAN, Physicist, Harvard University, Cambridge, Mass.
EDWARD LEE THORNDIKE, Psychologist, Columbia University, New York, N. Y.
WILLIAM STEWART HALSTED, Surgeon, Johns Hopkins Medical School, Baltimore, Md.

SCIENTIFIC SESSIONS

Two public lectures on the WILLIAM ELLERY HALE Foundation were given on April 16 and 18 by EDWIN GRANT CONKLIN, of Princeton University, on Methods and Causes of Organic Evolution.

Four public scientific sessions were held on April 16 and 17 at which the following papers were presented:

J. P. IDDINGS and E. W. MORLEY: Report of progress in the study of igneous rocks from the East Indies and Islands of the South Pacific.

W. M. DAVIS: The Great Barrier Reef of Queensland, Australia (illustrated).

CHARLES D. WALCOTT: Searching for a doubtful geological zone in the Canadian Rockies (illustrated).

ARTHUR L. DAY: The rôle of the gases in volcanic activity (illustrated).

W. A. NOYES: A kinetic hypothesis to explain the function of electrons in the chemical combination of atoms.

A. A. MICHELSON (Draper Medalist, 1917): Some recent work in physics.

W. B. CANNON: Some considerations regarding the nature of thirst.

ERWIN F. SMITH: On Resemblances of Crown Gall to Cancer: A synopsis of work done in the United States Department of Agriculture.

R. J. ANDERSON and GRAHAM LUSK: The influence of diet upon the heat production during mechanical work in the dog.

JACQUES LOEB and J. H. NORTHROP: What determines the natural duration of life?

A. G. MAYER: Biographical memoir of William Stimpson. (By title.)

BENJAMIN BOSS: Biographical Memoir of Lewis Boss. (By title.)

SIMON FLEXNER: Mechanisms that defend the body from poliomyelitic infection, (a) external or extranervous, (b) internal or nervous.

T. S. GITHENS and S. J. MELTZER: The difference in the action of antipyretics according to the species of animals subjected to this action, the state of health of the animals, the height of their normal temperature and the substance employed.

CHARLES B. DAVENPORT: Heredity and juvenile promise of eminent naval men.

HENRY FAIRFIELD OSBORN: The causes of the evolution of proportions by mammals.

W. V. KING (introduced by L. O. Howard): Sporogony of malaria parasites. Photomicrographs of infected anopheles.

FRANK R. LILLIE: Sex-determination and sex-differentiation in mammals.

HERBERT J. SPINDEN (introduced by W. H. Holmes): Economic contributions of ancient America.

SYLVANUS G. MORLEY (introduced by J. W. Fewkes): The Naya hieroglyphic writing.

E. E. BARNARD: The star in Ophiuchus with great proper motion, total lunar eclipse of January 7, 1917; measures of the position of the nucleus of the great nebula of Andromeda (illustrated).

EDWIN B. FROST: Recent remarkable changes in a nebula (illustrated).

THE WORK OF THE NATIONAL RESEARCH COUNCIL, by GEORGE E. HALE, Chairman, National Research Council; CHARLES D. WALCOTT, Chairman, Military Committee; ROBERT ANDREWS MILLIKAN, Chairman, Physics Committee; MARSTON TAYLOR BOGERT, Chairman, Chemistry Committee; VICTOR CLARENCE VAUGHAN, Chairman, Committee on Medicine and Hygiene; followed by Discussion.

AWARD OF MEDALS

The following medals were awarded at the annual dinner on the evening of April 17, 1917, at the Raleigh Hotel:

The Henry Draper Medal, to A. A. Michelson of the University of Chicago for his numerous and important contributions to spectroscopy and astronomical physics.

The Medal for Eminence in the Application of Science to the Public Welfare, to Samuel Wesley Stratton for his services in introducing standards into the practice of technologists in the United States.

RESEARCH GRANTS FROM THE TRUST FUNDS OF THE ACADEMY

During the twelve months preceding the Annual Meeting of the Academy the following grants for the promotion of research, in addition to those listed in these PROCEEDINGS, **2**, 743, were made from the Trust funds of the Academy.

GRANTS FROM THE BACHE FUND

No. 202, W. C. ALLEE, Lake Forest College, Ill., $415. For an investigation of a possible correlation between the rate of carbon dioxide production and reversal in light reaction of certain Arthropods, especially May-fly nymphs.

No. 203, JOSEPH P. IDDINGS, Brinklow, Md., $500. For a continuance of the microscopical and chemical investigation of rocks collected by Mr. Iddings in Japan, Philippine Islands, Java, Borneo, Celebes, Sumatra, New Zealand, Tahiti, and the Leeward Islands, and the Marquesas.

No. 204, IRVING W. BAILEY, Bussey Institution, Harvard University, $300. To secure field data in regard to the effects of various tropical environments upon the growth forms of the Angiosperms and to secure material for a study of the effects of these environments upon the anatomical structure of the Angiosperms. (Expedition to Guatemala has been delayed.)

GRANTS FROM THE WATSON FUND

No. 14, JOHN A. MILLER, Sproul Observatory, Swarthmore College, $500. To measure plates for determining stellar parallaxes. (Supplementary to Grant No. 13.)

No. 15, HERBERT C. WILSON, Goodsell Observatory, $325. To determine the position and brightness of asteroids (chiefly those discovered by Watson) by the photographic method, together with a study of the brightness of some variable stars.

GRANTS FROM THE WOLCOTT GIBBS FUND

No. 7, W. D. HARKINS, University of Chicago, $200. For the purchase of an air-pump which will be of use in five researches either now in progress or to be undertaken in the near future.

No. 8, H. L. DATTA, Presidency College, Calcutta, India, £25. To provide organic substances for his researches of chlorination.

GRANT FROM THE J. LAWRENCE SMITH FUND

No. 8, S. A. MITCHELL, University of Virginia, $400. To continue his researches on the paths, radiants, and orbits of meteors. (Supplementary to Grant No. 6.)

VOLUME 3 JUNE, 1917 NUMBER 6

PROCEEDINGS

OF THE

NATIONAL ACADEMY OF SCIENCES

OF THE

UNITED STATES OF AMERICA

Publication Office: Williams & Wilkins Company, Baltimore
Editorial Office: Massachusetts Institute of Technology, Cambridge
Home Office of the Academy: Washington, D. C.

Entered as second-class matter at the Post Office at Baltimore, Md., under the Act of August 24, 1912

INFORMATION TO CONTRIBUTORS

THE PROCEEDINGS is the official organ of the Academy tor the publication of brief accounts of important current researches of members of the Academy and of other American investigators, and for reports on the meetings and other activities of the Academy. Publication in the Proceedings will supplement that in journals devoted to the special branches of science. The Proceedings will aim especially to secure prompt publication of original announcements of discoveries and wide circulation of the results of American research among investigators in other countries and in all branches of science.

ARTICLES should be brief, not to exceed 2500 words or 6 printed pages. although under certain conditions longer articles may be published, Technical details of the work and long tables of data should be reserved for publication in special journals. But authors should be precise in making clear the new results and should give some record of the methods and data upon which they are based. The viewpoint should be comprehensive in giving the relation of the paper to previous publications of the author or of others and in exhibiting where practicable, the significance of the work for other branches of science.

MANUSCRIPTS should be prepared with a current number o. the Proceedings as a model in matters of form, and should be typewritten in duplicate with double spacing, the author retaining one copy. Illustrations should be confined to text-figures of simple character, though more elaborate illustrations may be allowed in special instances to authors willing to pay for their preparation and insertion. Particular attention should be given to arranging tabular matter in a simple and concise manner.

REFERENCES to literature, numbered consecutively, will be placed at the end of the article and short footnotes should be avoided. It is suggested that references to periodicals be furnished in some detail and in general in accordance with the standard adopted for the Subject Catalogue of the International Catalogue of Scientific Literature, viz., name of author, with initials following (ordinarily omitting title of paper), abbreviated name of Journal, with place of publication, series (if any), volume, year, inclusive pages. For example: Montgomery, T. H., *J. Morph., Boston,* 22, 1911, (731–815); or, Wheeler, W. M., *Königsburg, Schr. physik. Ges.*, 55, 1914, (1-142).

PAPERS by members of the Academy may be sent to Edwin Bidwell Wilson, Managing Editor, Mass. Institute of Technology, Cambridge, Mass. Papers by non-members should be submitted through some member.

PROOF will be sent to authors only on their request, in order to avoid delay in publication. Authors are therefore requested to make final revisions on the typewritten manuscripts. The editors cannot undertake to do more than correct obvious minor errors.

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PROCEEDINGS
OF THE
NATIONAL ACADEMY OF SCIENCES

Volume 3 JUNE 15, 1917 Number 6

THE STARK EFFECT IN HELIUM AND NEON

By Harry Nyquist

SLOANE LABORATORY, YALE UNIVERSITY

Communicated by H. A. Bumstead, April 20, 1917

Stark[1] and Koch[2] have studied the effect of an electric field on the spectral lines of helium by a method which was designed by Stark. Brunetti[3] and Evans and Croxson[4] nave studied the effect in the same substance by a method devised by Lo Surdo.[5] The results obtained by the two methods agree in the main, but hitherto the method of Stark has proved capable of greater refinement than that of Lo Surdo.

In the present investigation a method has been employed which is essentially a modification of Lo Surdo's but avoids the greatest disadvantages of that method. Lo Surdo's discharge tube is about 3 mm. in diameter and has a cylindrical cathode nearly filling it. The positive rays in the space before the cathode are in a strong field and are suitable for the study of the effect. The disadvantages of the method are, first, that the walls of the tube are quickly covered with a metallic film due to sputtering and, second, the tube breaks easily on account of the heating. In the present apparatus these difficulties are overcome by having within the discharge tube proper an inner tube made of aluminum through which the discharge takes place, and which has a slit through which the light passes. Opposite this slit there is a side tube having a window at its end. The aluminum is not seriously affected by the heat and the trouble from sputtering is eliminated.

The optical apparatus consists of a six prism spectrograph and a double image prism so arranged that photographs of the parallel and perpendicular components of the lines can be obtained simultaneously.

The source of current is a 13,200 volt transformer, the current from which is rectified by two kenotrons and a suitable arrangement of condensers and inductances.

The following is a brief summary of the results obtained in helium and neon:

1. The equation connecting the displacement of a component and the electric field is of the form

$$\delta\lambda = a + bE$$

where a differs from 0 for many components.

2. When $a = 0$, b is positive. Only two exceptions have been found to this rule, viz., He 4686 and He 3965.

3. There are simple numerical relations connecting the values of a for the different components of a given line. Such numerical relations exist to a less degree between the values of b.

4. The effect in the line He 4686 agrees qualitatively with that predicted by Evans and Croxson on Epstein's theory. The numerical

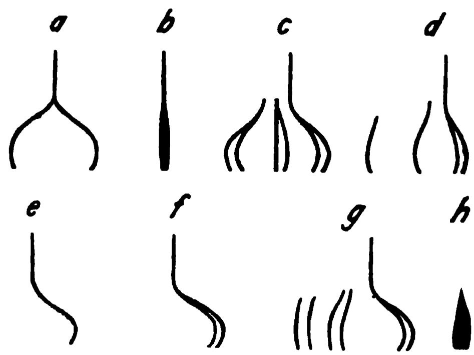


FIG. 1.

value of the ratio of the distance between the outer components of H_3 to that of He 4686, however, was found to be $\frac{24}{3^2 - 2^2}$ instead of $\frac{24}{4^2 - 3^2}$ as predicted by them.

5. Two new lines of the same type as those discovered by Koch were observed in helium. In neon, 34 such lines were observed.

6. In the accompanying figure are shown some illustrations of the lines as they occur on the photographic plate. In these drawings the wave-length increases from left to right. The upper part of the illustrations shows the line as it appears when the field is zero, the lower part shows the effect of the field.

In the modified discharge tube used, the maximum value of the field occurs some distance above the cathode, instead of in its immediate vicinity. This causes the displaced lines to have the form shown instead of the simple Y shape given by Lo Surdo's tube.

The pair of symmetrical components illustrated at (*a*) is typical of the Balmer series of hydrogen. The separation of such components has been taken as a measure of the field, the constant obtained by Stark being used in each case to calculate the field intensity. The form illustrated in (*b*) is typical of lines which show no appreciable Stark effect. While it is distinctly broadened in its lower portion, there is no doubt that the greater part of that broadening is due to increased intensity in the stronger field. The illustration (*c*) represents those components of He 4388 whose electric vector is perpendicular to the field. It shows, in order from left to right, two components having $a < 0$ and $b < 0$, one component having $a < 0$ and $b = 0$, one having $a < 0$ and $b > 0$, and finally two having $a = 0$ and $b > 0$. The line sketched at (*d*) is He 4922. The type (*e*) is very common in neon as is also type (*f*). A few lines in neon are of the general type illustrated at (*g*). At (*h*) is shown the appearance of a typical new line. It is very broad and intense in the field but does not exist where the field is zero, or at least is so faint that it produces no effect on the photographic plate.

A detailed statement of the results together with a full description of the apparatus and method will be published shortly.

[1] Stark, J., *Elektrische Spektralanalyse chemischer Atome*, Hirzel, Leipzig, 1914; *Ann. Physik, Leipzig*, **48**, 1915, (193).

[2] Koch, J., *Ann. Physik, Leipzig*, **48**, 1915, (98), Cf. J. Stark, *Elektrische Spektralanalyse chemischer Atome*, 73.

[3] Brunetti, R., *Nuovo Cimento, Pisa*, 10, 1915, (34).

[4] Evans, E. J., and Croxson, C., *Phil. Mag., London*, 32, 1916, (327).

[5] Lo Surdo, A., *Roma Atti Acc. Nuovi Lincei*, **22**, 1913, (664); **23**, 1914, (82); *Physik. Zs., Leipzig*, **15**, 1914, (122).

NEW ANALYSES OF ECHINODERMS

By F. W. Clarke and R. M. Kamm

UNITED STATES GEOLOGICAL SURVEY, WASHINGTON[1]

Communicated April 18, 1917

In a recent publication of the United States Geological Survey[2] Clarke and Wheeler have reported 250 analyses of the shells and skeletons of marine invertebrates; analyses which were made in order to determine what each class of organisms contributes to the formation of marine limestones. In that investigation the echinoderms and alcyonarians were peculiarly interesting, not only because they were notably magnesian, but also because the proportion of magnesia in them was found to be related to temperature. The warm water forms were all relatively rich in magnesium carbonate, while the cold water forms were much

poorer. In specimens from high latitudes or from very deep water the proportion of magnesium carbonate ranged from 7 to 10%, while those from tropical waters contained from 11 to over 14%. Intermediate conditions of environment gave intermediate values.

This strange relation between composition and temperature naturally suggested to us the desirability of comparing a series of echinoderms from one definite locality; and an opportunity to do so fortunately presented itself. An expedition sent by the Carnegie Institution of Washington, to the island of Tobago in the British West Indies, made a large collection of just such material as we needed; and Dr. Hubert Lyman Clark, who had charge of the echinoderms, kindly furnished us with specimens. All of them were collected at Pigeon Point, in shallow water along shore, and at a temperature of about 28°C. That is, all the specimens came from exactly the same environment, and were, therefore, for our purpose, strictly comparable. The list of them is as follows:

1. *Mellita sexiesperforatus*, Leske. A sea urchin. Adult.
2. *Echinometra lucunter*, Linné. A sea urchin. Half grown.
3. *Tropiometra carinata*, Lamarck. A crinoid. Adult.
4. *Asterina minuta*, Gray. A starfish. Large adult. Weight 0.1009 gramme.
5. *Linckia guildingii*, Gray. A starfish. Young.
6. *Ophiocoma pumila*, Lutken. A brittle star. Adult.
7. *Ophiomyxa flaccida*, Say. A brittle star. Half grown.
8. *Ophiomyxa flaccida*. Adult.

The analyses, reduced to uniformity by rejection of organic matter and water, and recalculation to 100%, are as follows and represent inorganic skeletal matter only.

	1	2	3	4	5	6	7	8
SiO_2	0.15	0.13	0.54	0.70	0.24	0.47	0.66	0.16
$(Al,Fe)_2O_3$	0.39	0.37	0.51		0.26		0.85	0.70
$MgCO_3$	11.91	11.56	13.74	12.53	14.31	12.97	14.95	14.56
$CaCO_3$	85.02	83.87	83.13	86.77	83.42	84.44	79.37	81.02
$Ca_3P_2O_8$	trace	1.85	0.64	trace	trace	0.14	trace	trace
$CaSO_4$	2.53	2.22	1.44	?	1.77	1.98	4.17	3.56
	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00

In these analyses the silica and the sesquioxides are doubtless impurities, due to adherent sand or mud from which the specimens could not be wholly freed. Analysis 4, of *Asterina*, is incomplete; for the amount of material, only 0.1009 gram, was insufficient for thorough work.

In such a small specimen the unavoidable errors of manipulation become serious, for the reason that they are magnified in the calculations. In this instance even so small an error in weighing as 0.1 mgm. would amount to 0.3 of 1% in the final statement. The material for analysis 6 was also hardly adequate. These two analyses, however, are not worthless, for they show the high percentage of magnesium carbonate which characterizes the organisms from tropical waters; and so too do the others.

The two analyses of sea urchins show less magnesium carbonate than was found in the other echinoderms, and so help to confirm our suspicion that as a group these animals are poorer in magnesia than either the crinoids, the starfishes, or the brittle stars. In the previously published analyses of crinoid skeletons, the highest proportion of magnesium carbonate found was 13.37%, in a specimen from the Aru Islands. The one from Tobago is notably richer. As for the starfishes and brittle stars the maximum figure previously found for magnesium carbonate was 14.08% in a specimen from Culebra; a quantity which is exceeded by three analyses in our new series.

The relation between magnesium carbonate and temperature is strikingly illustrated by four new analyses of starfishes, which we append here for comparison with the Tobago series. They are as follows:

1. *Pontaster tenuispinus*, Verrill. *Albatross* Station No. 2095, between Cape Hatteras and Nantucket. Latitude, 39°.29′.00″ N. Longitude 70°.58′.40″ W. Depth of water, 2456 meters. At that depth the temperature must have been low; not far from 0°C.

2. *Plutonaster agassizii*, Verrill. Collected off Marthas Vineyard, Massachusetts. Depth of water, 584 meters. Bottom temperature 6.6°C.

3. *Odontaster hispidus* Verrill. Collected off Marthas Vineyard. Depth, 245 meters. Bottom temperature 11.1°C.

4. *Astropecten articulatus* Say. West Coast of Florida.

	1	2	3	4
SiO_2	0.31	0.35	0.62	0.24
$(Al,Fe)_2O_3$	0.45	0.18	0.12	0.23
$MgCO_3$	8.86	9.09	10.58	13.02
$CaCO_3$	89.34	89.18	87.16	85.08
$CaSO_4$	0.89	1.20	1.44	1.43
$Ca_3P_2O_8$	0.15		0.08	trace
	100.00	100.00	100.00	100.00

For these starfishes we are indebted to the United States National Museum. They form part of a much larger series carefully selected for us by Mr. Austin H. Clark. The other specimens of the series are yet to be analyzed. These four, however, fall into place with the Tobago echinoderms and with those previously published. The progressive enrichment in magnesia, following increase of temperature, is unmistakable.

[1] This paper is here published by permission of the Director of the United States Geological Survey.

[2] Clarke and Wheeler, *Washington, U. S. Geol. Surv., Profess. Paper*, No. 102, 1917.

ON UTILIZING THE FACTS OF JUVENILE PROMISE AND FAMILY HISTORY IN AWARDING NAVAL COMMISSIONS TO UNTRIED MEN

By C. B. Davenport

STATION FOR EXPERIMENTAL EVOLUTION, CARNEGIE INSTITUTION OF WASHINGTON

Read before the Academy, April 17, 1917

We are about to organize an army of 1,000,000 men and to add greatly to our navy. Within the next few months, over 20,000 commissions will be given, largely to men who have never seen service. It is of primary importance to the conduct of this war that they should be properly selected. In our civil war incompetence of officers was responsible for thousands of unnecessary deaths.

In the past, appointments have been made largely as political favors, a very bad method of selecting. As Mahan says: "In the stringent and awful emergencies of war, too much is at stake for easy tolerance." In this paper I suggest a new method of selection in its particular application to untried naval officers. This method is, in brief, the utilization, among other data, of the facts of juvenile promise and family history.

The basis of this paper is the study of the biographies of 30 naval officers, and genealogies and supplementary data relating to their families. It appears at once that naval officers are of different types—there are naval fighters (like Nelson, Farragut, Porter and Cushing), naval explorers (like Sir John Franklin, McClintock, and our own Wilkes), naval inventors (like Dahlgren), naval diplomats (like Hornby) and so on. Of the 30 officers 14 are clearly *fighters*, and these are utilized in this study.

The essential traits of successful fighting naval officers are (*1*) *Love of the sea*—perhaps an elementary instinct, but not yet fully studied: (*2*) *nomadism*, whose inheritance is known to be sex linked; (*3*) *hyperkinesis*, which is inherited as a dominant trait: i.e., does not skip a

Perry-Rodgers

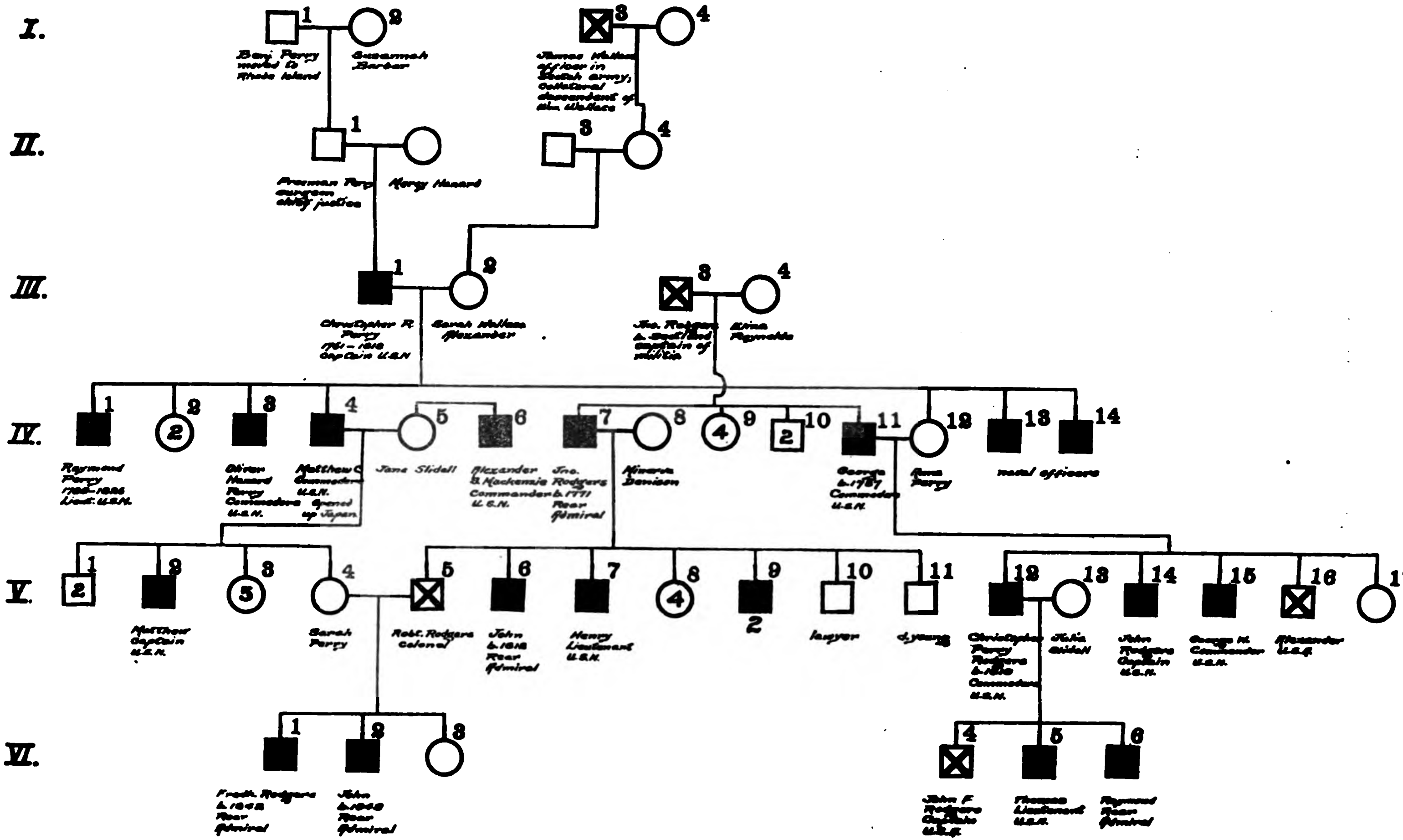


FIG. 1

Pedigree chart of The Perry-Rodgers complex of naval officers. Black symbols indicate men who were either naval officers or captains in the merchant marine. An × in the symbol means a military man. Generations numbered at the left hand margin.

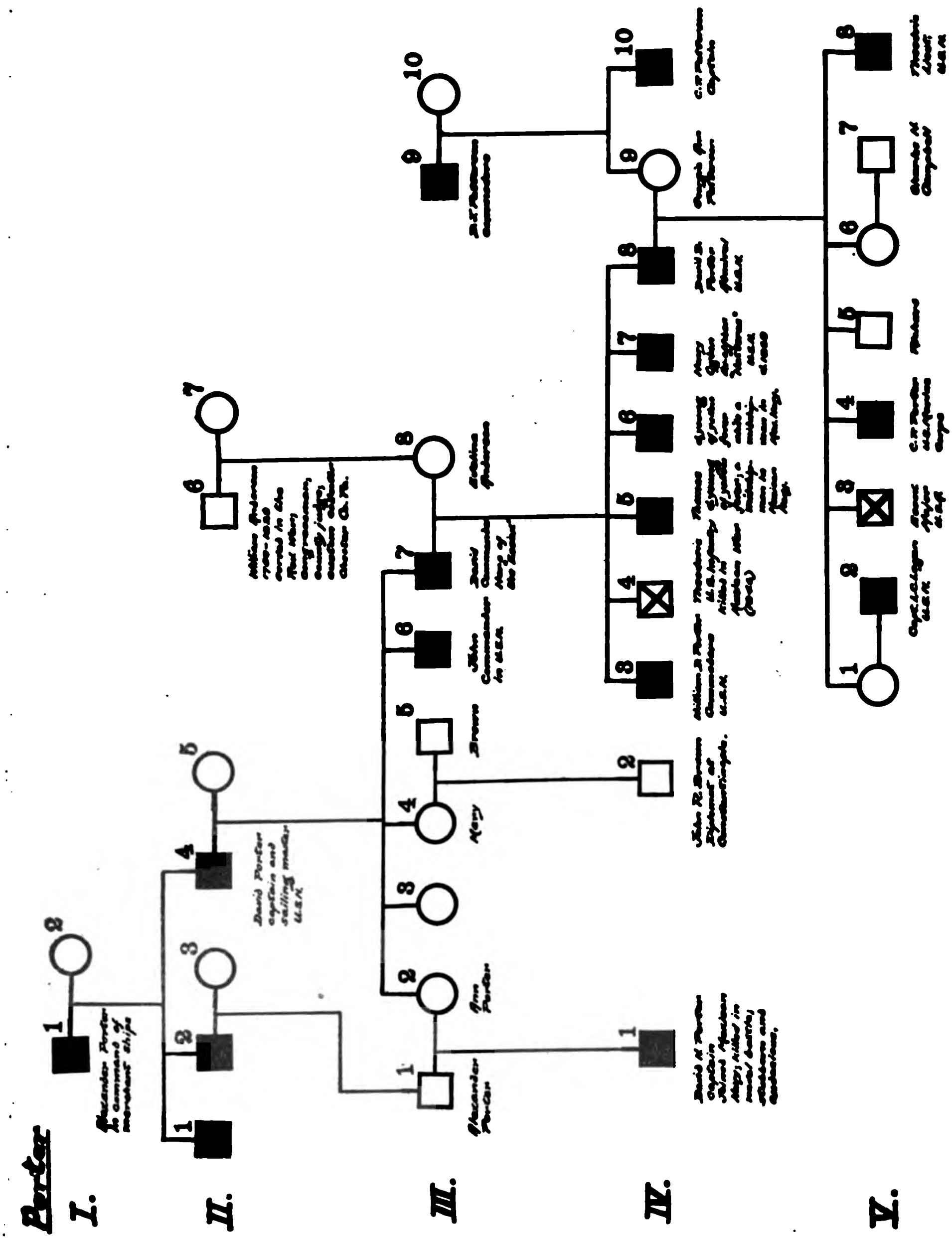


FIG. 2

Pedigree chart of the Porter family of naval officers. Symbols as in figure 1.

generation—it is indicated by energetic activity, push and marked emotional output; (4) *absence of fear*, such as permitted Perkins at 6 years to drive, tied in a sleigh, 10 miles and back to town to meet an emergency; or Maffitt at 10 years to travel alone with a ticket on his coat from North Carolina to White Plains, N. Y., in the days of stage coaches; or Winslow, at 10 years, to set to sea in a skiff, with a young cedar tree for a sail, from which he was rescued by an incoming vessel. Important also is (5) *ability to command men.*

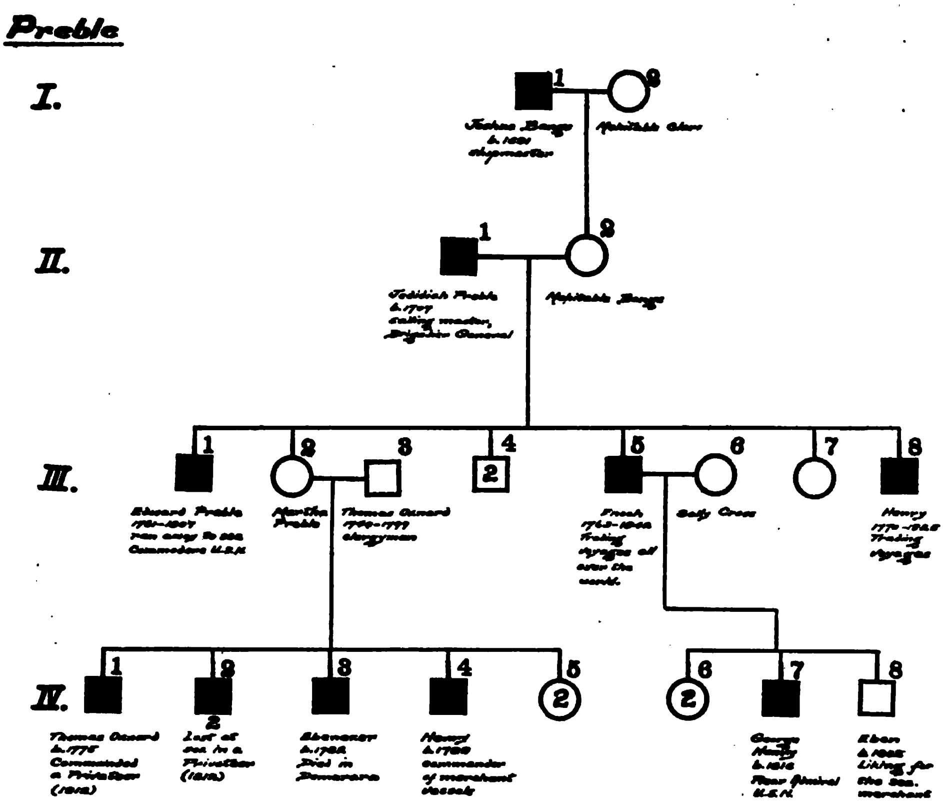


FIG. 3

Pedigree chart of the Preble family. Commodore Edward Preble, U. S. N., is at Generation III, No. 1.

An analysis of these 14 cases of naval fighters shows that all of those about whose youth data could be obtained (11) were adventurous, nomadic, fearless or "quarrelsome." Such then are the juvenile reactions of future successful naval fighters. In temperament 10 were hyperkinetics, 2 hypokinetic, 1 mixed and 1 calm. The hyperkinetic is the commonest reaction of successful naval fighters; but some of the great-

est naval men, like Nelson, have had the somber spirits and the thoroughness of the hypokinetic.

As to family history, there is a record of the *brothers* in 8 cases. In 5, one or more brothers were active in the army or navy, 1 had a nomadic brother; 2 had brothers not especially noteworthy. The *Father*, in 3 cases out of the 14 was a legal man, in 2 cases agricultural, in 2 medical; 1 each was a merchant, inventor, evangelist, 2 were temporarily in the army, 1 was a purser in the navy and 1 was a naval officer (Admiral Porter's father). *The Mother's Father*, is described in 8 cases. One was a naval officer; 1 was a sailor, 3 were in the army and 3 apparently stayed at home. A *Mother's brother* is described in 5 cases: 1 admiral, 1 sea captain, 1 seaman, 1 "dare-devil," and 1 in parliament.

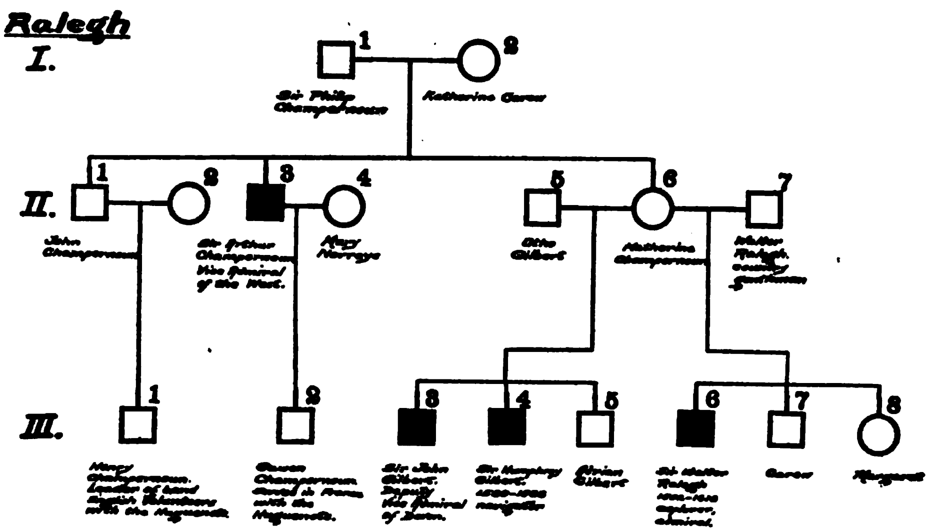


FIG. 4

Pedigree chart of naval officers in descendants, by two husbands, of Katherine Champernoun, the sister of Sir Arthur Champernoun; namely, Sir John Gilbert, Vice Admiral; Sir Humphrey Gilbert, the navigator; and Sir Walter Raleigh.

The conclusion from this study is first; the future fighting naval officer has, in childhood, a history of adventuresomeness and fearlessness. His *mother's* close male relatives (father, or brother, usually) show love of the sea, of travel and of adventure. Some of them are apt to be successful fighters. The hyperkinetic tendency shows in either father or mother. If the father is a naval man and the mother of naval or nomadic stock the chances of a combination for success of the son as a naval fighter are increased. The nomadic and adventurous traits of the great naval fighters are usually carried in the female fertilized eggs

only. Therefore the sons of the daughters, or sisters, of successful naval fighters are more apt to be successful naval men than are the sons or the sons' sons of fighters, unless (as often happens) these fighting fathers marry daughters or sisters of naval men.

Hyperkinesis in either parent gives a favorable prognosis for naval success in the son: but some of the very best naval fighters have been hypokinetics and, under modern conditions of naval warfare, this type is less handicapped than formerly.

Four typical family pedigrees are annexed.

List of the 14 naval officers utilized in this study:—

Bainbridge (U. S.), Barney (U. S.), Cushing (U. S.), Cochrane (Eng.), Paul-Jones (U. S.), Lawrence (U. S.), MacDonough (U. S.), Keppel (Eng.), Maffitt (U. S.), Morris (U. S.), Perkins (U. S.), Porter (U. S.), Battnall (U. S.), Wolsely (Eng.).

THE TRIPLET SERIES OF RADIUM

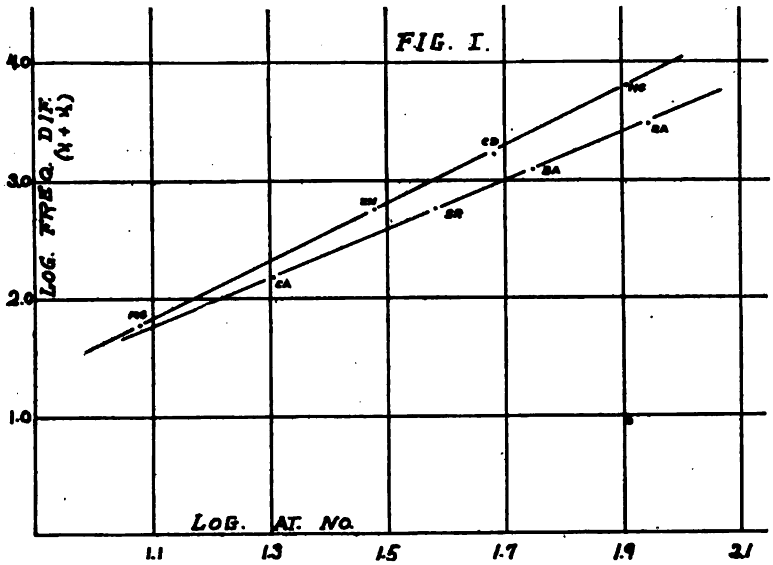
By Gladys A. Anslow and Janet T. Howell

DEPARTMENT OF PHYSICS, SMITH COLLEGE

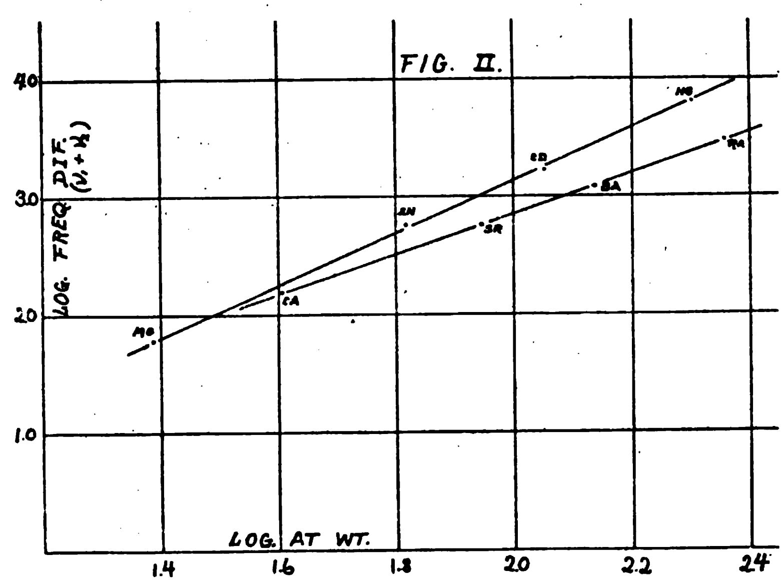
Communicated by G. E. Hale, May 2, 1917

Many attempts have been made to establish a relationship between the spectra of elements and their atomic weights. Referring to the doublets which occur in the first and second subordinate series, Runge and Precht[1] stated the following law. "In each group of chemically related elements the atomic weight varies as some power of the distance apart of a pair." They applied this law to the group of alkaline earths, and when they plotted the logarithm of the frequency difference of the pair series in each element against the logarithm of the atomic weight they obtained a straight line which gave by extrapolation an atomic weight of 257.8 for radium. As this was too large a value Ives and Stuhlmann[2] replotted the results of Runge and Precht using the atomic number in place of the atomic weight. Although their results were in general more consistent the atomic number found for radium was 96 instead of 88.

Now the elements of the second group of the periodic table are characterized by the presence of both doublet and triplet series of the principal, first, and second subordinate types. On plotting the logarithms of the atomic numbers against the logarithms of the frequency differences between the extreme members of the triplets we found that alternate elements fall on straight lines as in figure 1. Therefore the triplets of radium should fall on a line with calcium, strontium, and barium. The



line through these three elements showed that the frequency difference of radium triplets should be approximately 3060. An examination of the known radium lines, measured by Runge and Precht,[3] showd a number of triplets with the average frequency differences, $\nu_1 = 2016.64$; $\nu_2 = 1036.15$; and $\nu_1 + \nu_2 = 3052.79$. Two triplets showed well marked satellites and seemed to belong definitely to the first subordinate series. Although a study of the magnetic resolution of these lines will be neces-



sary to establish the triplets, there seems little reason to doubt that the frequency difference 3053 is characteristic of the radium spectrum.

In figure 2 the logarithms of the frequency differences are plotted against the logarithms of the atomic weights. The general result is the same, but less accurate than in figure 1. The points plotted for radium in figures 1 and 2 are the points found with the actual values for the atomic number and the atomic weight. They fall almost exactly on the lines through calcium, strontium, and barium, in the one case above the line, in the other case below. Extrapolating from the curve we find values of 87 for the atomic number and of 231.7 for the atomic weight, which are remarkably close to the true values, 88 and 226.

The triplets as identified so far are given in table 1. H indicates principal series; I N first subordinate; and II N second subordinate.

TABLE 1

	λ IN A. U.	INT.	$\frac{10^8}{\lambda}$	FREQUENCY DIFFERENCE		λ IN A. U.	INT.	$\frac{10^8}{\lambda}$	FREQUENCY DIFFERENCE
H III 1	6200.6	10	16127.47		I N II 3	5081.2	6	19680.39	2015.07
				1043.15				129.04	
H II 1	5823.9	5	17170.62		I N II 3	5048.1	1	19809.43	2017.74
				2038.13				93.05	
H I 1	5205.96	6	19208.75		I N II 3	5024.5	1	19902.48	2017.77
					I N III 3	4826.12	20	20720.58	1040.19
II N I 3	5958.4	10	16783.03					99.31	
				2015.06	I N III 3	(4803.1)	2	20819.89	1010.46
II N II 3	6319.69	6	18798.09						
				1037.2	I N I 4	4882.3	2	20482.15	
II N III 3	5041.52	6	19835.29					95.35	
					I N I 4	4859.7	1	20577.40	
II N I 4	4997.4	1	20010.41					94.05	
II N II 4				3060.20	I N I 4	4837.59	2	20671.45	
II N III 4	4334.5	5	23070.71						
					I N II 4	4444.4	2	22500.22	2018.07
I N I 3	5660.81	10	17665.32					93.54	
			126.37		I N II 4	4426.0	4	22593.76	2016.36
I N I 3	5620.6	3	17791.69						
			92.92		I N III 4	4245.4	2	23554.91	1054.69
I N 1 3	5591.4	1	17884.61						

In addition to the series triplets in table 1 there are three pairs with a frequency difference of 1035.34 and one of 2017.57 as shown in table 2. There is one weak triplet at λλ 6487.4, 5729.2, and 5409.16 with $\nu_1 + \nu_2 = 3071.16$, which does not fit into any series. A stronger pair at λλ 4903.2 and 4265.1 with a frequency difference of 3051.27 may possibly be a part of the I N 4 group.

TABLE 2

λ IN A. U.	INT.	$\frac{10^8}{\lambda}$	FREQUENCY DIFFERENCE	λ IN A. U.	INT.	$\frac{10^8}{\lambda}$	FREQUENCY DIFFERENCE
6167.4	8	16214.29	1038.41	4366.5	4	22901.64	1033.25
5796.2	5	17252.70		4178.0	6	23934.89	
5811.7	3	17206.67	1034.35	5907.4	2	16927.92	2017.57
5482.15	6	18241.02		5278.3	1	18945.49	

A preliminary attempt at a Rydberg formula gives the following results for the first line of the three classes of triplets:

$$\text{Principal series,} \qquad n = 44349.0 - \frac{109675}{(m + 1.0855)^2}$$

$$\text{First subordinate series,} \qquad n = 25236.8 - \frac{109675}{(m + .8062)^2}$$

$$\text{Second subordinate series,} \qquad n = 25153.0 - \frac{109675}{(m + .6196)^2}$$

It is very probable that further investigation will modify these formulae, but the existence of the triplets seems well established, and the fact that alternate elements in the chemical table show a spectroscopic relation is suggestive for further work along this line.

[1] Runge, C., and Precht, J., *Phil. Mag.*, *London*, **5**, 1903, (476).
[2] Ives, H. E., and Stuhlmann, O., *Physic. Rev.*, *Ithaca*, *N. Y.*, (Ser. 2), **5**, 1915, (368).
[3] Kayser, H., *Handbuch der Spectroscopie*, vol. 6, p. 325.

THE MEASUREMENT OF SMALL ANGLES BY DISPLACEMENT INTERFEROMETRY

By Carl Barus

DEPARTMENT OF PHYSICS, BROWN UNIVERSITY[1]

Communicated April 21, 1917

Parallel rays retracing their path.—The following method was devised with a view to the micrometric measurement of angles. It is to be used below in connection with an electrometer for reading microvolts. An interference method of a different kind for measuring small angles was developed some time since and used at length in connection with the deviation of the horizontal pendulum.[2] Again the electrometer was treated in different ways[3] by the aid of the displacement interferometer. The present method, however, will differ from all of these. In figure 1, L is a horizontal beam of white light from a collimator. After passing

through the auxiliary clear plate P (to be used preliminarily for parallelizing the mirrors of the system in a way presently to be shown), the beam is reflected at a and b by the half silver plates H_1 and H_2 respectively to the wide opaque mirror m. The rays now retrace their paths or nearly so to be in turn transmitted at a and b by the half silvers H_1 and H_2. These transmitted pencils similarly impinge on the opaque mirror M and the half silver H_3 at c and d respectively, and pass thence (the ray from c being transmitted) into the telescope at T. The direct vision grating-prism g may be swivelled in place or removed, at pleasure.

To bring the system of four mirrors into complete parallelism is here of considerable importance if the spectrum fringes or the residual phenomenon are to be adequately large for measurement. The presence of the common mirror m, however suggests the procedure. When the clear plate P is in place, the rays ae and bf on returning are also again reflected at a and b toward L and may be clearly seen in a telescope[4]

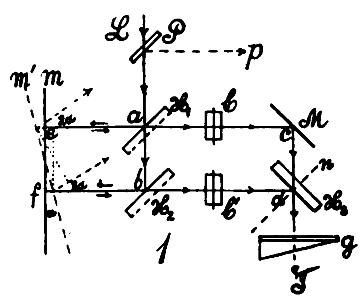


FIG. 1.

at p. Hence if m is the standard plane and nearly vertical, the mirrors H_1 and H_2 will be parallel when the slit images seen at p coincide horizontally and vertically, while H_1, H_2 and m will have their common normal plane in the diagram. In the same way the mirrors M and H_3 may be parallelized with their common normal plane in the diagram. If the distances ac and bd, ab and cd have previously been made nearly equal and the angles approximately 90°, the fringes will usually be found on moving the micrometer screw normal to H_3.

As the mirrors are thick glass plates it is preferable that the half silvered sides of H_1 and H_2 be toward L, and the half silvered side of H_3 toward M. In this case each ray passes the plates twice, as indicated in figure 1. With ordinary plate glass the fringes when found are still apt to be small. They are then to be enlarged and centered, by compensator of clear glass C and C', in the two rays respectively, rotated in opposite directions around a horizontal axis until the center of ellipses

is in the field of the spectroscope. It may be necessary to actuate the micrometer screw at *d* to complete the adjustment.

When the ellipses are centered, the direct vision spectroscope *g* removed, and the slit widened or removed, the residual or achromatic fringes appear in sight and are ready for use. These are always strong. The spectrum fringes are apt to be less so, since the parts of the ray *L* pass through two half silvered surfaces H_1H_2 or H_1H_3 in succession.[5] The spectrum fringes are sharp when the slit is fine. If the white residual fringes are too dazzling a single or two half silvers may be placed before the objective of the telescope with advantage. Two plates with their half silvered sides in contact and held so by a steel clip, are excellent for this purpose while they are at the same time protected from sulphur corrosion. This in fact is the best method of preserving silver mirrors (in pairs) when not in use.

When the spectra are in coincidence and the fringes sharp, the mirror *m* may be rotated around a vertical axis at *A* into some position, *m'*. In such a case the two spectra will move through the field of the telescope at *T*, but their coincidence will not be destroyed. The *D* lines, for instance, will continue to be superposed throughout. Considerable path difference is however introduced in this way and hence the fringes will march through the spectrum at an enormously more rapid rate. The following data may be given, where α is the angle of rotation of the mirror *m* and *N* the reading of the micrometer at H_3 (screw in the normal *dn*) necessary to bring the center of ellipses back to the sodium lines. In both cases the centers were out of the field (above or below), so that horizontal fringes were made the criterion for adjustment. This method is somewhat rough, but adequate for the present purposes.

1. Fine thin fringes. Relatively large differential glass path. Distance *ab*, figure 1, $2R = 21$ cm. Thickness of glass plates (half silvers) $e = .70$ cm.

$\alpha =$	0°	.05°	.20°	.30°	.40°	.50°	.60°
$N \times 10^3 =$	23	30	128	162	215	258	299 cm.

This is curve *a* in figure 2. From it the mean rate

$$\frac{\Delta N}{\Delta \alpha} = .47 \text{ cm/degree, or } 27 \text{ cm/radian}$$

may be found.

2. Coarse large fringes. Smaller differential glass path.

$\alpha =$	0°	.1°	.2°	.3°	.4°	.5°	.6°	.7°	.8°	.9°	1.0°
$N \times 10^3 =$	−25	+29	84	134	176	217	265	323	365	420	467 cm.

This is the curve given (with double ordinates for distinction), in curve b, figure 2, and in figure 3. Besides this the datum $\alpha = -.6°$, $N = -.320$ cm was obtained. In figure 3 the mean rate is

$$\frac{\Delta N}{\Delta \alpha} = .465 \text{ cm/degree, or } 26.6 \text{ cm/radian,}$$

agreeing with the preceding as closely as may be expected. We may thus estimate $\Delta N = 27 \times 10^{-6}$ of displacement at the micrometer at H_3 per micro-radian of turn α at the mirror m, which amounts to a little less than one interference ring per microradian (about one-fifth second of arc) of turn. The theory will be given later. With regard to the application to the electrometer we may come to the following

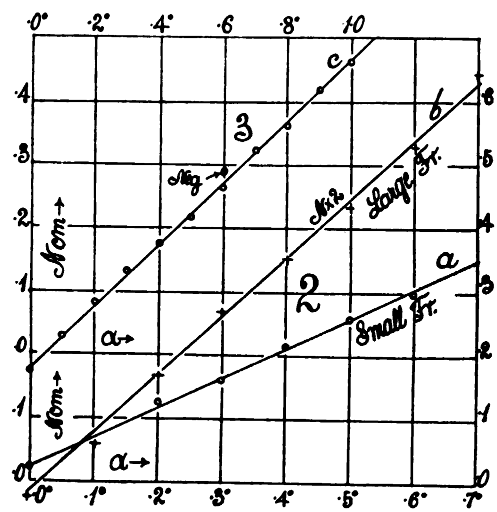


FIG. 2 AND 3.

conclusion. A good instrument of the quadrant or similar type should give about a radian of deflection per volt, or a microradian per micro-volt. In the present interferometer the microradian is about equivalent to the passage of one interference fringe. Hence one fringe per microvolt is about the order of sensitiveness to be expected.

[1] This contribution is a note from a Report to the Carnegie Institution of Washington.

[2] *Washington, Carnegie Inst., Pub.*, No. 229, 1915, (¶ 10 et seq.)

[3] *Ibid.*, (¶ 67 et seq.)

[4] The return rays may also be projected in a screen near the objective of the properly focussed collimator and the sharp images put in contact.

[5] If α is the refraction of light transmitted and $1 - \alpha$ reflected, the fraction of the original light, L, reaching the telescope T will be $2\alpha^2(1 - \alpha)^2$. This is a maximum if $\alpha = \frac{1}{2}$. Thus the illumination is reduced to $\frac{1}{8}$.

MECHANISMS THAT DEFEND THE BODY FROM POLIOMYELITIC INFECTION, (a) EXTERNAL OR EXTRA-NERVOUS, (b) INTERNAL OR NERVOUS

By Simon Flexner

ROCKEFELLER INSTITUTE FOR MEDICAL RESEARCH

Read before the Academy April 17, 1917

I am led to report the results of recent experiments looking toward the solution of the problem of susceptibility in epidemic poliomyelitis, by reason of the intrinsic interest of the subject and because of certain advances in knowledge which have been made recently.

The mass of the population appears insusceptible to the disease. That is to say even under conditions in which poliomyelitis is severely epidemic the real incidence is low. Thus the incidence during the past summer and autumn in Greater New York City, in which more than 9000 cases of the disease were recognized, was 1.59 per 1000 of the population.

Our present knowledge indicates that during epidemics, the microbic cause or virus of the disease is very widespread. This virus leaves and enters the body by way of the nasal and buccal mucous membranes. It is present not only in paralyzed persons but equally in the greater number of the affected who are often only slightly ill and do not develop any paralysis whatever, and in an undetermined number of healthy persons who have been in intimate contact with both classes of patients mentioned.

In view of the wide distribution of the virus and the relatively low case incidence, we must suppose that many more persons are exposed to than acquire the infection. Hence the body must possess defensive mechanisms usually sufficing to protect it from invasion.

Two sets of defences have been detected. The first or external consists of the secretions of the nasal and probably pharyngeal mucous membranes. Their action has been especially studied by Amoss and Taylor.[1] The secretions in many if not in most persons when left in contact for a relatively short time with the virus of poliomyelitis, inactivate or neutralize it. This test is readily made because monkeys are highly susceptible to inoculation with the virus. When an active virus has been mixed with the bacteria-free nasal secretions obtained by filtration through porcelain, it is no longer active for monkeys.

Some persons fail to yield this neutralizing nasal secretion; in others, a temporary, pathological state of the mucous membranes removes the inactivating property previously present. The number of tests is still

too small to determine whether young persons who are the more susceptible yield secretions which are numerically inferior in neutralizing power to those supplied by older persons.

Probably the failure of this external defensive measure is not in itself decisive, because of the existence of the second or inner defensive mechanism. It consists of the membranes about the brain and spinal cord and attached secreting organ of the choroid plexus. This meningeal-choroid complex is remarkably efficient in excluding from the cerebrospinal fluid, and hence from the substance of the brain and spinal cord, almost everything present in the circulating blood, except water and a few inorganic salts. The fluid is also almost free of cells. Only when the complex has been injured in some way and its integrity impaired does it permit even protein and cells to pass through from the blood into the cerebrospinal fluid.

The fact had previously been determined,[2] that the virus of poliomyelitis passes with great difficulty from the blood into the nervous organs, unless the choroid plexus and meninges had previously been injured, say through setting up an aseptic inflammation by injecting sterile horse serum into the meninges by means of lumbar puncture. What the present studies have brought out is the extraordinary sensitiveness of those structures to the injurious action of othewise bland fluids,[3] for not only is their permeability affected by irritants such as horse and even normal monkey serum, both of which produce visible signs of inflammation, but also by sterile physiological salt, Ringer's or Locke's solutions which set up only evanescent inflammatory changes, and of the cerebrospinal fluids from other monkeys which produce no detectable inflammatory changes whatever. The injury produced by the last fluid mentioned is so slight as possibly to be regarded in Cohnheim's sense as merely molecular.

Possibly poliomyelitis arises during the prevalence of the malady when both sets of defensive measures fail. This probably would occur only in exceptional instances in individuals among populations of any size. It is for the moment not difficult to conceive of reasons to account for the failure of the external mechanism, and more difficult to account for failure of the internal mechanism of defense. Not improbably the neutralizing power of the nasal secretions tends to reduce the carriage of the virus upon the nasal mucosa of persons exposed to and having suffered from infection with the virus of poliomeylitis. It becomes, therefore, an essential agency in diminishing public danger through reduction in the number of the potential virus carriers which arise.

There is one irritating fluid only so far detected which does not pro-

mote infection when injected into the cerebrospinal meninges. This fluid is an immune serum obtained from monkeys or human beings previously recovered from poliomyelitis. The immune serum carries neutralizing principles which inactivate the virus as it passes from the blood into the cerebrospinal fluid. This observation is in harmony with the curative action exercised by the serum, as was first shown some years ago in inoculated monkeys,[4] and has recently been confirmed for human cases of epidemic poliomeylitis.[5]

[1] Amoss, H. L., and Taylor, E., *J. Exp. Med.*, **25**, 1917, (507).
[2] Flexner, S., and Amoss, H. L., *Ibid.*, **19**, 1914, (411).
[3] Flexner, S., and Amoss, H. L., *Ibid.*, **25**, 1917, (525).
[4] Flexner, S., and Lewis, P. A., *J. Amer. Med. Assn.*, **54**, 1910, (1780); **55**, 1910, (662).
[5] Amoss, H. L., and Chesney, A. M., *J. Exp. Med.*, **25**, 1917, (581).

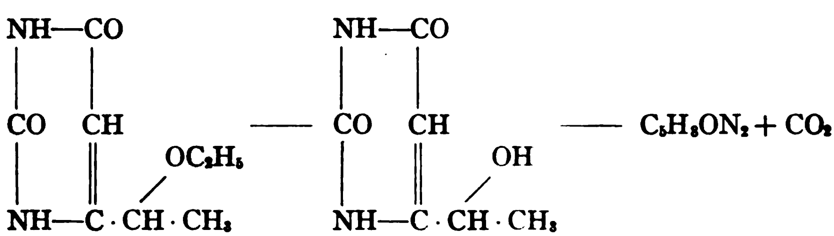
THE BEHAVIOR ON HYDROLYSIS OF THE SIMPLEST SECONDARY NUCLEOSIDE ON THYMINE

By Treat B. Johnson and Sidney E. Hadley

SHEFFIELD SCIENTIFIC SCHOOL, YALE UNIVERSITY

Communicated by L. B. Mendel, May 5, 1917

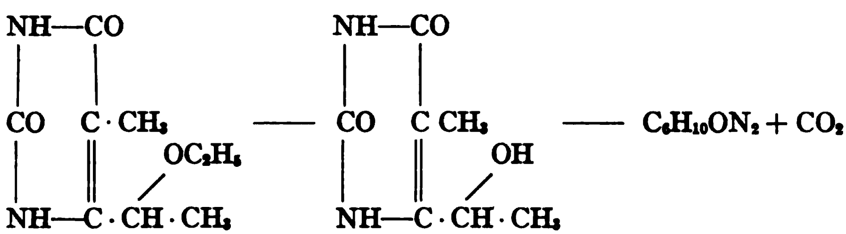
In a previous publication from the Sheffield Chemical Laboratory Johnson and Hadley[1] have shown that the ethyl ether of the secondary uracil-nucleoside I undergoes a very unique transformation when subjected to hydrolysis by heating with hydrobromic acid in aqueous solution. Ethyl bromide is first evolved with formation of the nucleoside II. This pyrimidine then undergoes a profound molecular change on prolonged heating and is transformed, with evolution of carbon dioxide, into a combination of unknown structure having the empirical formula $C_5H_8ON_2$.



I. II.

Great biochemical interest is attached to nucleoside transformations of this character, and we have now in progress an investigation planned to establish experimentally the structure of this interesting product of hydrolysis.

We have now continued our investigation on the hydrolysis of pyrimidine-nucleosides and have examined the behavior towards acids of the ethyl ether of the corresponding secondary nucleoside of thymine III. This pyrimidine has already been synthesized and a description of the properties of the compound is given in a dissertation presented by Dr. Hadley in 1916.[2] We now find that this pyrimidine III interacts in a manner perfectly analogous to that shown by its lower homologue I. When heated under pressure with an aqueous solution of hydrochloric acid it is transformed as expected into the nucleoside IV with evolution of ethyl chloride. On continued heating, however, the nucleoside IV gradually breaks down with evolution of carbon dioxide, and is changed into a compound of unknown structure having the formula $C_6H_{10}ON_2$. The transformation is expressed by the following equation.



III. IV.

In other words, the two hydrolytic products $C_5H_8ON_2$ and $C_6H_{10}ON_2$ are combinations of the same order and are representatives of an homologous series differing in constitution by a CH_2 radical.

The compound $C_6H_{10}ON_2$ derived from the nucleoside ether III has a melting or decomposition point of 270°. We have subjected the combination to a complete analysis and have obtained the following analytical results. Carbon 56.88%, hydrogen 8.10% and nitrogen 21.96%. The theoretical values for these three elements respectively in a compound having the formula $C_6H_{10}ON_2$ are carbon 57.1%, hydrogen 7.9% and nitrogen 22.2%.

Our observation that pyrimidines of the type represented by formulas II and IV readily loose one of their carbon atoms is unique in that the change can be brought about by simple hydrolysis and without the employment of an oxydizing agent. The mother compounds from which the nucleosides are derived viz.: uracil and thymine, can be heated with acids at high temperatures for hours without destruction of the pyrimidine ring. The final results of our investigation will be published in the *Journal of the American Chemical Society*.

[1] Johnson and Hadley, *J. Amer. Chem. Soc., Easton, Pa.*, 38, 1916, (1844).

[2] Hadley, Sidney E., *Dissertation*, Yale University, 1916.

THE OCCURRENCE OF HARMONICS IN THE INFRA-RED ABSORPTION SPECTRA OF DIATOMIC GASES

By James B. Brinsmade and Edwin C. Kemble

JEFFERSON PHYSICAL LABORATORY, HARVARD UNIVERSITY

Communicated by E. H. Hall, May 1, 1917

The study of the infra-red absorption bands of gases has within the last few years received a great impetus through Bjerrum's[1] explanation of their structure as the result of the superposition of the molecular rotations on the vibrational motion of the charged atoms, which is the primary cause of the absorption. Bjerrum's suggestion was quickly followed by the discovery of v. Bahr[2] that these bands are, in reality, clusters of comparatively narrow lines, corresponding to the discrete angular velocities predicted for the molecular rotations by the older form of the quantum theory. In a recent number of the *Physical Review*[3] Kemble has called attention to the fact that if the energy of the vibrational motion also takes on only the quantum values the corresponding infra-red absorption bands in the spectra of diatomic gases ought to be accompanied by faint harmonics. An examination of the literature showed only one case, viz., carbon monoxide, in which more than one band was known to exist and this was a faint one at very nearly half the wave length of the fundamental. The present article is a preliminary report on an experimental search for similar harmonics in the infra-red absorption spectra of other diatomic gases and an examination of their structure. First harmonics have been found in the spectra of hydrochloric acid and hydrobromic acid and their structure as well as that of the faint carbon monoxide band has been shown to be in general agreement with the theory. Search was also made for possible second and third harmonics in the spectra of carbon monoxide and hydrochloric acid, but without result.

Apparatus and Method of Procedure.—The work was done on a prism spectrometer with a combination Wadsworth-Littrow mounting similar to that described by Gorton.[4] This arrangement gives an unusually high resolving power by a double passage of the energy through the prism. The energy was measured with a thermopile. The prism was of quartz, the refractive indices given by Carvallo and Rubens[5] being used in computing the wavelength scale. The absorption tube was 78 cm. long and had a minimum inside diameter of 3.5 cm. The ends were closed with windows of fused quartz.

Each galvanometer deflection with the absorption tube in place was 'sandwiched' between two deflections with the tube drawn to one side.

In this manner a curve was obtained, showing the transmission of the tube relative to an air path of equal length. The absorption in the gas was computed by comparing the transmission curve for the tube when evacuated with that when filled with the gas under investigation. From two to six complete sets of observations were taken at each spectrometer setting.

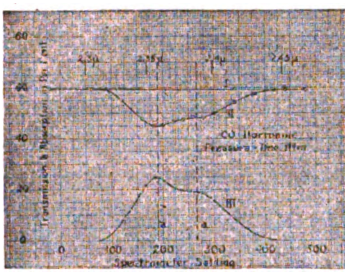


FIG. 1.

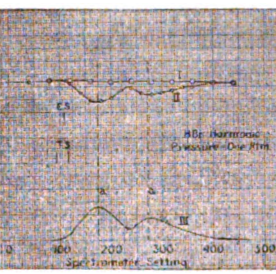


FIG.

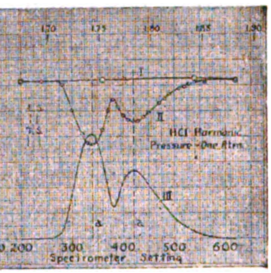


FIG. 3.

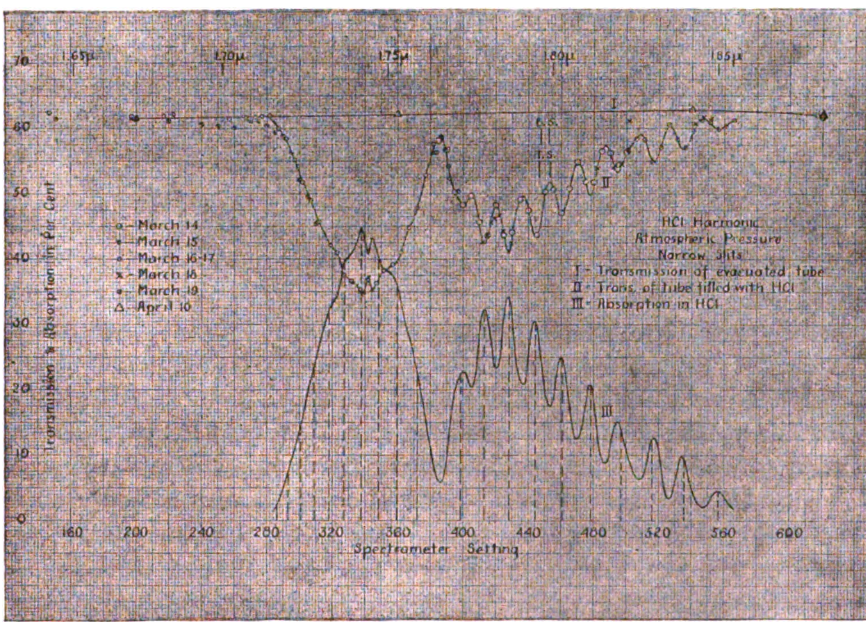


FIG. 4.

Carbon Monoxide.—According to Burmeister[6] the principal infra-red absorption band of CO is a doublet whose maxima are at 4.60 μ and 4.72 μ. The center of the harmonic should accordingly be at 2.33 μ. It should be a doublet with one fourth the wavelength difference between the maxima that is observed in the fundamental. Figure 1 shows the

absorption curve obtained. When this curve was taken the spectrometer was not properly adjusted to give its full resolving power. However, it is easy to see that the band is actually a doublet and that within the limits of error of our observations the separation of the maxima of the components is that predicted. The vertical lines *a-a* in the figure have the wavelength separation predicted for the maxima from Burmeister's observations on the fundamental. On the other hand, the wavelength of the center of the band is greater than it should be by 0.043 μ.

This band had previously been observed by Coblentz, Burmeister, and others, but had not been resolved into its two components.

Hydrobromic Acid.—Burmeister gives only one absorption band in the infra-red absorption spectrum of HBr, viz., a doublet with maxima, (corrected for slit-width by the Paschen-Runge formula) at 4.013 μ and 3.836 μ. The center of the harmonic should be at 1.962 μ and the separation of the maxima should be 0.0442 μ. Figure 2 shows the absorption curve obtained. The widths of the entrance slit and thermopile slit are indicated by two pairs of short vertical lines marked E. S. and T. S. respectively. The distance between the maxima is in satisfactory agreement with the theoretical distance (cf. vertical lines *a-a*). As in the case of CO the wavelength of the center of the band is greater than that expected, the error being 0.026 μ.

Hydrochloric Acid.—The infra-red absorption spectrum of HCl consists, according to Burmeister, of a single doublet with maxima (corrected for slit-width) at 3.394 μ and 3.557 μ. We have found an additional doublet (fig. 3) with maxima at 1.783 μ and 1.742 μ. As in the cases of the other gases examined the observed wavelength of the center of the band is somewhat greater than it should be if the band is a true harmonic of the one at 3.475 μ, but the separation of the maxima agrees with the theoretical value predicted from Burmeister's observations.

Figure 4 shows this same HCl band as observed with narrower slits. The quantum lines discovered by v. Bahr in the fundamental are resolved on the long wavelength side of the band but not on the short wavelength side. The reason for the asymmetry in the positions of the quantum lines will be discussed below.

Figure 5 shows the fundamental as observed when the tube was filled with a mixture of air and HCl, the latter having a partial pressure of 20.3 cm. The absolute magnitudes of the absorption shown by this curve have not been determined as accurately as we hope to determine them in the future, but the positions of the bands are probably to be relied on. The wavelength of the center of this band is less than that observed by Burmeister with a prism by 0.01 μ.

Discussion.—According to the theory previously referred to,[3] the infra-red absorption spectrum of a diatomic gas at moderate temperatures should consist of a relatively strong fundamental doublet accompanied by a faint harmonic. The latter band should also be a doublet and the wavelength difference of the components should be one fourth of the wavelength difference of the components of the fundamental. The theory applies to five gases, viz., CO, NO, HCl, HBr, and HI. Of these we have examined three, and in each case the prediction of the theory has been verified. The only discrepancy between theory and observation consists in a slight excess in the observed wavelengths of the 'harmonics' over their computed values, amounting to 0.043 μ, 0.026μ, and 0.025 μ, in the cases of CO, HBr, and HCl respectively.

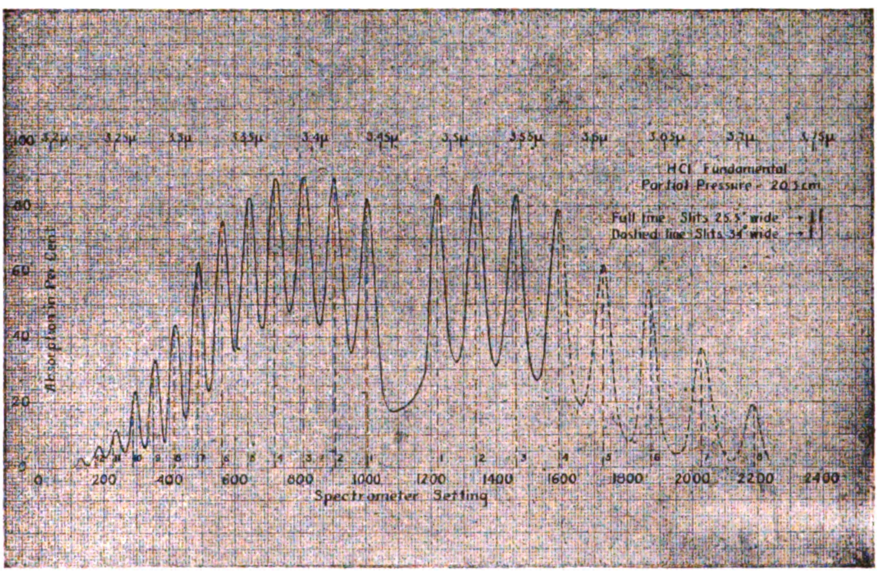


FIG.

This disagreement may be attributed to a slight error in the dispersion curves on which the wavelength measurements are based. As no absorption bands other than those here mentioned are known in the near infra-red spectra of the gases investigated, it seems out of the question that the occurrence of these weak bands at wavelengths so near the computed values can be a matter of chance. Moreover, as these diatomic molecules have but one vibrational degree of freedom, it would be difficult to explain the occurrence of two absorption bands in the infra-red spectra of these gases except as the result of the non-simple harmonic character of motion of the two atoms along their line of centers. That the faint bands are not due to impurities is proved by the spacing of the maxima

of the two components. We therefore look upon the small wave length discrepancy referred to as an indication of a slight error in the accepted values of the infra-red refractive indices of quartz.

The Asymmetry of the Bands.—According to the elementary theory of the structure of the infra-red absorption bands of diatomic gases given by Bjerrum[1], the curve obtained when absorption coefficients are plotted against frequency should be a symmetric doublet, of which each half should have the form of the distribution function for angular velocities. He assumes implicitly that the frequency of the atomic vibration is independent of the angular velocity of the molecule. The observed absorption bands are all asymmetric, however, the high frequency component being narrower and more intense. In an unpublished paper read before a recent meeting of the American Physical Society by Kemble[7], it was suggested that this asymmetry might be due to the fact that the frequency of the atomic vibration is lowered by the expansion of the molecule under the influence of the centrifugal force produced by the molecular rotation. This change of frequency would cause a crowding together of the quantum lines in the high frequency component and a corresponding increased separation in the low frequency component. On the basis of this theory the frequency of vibration should be

$$\nu = \nu_0 - a\nu_r^2$$

where a is a constant, ν_0 is the frequency of vibration for zero angular velocity, and ν_r is the frequency of rotation. Combining this assumption with the quantum theory we obtain the following formula for the positions of the two elementary lines corresponding to the pth unit of angular velocity.

$$\bar{\nu} = \nu_0 \pm p\nu_1 - ap^2\nu_1^2.$$

ν_1 is the unit of angular frequency. The dashed vertical lines on figure 4 indicate the theoretical positions of the quantum lines in the HCl harmonic as computed by the above formula, using the values 3.7×10^{-14} and 6.32×10^{11} for a and ν_1 respectively. The vertical lines in figure 5 give the corresponding positions for the fundamental, using the values 2.55×10^{-14} and 6.32×10^{11} for a and ν_1. In each case the agreement is satisfactory.

The unit of angular frequency, ν_1, is inversely proportional to the moment of inertia of the molecule and v. Bahr interpreted an apparent decrease in the value of ν_1, for the larger values of p which was observable on her absorption curve for the HCl fundamental, as an indication of an increase in the moment of inertia with increasing angular velocity. It will be observed that our observations show no perceptible variation in the value of ν_1.

One difficulty arises in connection with this theory of asymmetry. If the faint HCl band is a true harmonic of its strong companion the constant *a* should have twice the value for the former that it does for the latter. According to our observations the ratio of the two values of the constant is about 1.5 instead of 2. However, a very slight change in the assumed form of the dispersion curve in the neighborhood of 3.5 μ would suffice to bring the observations on the two bands into agreement. As a matter of fact the shape of the dispersion curve in this region is somewhat uncertain, for the points determined by Rubens are far apart and slightly irregular. The wavelength scale shown in figure 5 was obtained by drawing the best possible *smooth* curve through the points given by Rubens.

Kinetic Energy at the Absolute Zero.—Figures 4 and 5 emphasize an extremely important fact already evident in v. Bahr's absorption curve for the HCl fundamental, viz., the absence of a central band corresponding to a group of molecules having zero angular velocity. This observation forces the conclusion that none of the molecules are without rotational energy and brings us to a combination of the first form of Planck's quantum theory with the zero-point energy hypothesis which has hitherto been associated with the second form of his theory. Additional evidence in favor of this view including a discussion of the specific heat of hydrogen will be published shortly.

Conclusion.—The complete verification of a natural theory of the asymmetry of the infra-red absorption bands of diatomic gases and the accompanying explanation of the apparent increase in the moment of inertia of the molecule with increasing angular velocity clear up the outstanding difficulties in the theory of the structure of these bands. Consequently there can be no further doubt that it is correct to conclude from the discontinuities in their structure that the angular velocities are distributed among the molecules in the discontinuous manner predicted by the older form of the quantum theory. The proved existence of harmonics in the infra-red spectra of the same gases is almost equally good evidence that the vibrational energy of these molecules is distributed in the same manner.

[1] Bjerrum, N., *Nernst Festschrift*, Halle, 1912; *Berlin, Verh. D. physik. Ges.*, **16**, 1914, (640).

[2] v. Bahr, Eva, *Berlin, Verh. D. physik. Ges.*, **15**, 1913, (1150); *Phil. Mag.. London*, **28**, 1914, (71).

[3] Kemble, E. C., *Physic. Rev., Ithaca, N. Y.*, (Ser. 2), **8**, 1916, (701).

[4] Gorton, A. F., *Ibid.*, **7**, 1916, (66).

[5] Landolt and Börnstein, *Physikalisch-Chemische Tabellen*, Berlin, 1912, p. 972.

[6] Burmeister, W., *Berlin, Verh. Dh. pysik. Ges.*, **15**, 1913, (589).

[7] Kemble, E. C., The Infra-Red Absorption Bands of Gases and the Application of the Quantum Theory to Molecular Rotations. New York Meeting of Amer. Phys. Society, Dec. 28, 1916.

THE LOSS IN ENERGY OF WEHNELT CATHODES BY ELECTRON EMISSION

By W. Wilson

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Communicated by R. A. Millikan, May 9, 1917

It was first shown by O. W. Richardson in 1903 that the thermionic current from a hot cathode is given by the equation $i = a\theta^{\frac{1}{2}}e^{-b/\theta}$ where θ is the absolute temperature and a and b are constants.

The constant b has the special significance that it is proportional to the work done by an electron in leaving the surface of the body in question. This work can be determined directly by measuring the difference in power required to maintain a body at a certain temperature when it is emitting electrons from when it is not. Richardson and Cooke and later Lester have obtained values for tungsten which are in very good agreement with the values of b obtained by Langmuir and K. K. Smith.

On the other hand Wehnelt and Jentsch, Schneider, Wehnelt and Liebreich, and Richardson and Cooke have all found that for lime covered cathodes either the effect is so small in comparison with other energy changes as to be completely masked or that there is no correspondence between the two quantities.

Since these experiments suggest that the mechanism of thermionic emission from Wehnelt cathodes is different from that for pure metals further experiments were made by the author to determine whether consistent results could be obtained by using more stable cathodes.

If W is the work done by an electron in leaving the surface of a hot body $W = bR$, where R is the gas constant for one molecule.

If it is assumed that the work done by the electron is that done by moving through a double layer of strength ϕ we have $W = \phi e$, an d $\phi = bR/e$.

The method of Richardson and Cooke was used for the direct determination of ϕ. The constant b was determined in the usual manner, thermionic current being measured with the cathode at different temperatures which were obtained by means of an optical pyrometer of the Holborn and Kurlbaum type.

The following are the results obtained:

FILAMENT NUMBER	B DEGREES	$\frac{bR}{e}$ VOLTS	ϕ VOLTS
1	27,200	2.34	2.39
2	30,100	2.59	2.54
3	23,500	2.02	1.97
4	25,200	2.16	2.28
5	38,200	3.28	3.22
6	40,600	3.49	3.45

Filaments 1 and 2 were coated with BaO 50%, SrO 25%, CaO 25%, Filaments 3 and 4 with BaO 50%, SrO 50%, and Filaments 5 and 6 with CaO alone.

It appears that for Wehnelt cathodes the values ϕ and bR/e show a good correspondence. This is a strong point in favor of the view that the emission of electrons from Wehnelt cathodes is due to a similar mechanism to that causing the emission from heated pure metals. It is also a further proof of the substantial correctness of Richardson's hypotheses to account for the emission of electrons by hot bodies.

DAILY VARIATION OF WATER AND DRY MATTER IN THE LEAVES OF CORN AND THE SORGHUMS

By Edwin C. Miller

KANSAS AGRICULTURAL EXPERIMENT STATION

Communicated by R. Pearl, May 14, 1917

In connection with the study of the water relations of corn and the non-saccharine sorghums previously reported by me (*J. Agric. Res.*, 6, 1916), it was thought advisable to determine the daily variation of the water and dry matter in the leaves of these plants. A knowledge of the variation of the water in the leaves should throw some light on the relative ability of these plants to absorb water from the soil and to transport it to regions of loss from transpiration, while a study of the daily variation of dry matter in the leaves would permit a comparison of the relative power of the plants to manufacture food under different climatic conditions. The experiments herein reported were conducted during the summers of 1914, 1915, and 1916 at the State Branch Experiment Station at Garden City, Kans.

Cultural Methods.—The plants used in these experiments were Pride of Saline corn, Blackhull kafir and Dwarf milo. In 1914 and 1915, the plants were grown in alternate rows on the same plot, while in 1916 the experiments were made with plants grown on a series of one twentieth acre plots. The plants were grown in a sandy loam soil that had been

fall plowed and irrigated with approximately 8 inches of water. The crops were surface planted in rows 44 inches apart. After the plants were a few inches high the corn was thinned to a distance of 2 feet between the plants, Blackhull kafir to 1½ feet and the Dwarf milo to 1 foot. The plots were scraped with a hoe to keep the weeds down but no other cultivation was given during the growing season. The plots received no water after the fall irrigation, except that which came from the rainfall.

Leaf Sampling.—The amount of water and dry matter in the leaves of a given variety of plant was determined every two hours from 30 leaf samples, each with an area of 1 sq. cm. Thirty representative plants of each variety were selected and a leaf chosen on each plant for furnishing all the samples for an experiment extending over the desired length of time. At 7 a.m. a single sample was taken from each of the 30 selected leaves toward their tips. This was done by means of a Ganong leaf punch with an area of one square centimeter. At 9 a.m. a sample was taken from each of the 30 leaves directly opposite the sample taken at 7 a.m. At 11 a.m. the samples were collected from the leaves directly below those taken at 7 a.m. and then at the next two hour period directly opposite these and so on. The samples for a forty-hour experiment were thus obtained from a portion of the leaves representing less than 6 inches of their respective lengths. The leaf samples from corn, kafir and milo at any period could be collected in the manner described in from five to eight minutes. Care was taken in the selection of the leaves, so that they would be as nearly the same age as possible for the three plants. The uppermost, fully-developed leaf of each plant was the one from which the leaf samples were obtained.

The samples were collected in weighed vials which were then quickly stoppered. The vials containing the moist material were weighed at once on balances sensitive to one tenth milligram. They were then placed in a drying oven at 100–105°C. until all the moisture was driven off. After drying, the samples were cooled in a desiccator over sulphuric acid and weighed so that the amount of water and dry matter could be obtained.

Results.—Nine experiments were conducted in 1914, two in 1915 and four in 1916. Four of the experiments in 1914 extended only through the daylight hours but all of the other experiments ranged in length from twenty-four to forty hours. In all the amount of water and dry matter in the leaves was determined every two hours for twenty-two days and ten nights. The amount of water in the leaves at any period of the night could be accurately determined owing to the fact that dew is very

rare during the summer in this portion of the Great Plains. Corn, kafir, and milo were used in all the experiments with but two exceptions in 1916. In these two experiments only corn and milo were used. The amount of water in the leaves was determined in grams per square meter of leaf; percentage on a wet basis and dry basis for each two hour period of the experiment. The amount of dry matter was determined in grams per square meter of leaf and in percentage of the moist weight of the leaf.

The leaves of milo contained less water at all times than the leaves of either corn or kafir at the same stage of development. The average water content per square meter of leaf for all the observations made was 111.4 grams for milo, 123.2 grams for corn and 126.3 grams for kafir. The amount of water in the leaves of corn and kafir was practically the same at like stages of growth. The small difference between the average amount of water in the leaves of kafir as compared with those of corn, is due to the fact that in one experiment in 1914 and in two experiments in 1916, the leaves of the kafir were about ten days younger than those of the corn and, as a consequence, contained a greater amount of water.

Under the conditions of these experiments, the leaves of corn in most cases were wilted during the greater portion of the day. The first signs of wilting were most generally observed between 9 a.m. and 10 a.m. and in most cases no visible wilting could be observed after 4 p.m. The kafir leaves wilted during the day, but not to the extent that the leaves of the corn did while the milo leaves showed little or no signs of wilting. Under these conditions the average range between the maximum and minimum amount of water per square meter of leaf during the two hour periods from 7 a.m. to 7 p.m. was 13.8 grams for corn, 8.4 grams for kafir and 7.8 grams for milo.

Table 1 shows the average gain or loss of leaf water during each two hour period of the day from 7 a.m. to 5 p.m. for the experiments conducted in 1914, 1915 and 1916. The average gain or loss for each period is expressed in grams per square meter of leaf and in percentage based on the water in the leaf at the beginning of the two hour period.

A consideration of the loss of the leaf water during the day shows that from 7 a.m. to 9 a.m. the rate of loss was practically the same for corn and milo and the least for kafir. From 9 a.m. to 11 a.m. as the aerial conditions became more severe, the rate of loss increased for corn and kafir, but decreased almost one half for milo. During the next two hours the rate of loss decreased for all three plants, but the rate of loss was approximately twice as great for corn and kafir as for milo. From 1 p.m. to 3 p.m. the rate of loss continued to decrease, while the rate of loss of

TABLE 1

SUMMARY OF THE VARIATION OF THE WATER CONTENT OF THE LEAVES OF CORN KAFIR AND MILO DURING THE YEARS 1914, 1915 AND 1916 AT GARDEN CITY, KANSAS

TIME	PLANTS	LOSS			GAIN			NET GAIN OR LOSS	
		Number of cases of loss	Average loss of leaf water per square meter of leaf	Average per cent of loss based on leaf water at beginning of period	Number of cases of gain	Average gain of leaf water per square meter of leaf	Average per cent of loss based on leaf water at beginning of period	Per square meter of leaf	Per cent based on leaf water at beginning of period
			grams			grams		grams	
7 a.m.– 9 a.m.	Corn	21	4.1	3.5	0	0	0	4.1 loss	3.5 loss
	Kafir	15	3.3	2.8	1	3.8	3.2	3.0 loss	2.5 loss
	Milo	18	4.2	4.0	3	1.3	1.3	3.6 loss	3.4 loss
9 a.m.–11 a.m.	Corn	18	4.8	4.2	2	2.5	2.2	4.1 loss	3.8 loss
	Kafir	16	3.9	3.4	1	1.3	1.0	3.7 loss	3.4 loss
	Milo	20	2.2	2.1	1	5.2	5.6	1.9 loss	1.8 loss
11 a.m.– 1 p.m.	Corn	15	3.7	3.4	6	2.9	2.4	2.5 loss	2.2 loss
	Kafir	14	3.1	2.8	3	4.2	3.6	2.5 loss	2.2 loss
	Milo	13	1.9	1.9	7	1.7	1.6	1.1 loss	1.1 loss
1 p.m.– 3 p.m.	Corn	10	3.1	2.8	10	2.5	2.2	1.8 loss	1.6 loss
	Kafir	4	2.5	2.1	13	3.1	2.7	2.0 gain	1.7 gain
	Milo	11	3.3	3.3	10	2.6	2.5	1.7 loss	1.6 loss
3 p.m.– 5 p.m.	Corn	4	2.7	2.3	17	5.7	4.9	4.6 gain	4.0 gain
	Kafir	0	0	0	17	4.3	3.6	4.3 gain	3.6 gain
	Milo	4	4.1	4.2	17	3.9	3.7	3.2 gain	3.0 gain

milo increased over one per cent. These results seem to indicate that the absorption of water by the milo from the soil and its translocation to leaves, was proceeding more rapidly in proportion to the loss of water from the plant than in the case of either corn or kafir. The fact that the leaves of milo seldom wilted during the day also indicated that fact. The wilting of the leaves of milo in contrast to either the corn or kafir usually could not be observed until much later in the day. The increase in the rate of loss of leaf water in the milo from 1 p.m. to 3 p.m. would indicate, that the rate of absorption of water from the soil during that period, was less than the loss by evaporation from the leaves.

The amount of dry matter in the leaves of milo was at all times greater than in the leaves of corn or kafir of the same age. Taking the average weight of a square meter of corn leaf as 1, the average weight of an equal area of leaf would be 1.08 for kafir and 1.16 for milo. These differences in weight could be due either to the more compact arrangement

of the cells, or to a difference in the thickness of the leaves of the three plants or to both of these factors.

The average difference between the maximum and minimum amount of dry matter in the leaves during the day from 7 a.m. to 7 p.m. was 4 grams for corn, 4.8 grams for kafir and 8.0 grams for milo. Table 2 shows the rate of increase in dry matter for corn, kafir and milo in grams per square meter of leaf for each of the two hour periods during the day, from 7 a.m. to 5 p.m.

TABLE II

AVERAGE RATE OF INCREASE OF THE DRY MATTER FOR EACH SQUARE METER OF LEAF FOR CORN, KAFIR AND MILO DURING EACH TWO HOUR PERIOD OF THE DAY

PLANT	A. M.			P. M.	
	7–9	9–11	11–1	1–3	3–5
	grams	*grams*	*grams*	*grams*	*gram*
Corn	2.2	1.1	0.8	0.7	0.8
Kafir	1.7	1.2	0.7	1.2	0.7
Milo	1.3	1.5	2.2	2.0	0.8

Two explanations are possible for these results. The milo plant either manufactures food in the leaves more rapidly than the corn or kafir, or the rate of translocation is higher in the latter plants. In most cases, under the conditions of these experiments, the leaves of corn were badly wilted during the greater portion of the day. The kafir leaves also wilted but not to the extent of the corn, while the leaves of milo very seldom showed signs of wilting. The smaller increase in dry matter in the leaves of corn and kafir during the greater portion of the day in comparison to the leaves of milo is evidently due to the severe climatic conditions. The high evaporation of water from the leaves of corn and kafir exceeds the intake by the roots and as a consequence, the water content is lowered to such an extent as to interfere with the vital processes of the protoplasm. The rise in temperature of the leaves due to the decreased transpiration may also be a factor in lowering the photosynthetic power of these plants.

The results indicate that under the conditions of these experiments, the sorghums and more particularly milo, can absorb water from the soil and transport it to the leaves more rapidly in proportion to the loss of water from the plant than can corn. As a result of this ability the sorghums can produce more dry matter for each unit of leaf area under severe climatic conditions than can the corn plant.

Further details are given in the *Journal of Agricultural Research*, 1917.

NOTE ON COMPLEMENTARY FRESNELLIAN FRINGES

By Carl Barus

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Communicated May 9, 1917

Measurement of Small Angles without Auxiliary Mirror.—This method makes use of the original apparatus heretofore described, but the two mirrors M and M' or N and N', figure 1, are rotated together as a rigid system around a vertical axis, at A for instance. In view of the absence of auxiliary reflection, the method will be but half as sensitive as the preceding one, so that the equation

$$R\Delta\alpha = \Delta N \cos i$$

is sufficient to approximately express the results. But on the other hand, if spectrum fringes are to be observed, there is greater abundance of light since the half silver film is penetrated but once by each component, *ac* or *bd*. When the achromatic fringes are used however the light is always superabundant and must be reduced in intensity. To try this method the mirrors N and N' were mounted on a good divided circle so as to rotate together on a rigid arm over a small angle α. The achromatic fringes displaced in this way were restored by advancing the mirror M' over the distance ΔN along the normal micrometer screw at n'. The following is an example of the results of corresponding values of ΔN and $\Delta\alpha$, when the distance apart of the rays *ac* and *bd* was $2R = 7$ cm.

α = 0.0°	0.1°	0.2°	0.3°	0.4°	0.5°	0.6°	0.7°	0.8°	0.9°	1.0°
$\Delta N \times 10^3$ = 0.0	8.0	16.6	35.6	34.9	42.7	50.7	61.7	70.5	78.5	90.6 cm.

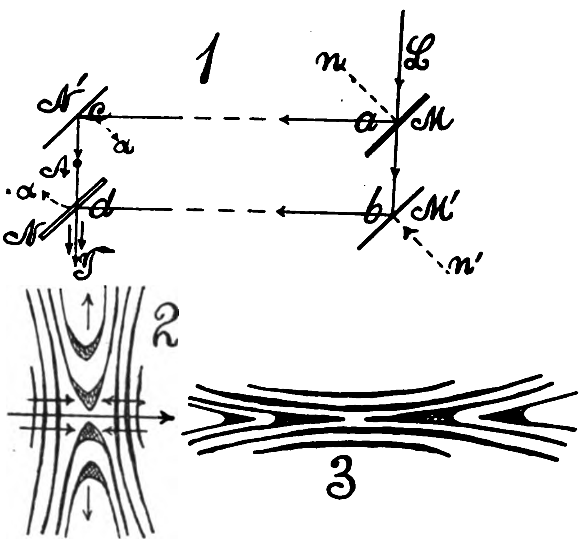
These results as a whole are much smoother than the earlier ones, for incidental reasons. From a graphic construction the mean rate $\Delta N/\Delta\alpha = 0.088$ cm/degree or 5.0 cm/radian may be obtained. Hence since $2R = 7$ cm., $(\Delta N/\Delta\alpha)2/R = 0.013$ in terms of degrees or 0.72 in terms of radians. This result is roughly half that in the preceding paper, if incidental errors be disregarded. From the above equation since $i = 45°$ nearly,

$$\frac{\Delta N}{\Delta\alpha} = \frac{2R}{2\cos i} = \frac{7.0}{1.41} = 5.0$$

agrees closely with the experimental result.

Complementary Fringes—Attention was now given to the hyperbolic fringes of a fine slit and white light, observed in the telescope at T when the ocular is drawn outward or to the rear of its position for the principal

focus and the spectroscope is removed. They appear and widen with the washed image of the slit. They are quite strong and sharp throughout and gorgeously colored, the fields and shades figure 2 being nearly complementary in color. The spectrum fringes must be centered if the others are to occur. The former (fig. 3) are usually long ellipses or hyperbolic with the major axis horizontal, while the corresponding new fringes (fig. 2) are hyperbolic with their major axis vertical. They are extremely sensitive to rotation of the micrometer mirror about a horizontal axis, rising or falling, but they soon vanish. When the micrometer mirror M' is rotated around a vertical axis, an operation which separates the white slit images in the telescope if originally coincident, the new fringes move bodily by displacement, from left to right, or the



reverse, depending on the sign of the rotation, while they continually change their color scheme. When the design is thus displaced as a whole the individual fringes move as shown in figure 2. As a group the fringes closely resemble the lemniscates of a binaxial crystal in polarized light. The variation of the color scheme is probably the same since with homogeneous light (sodium) the design is in yellow and black. The pattern is not quite dichroic but appears so, red-green, blue-yellow combinations with intermediate violet-yellowish succeeding each other. In polarized light the figures are sharpened as a whole, but there is no discrimination. The pattern gradually vanishes with a wide slit, whereupon the achromatic fringes may be seen when the ocular is restored to the principal focal plane.

If the white slit images in the telescope pass through each other (in consequence of the rotation of M' about a vertical axis, as specified) the direction of fringes *twice* changes sign in rapid succession and this probably occurs when the white slit images are coincident. Barring this inversion, the march is regular and proportional to the rotation.

With the displacement (ΔN) if the mirror M' on the micrometer screw normal to its face, the fringes pass through a continuous succession of color schemes, but soon vanish; for they coincide in adjustment with the centered spectrum fringes. Similarly if a pair of mirrors (MM' or NN' (fig. 1)) rotates about a vertical axis as a rigid system, the same continuous change of color scheme and evanescence is apparent.

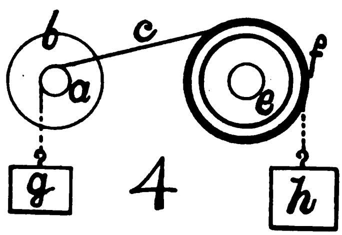
These interferences differ, naturally, from the spectrum interferences; they also differ from the achromatic interferences, which are much finer fringes, partaking of the regular fringe pattern seen with biprisms. They are a separate phenomenon, quite sharp and definite, occurring under like conditions of adjustment, but under different conditions of observation (ocular out of focus and fine slit). In the principal focus the two sharp extremely bright slit images are alone present. They are absolutely identical in structure, however, and their spectra when superposed would interfere symmetrically throughout their extent. Under these circumstances the rays intersecting in the white slit images also interfere before and behind the principal focal plane of the telescopic images specified, and this interference is not destroyed when the slit images are separated (rotation of opaque mirror M' about vertical axis), or when the slit images are passed through each other. What is not at once seen however is the reason of the occurrence of large sharp definite hyperbolic forms instead of the usual Young or Fresnel fringes of two slits or slit images.

On the Michelson interferometer these fringes (like the achromatic fringes) are extremely faint and can hardly be detected except by putting them in slow motion. The spectrum fringes are equally strong in all cases. It appears therefore that the two half silvers are favorable to evolving both the hyperblic and the achromatic set of fringes. The Michelson design is thus not useful here, nor for the measurement of small angles of rotation by the methods described, as the mirrors would have to be rotated in opposite directions.

Further work with the complementary fringes on different interferometers of the Jamin type showed that to produce the hyperbolics, the fine slit images must coincide horizontally and vertically. They do not in this case probably coincide in the fore and aft direction; for the plates, etc., were not optically flat. When the slit images are separated at the

same horizontal level into two fine parallel lines, the complementary fringes in fact become Fresnellian fringes, finer as the slit images are more separated and as the ocular is more rearward or forward. This is precisely what should occur. We may conclude therefore that the complementary fringes are Fesnellian interferences of two slit images and that the central hyperbolic forms are due to outstanding front and rear positions of the two slit images, which seem to coincide in the field of the telescope. Differentiated from these, the achromatic fringes are referable to the colors of thin plates. I have in fact since succeeded in obtaining the complementary fringes in the shape of broad straight gorgeously colored vertical bars, without suggestion of hyperbolic contour.

An attempt was made to get quantitative estimates of the passage of fringes, on rotating the paired mirrors NN' over an angle α. To control the small angles the device figure 4 was improvised and did good service. Here e is the tangent screw of a 6″ divided circle. It is surrounded by a snug anulus of cork and holds the brass ring f, on whose surface a



4

coarse screw thread has been cut. Near this and with its axis in parallel is a $\frac{1}{4}$ inch screw a and socket (not shown), controlled by the disc b. A strong linen thread c is looped once around f in the grooves of the screw and once or twice around the grooves of a, the string being normal to the cylinders and kept taut by two small weights, g, about a half ounce and h, about an ounce. The head b may be turned either way and the angle read off in minutes on the head of the tangent screw e.

The theoretical value apart from glass paths and other corrections should be, per fringe vanishing

$$2R\Delta\alpha = \lambda$$

where R is the radius of rotation corresponding to the angle $\Delta\alpha$ and λ the mean wave length of light. In the given adjustment $2R = 10$ cm. was the normal distance apart of the two interfering beams. Hence

$$\Delta\alpha = \frac{\lambda}{2R} = \frac{60 \times 10^{-6}}{10} = 6 \times 10^{-6} \text{ radians or } 1.2''.$$

per vanishing fringe. As the change of glass path of one beam would have to be deducted from $2R$, a somewhat larger value would be anticipated. Testing the complementary fringes (white light) the passage of about 25 fringes completed the phenomenon after which it paled to whiteness. These 25 fringes passed within $\Delta\alpha = 0.75'$, or per fringe about $0.03'$ or $1.8''$ of arc. Of course this is merely an estimate from the small angles of turn involved.

The complementary fringes with sodium light are available indefinitely. I counted about 100 fringes for an angle of $2.7'$, i.e., $1.6''$ per fringe.

Finally using the spectrum fringes of the spectroscope, about 120 fringes were counted within $3'$, i.e., $1.5''$ per fringe. All of these values are larger than the computed value $\lambda/2R$ without correction, but in view of the large number of fringes within exceedingly small angle $\Delta\alpha$, sharp agreement is not to be expected.

[This note is from a Report to the Carnegie Institution of Washington, now in preparation.]

THE DISPLACEMENT INTERFEROMETRY OF LONG DISTANCES

By Carl Barus

DEPARTMENT OF PHYSICS, BROWN UNIVERSITY

Communicated, May 22, 1917

1. *Small Angles*.—In my preceding notes I suggested two methods for the measurement of small angles. The first used an auxiliary mirror and apart from corrections the angle $\Delta\alpha$ over which the auxiliary mirror turns is

$$\Delta\alpha = \Delta N \cos i/2R \qquad (1)$$

where ΔN is the displacement of one of the plane mirrors parallel to itself necessary to restore the achromatic fringes to their former position in the field of the telescope, i the angle of incidence (conveniently 45°), $2R$ the normal distance apart of the (parallel) interfering pencils in the fore and aft direction of the incident beam. In the second method the auxiliary mirror is dispensed with and the rotation of a rigid system of paired mirrors is used. The sensitiveness is half the preceding.

2. *Distances*.—Suppose now that the paired mirrors near the telescope confront but a part of the area of the objective and that the telescope can therefore look over the mirrors directly into the region beyond. (A series of small mirrors or reflecting prisms may be employed

to the same purpose; or the mirrors may both be half silvered and transparent.) The telescope now contains two images, the first due to rays (K) entering it directly, the second due to rays (L) reflected into it by the mirrors of the interferometer. Suppose the object seen lies at infinity like a star, that its two images are made to coincide by adjusting the angle α, and that the achromatic fringes have been brought into the field by adjusting the micrometer displacement N.

Now let the angle α be changed by $\Delta\alpha$ until the two images of an object M, at a measurable distance d, coincide. Displace the micrometer mirror by ΔN until the achromatic fringes are restored to their former position. Let b be the effective distance apart of the paired mirrors in the direction right and left to the observer or transverse to the impinging rays (L) and finally s the angle at the apex of the triangle of sight on the base b; i.e. the small angle between the present rays KL. Then

$$d = b \cos g \quad s = b \cos g \quad 2\Delta\alpha = b/2\Delta\alpha \tag{2}$$

(nearly) by the laws of reflection. Hence from equation (1)

$$d = bR/\Delta N \cos i \tag{3}$$

Here $2bR$ is the area of the *ray parallelogram* of the interferometer. Using the constants of my apparatus, let $i = 45°$, $R = 10$ cm., $b = 200$ cm., $\Delta N = 10^{-4}$ cm., the latter being the smallest division on the micrometer. Hence

$$d = 200 \times 10/10^{-4} \times 0.71 = 2.8 \times 10^{7} \text{ cm.} = 280 \text{ kilometers.}$$

or about 170 miles is the limit of measurement of the apparatus.

3. *Performance.*—Again from equation (3) the sensitiveness $\delta(\Delta N)/\delta d$, since

$$\delta d = (d^2 \cos i/bR)\delta(\Delta N) \tag{4}$$

is inversely proportional to the square of the long distance d and the area of the ray parallelogram $2bR$. Thus with the above constants, if d is 2 kilometers, $\delta(\Delta N) = 10^{-4}$ cm., then

$$\delta d = (2 \times 10^5)^2 \times 0.71 \times 10^{-4}/200 \times 10 = 14 \times 10^2 \text{ cm.} = 14 \text{ meters.}$$

Thus an object at about a mile should be located to about 30 feet. Per fringes of mean wave length λ, moreover, $\delta d = \lambda d^2/2bR$, the placement should be about 6 meters at 2 kilometers. I have stated the case, of course, merely for the interferometer, not for subsidiary optical appurtenances.

NATIONAL RESEARCH COUNCIL

MEETINGS OF THE EXECUTIVE COUNCIL

The tenth meeting of the Executive Committee was held at the offices of the Council on January 25, 1917. Messrs Carty, Chittenden, Conklin, Dunn, Pupin, Vaughan, Welch, and the Secretary were present.

The Secretary presented Dr. Holmes' report, dated January 16th, on the composition of the Committee on Anthropology. It was understood that this committee will be formed in coöperation with the American Association for the Advancement of Science, as has been done with other science committees.

The Secretary presented the following resolution of the National Advisory Committee for Aeronautics, accepting designation by the National Research Council as the Aeronautical Committee of the Research Council:

> RESOLVED: That the invitation of the National Research Council that the members of the National Advisory Committee for Aeronautics act as the Committee on Aeronautics of the National Research Council be accepted on appointment of its members by the National Research Council as such committee.

The Executive Committee adopted the following resolutions:

> RESOLVED: That the National Research Council acknowledges the acceptance by the National Advisory Committee for Aeronautics of the invitation of the National Research Council to serve as its "Aeronautics Committee" and hereby appoints Dr. Charles D. Walcott, Chairman, and Dr. Joseph S. Ames, Prof. Chas. F. Marvin, Prof. M. I. Pupin, Brig. Gen. Geo. O. Squier, and Dr. S. W. Stratton, members of the Aeronautics Committee of the National Research Council.
>
> RESOLVED: That the Executive Committee of the National Research Council cordially thanks the National Advisory Committee for Aeronautics for its generous action in placing its services at the disposal of the National Research Council in this matter.

Dr. Vaughan then presented the matter of the Medicine and the Hygiene Committees, urging that these two committees be merged. After considerable discussion, it was agreed that the former action of the Executive Committee creating separate committees for medicine and for hygiene be reconsidered, and that one committee, to be designated the Medicine and Hygiene Committee, be appointed. Dr. Vaughan then presented a list of names for this committee; explaining the need of a large committee by the fact that the many different aspects of medicine and hygiene must be considered.

The Executive Committee approved the appointment of the persons listed and authorized the organization of the committee.

The eleventh meeting of the Executive Committee was held on February 7, 1917. Messrs. Carty, Dunn, Pupin, and the Secretary were present; also Dr. Bogert by invitation.

The meeting was devoted principally to the discussion of the appointment of a committee of submarine detection, recommended in a telegram received from

the Chairman of the Council. It was voted to call a joint meeting of the Executive Committee and the Military Committee in Washington on February 17 to act upon this question.

The general policy of the Research Council in regard to the giving out of information was discussed; and it was voted that the chairmen of all committees be requested not to give out any statements, written or oral, regarding matters of public policy or interest, save through the intermediary of the office of the Council.

Dr. Bogert gave an informal account of some of the work of the Chemistry Committee. He recommended the appointment of older, retired chemists as advisory members of the committee. He also reported the appointment of Wilder D. Bancroft, of Cornell University, as Chairman of the Sub-Committee on Electro-chemistry. All of Dr. Bogert's recommendations were agreed to by the Committee.

The twelfth meeting of the Executive Committee, consisting in a joint session with the Military Committee of the National Research Council, was held on February 17, 1917, at the Smithsonian Institution, Washington. Messrs. Carty, Chittenden, Conklin, Dunn, Millikan, Pearl, Pupin, Stratton, Vaughan, Walcott, and Welch, and the Secretary of the Executive Committee, and General Gorgas, General Squier, Admiral Taylor, and Admiral Griffin of the Military Committee, were present; also, by invitation, Dr. Bogert, Chairman of the Chemistry Committee.

Dr. Stratton, speaking as Secretary of the Military Committee, told of two important problems that are already under way, which had originated with this committee. The first is the disposition of the opium confiscated by the Government; the second, the preparation of a specification for a good blanket. The Military Committee was of the opinion that the Government should use confiscated opium, turning it into harmless salt rather than to destroy it. Action to this end has been taken. Conferences regarding the matter of bla ket specifications are under way.

The procedure to be followed by the Military Committee was discussed; and the Executive Committee agreed that the Military Committee shall correspond with the chairmen of the various other committees of the Council which may be concerned in the problems originating with the Military Committee.

It was voted to recommend to the President of the National Academy of Sciences that Admiral Ralph Earle, Chief of the Bureau of Ordnance of the Navy Department, and Mr. Herbert C. Hoover be made members of the Research Council. The former thereby becomes a member of the Military Committee.

The Secretary reported that Eliakim H. Moore had been made a member of the Council and Chairman of the Mathematics Committee.

It was voted that an Engineering Committee be authorized and that the

personnel be determined by conference between members of the Executive Committee and the presidents of the national engineeing societies, in order that the full coöperation of the engineers of the country might be secured.

It was voted that a Committee on Anatomy be authorized and that Dr. Hale be requested to nominate its chairman.

At this point the meeting adjourned, and the members assembled at a luncheon kindly provided by Dr. and Mrs. Walcott. Members of the Council of National Defense and its Advisory Commission, namely, Messrs. Godfrey, Coffin, and Gifford, joined the members of the Council at luncheon.

The members reassembled thereafter in conference with the members of the Council of National Defense; and, after an explanation by Dr. Carty of the formation and scope of the work of the National Research Council, the relations of the two bodies were discussed.

The thirteenth meeting of the Executive Committee was held on February 26, 1917. Messrs. Carty, Dunn, Pupin, and the Secretary were present.

The particular object of the meeting was to consider a resolution to be forwarded to the Council of National Defense, defining the form of relations that the National Research Council desires to hold with the Council of National Defense. After full discussion, the following resolution was adopted:

RESOLVED: That the National Research Council, established at the request of the President of the United States for the organization and promotion of scientific research in the interest of national defense and national welfare, is of the opinion that its services to the National Government in its coöperation with the Council of National Defense will be most effective if acting directly with the Council of National Defense through its Director.

The Secretary was instructed to transmit this to the Director of the Council of National Defense as an expression of the attitude of the Research Council.

The fourteenth meeting of the Executive Committee of the National Research Council was held on March 7, 1917. Messrs. Carty, Chittenden, Conklin, Dunn, Stratton, and the Secretary were present.

The Secretary announced the completion of the Committees on Astronomy, Agriculture, Physiology, Geology; and stated that the Committees on Botany, Zoology, and Anthropology have not yet been completed. He reported Dr. Hale's recommendation that a Committee on Anatomy be authorized.

Messrs. John C. Merriam and Charles R. Cross were appointed members of the Census Committee.

The Secretary reported that Carl L. Alsberg and Alonzo E. Taylor have been appointed members of the National Research Council by the President of the National Academy of Sciences.

The Secretary read the offer of the faculty of Sloane Laboratory of Yale University of its services and facilities, to the National Research Council for research work for national defense. Dr. Stratton presented letters with similar offers from the American Society of Mechanical Engineers, the General

Electric Company, and the Jefferson Physical Laboratory of Harvard University. These offers all placed at the disposal of the Research Council valuable facilities for conducting research. The Executive Committee instructed the Secretary to prepare and transmit a resolution, expressing appreciation of these offers; and to inform the Council of National Defense that the National Research Council holds these facilities at its disposal.

The Secretary presented Dr. W. M. Davis's report on the work of the Geography Committee. It was received, and the Secretary was instructed to inform Dr. Davis that the Executive Committee approves the proposed plan.

The Secretary presented the report of Dr. J. M. Clarke, Chairman of the Geology Committee, requesting the Executive Committee to approve his action in taking steps to better the instruction in geology in the engineering schools and colleges of the country. The Executive Committee voted to endorse Dr. Clarke's action. Reports were also presented from Prof. E. C. Pickering, Chairman of the Committee on Astronomy, and Dr. V. C. Vaughan, Chairman of the Committee on Medicine. It was voted that an Engineering Committee be authorized, to consist of the following members of the National Research Council, with whom others may be joined from time to time by the Executive Committee: Gano Dunn (Chairman), J. A. Brashear, J. J. Carty, W. F. M. Goss, Clemens Herschel, C. F. Rand, C. E. Skinner, S. W. Stratton, Ambrose Swasey, and Elihu Thomson.

It was voted to appoint a new Nitrate Supply Committee to consist of the Chiefs of the Departments of Ordnance of the Army and of the Navy, representatives of the Department of Agriculture, the Bureau of Mines, and Bureau of Standards, and Messrs. Leo H. Baekeland, Gano Dunn, C. H. Herty, A. A. Noyes, and W. R. Whitney, members of the former Nitrate Supply Committee. (This committee was subsequently replaced by a similarly constituted one appointed officially by the Secretary of War.)

The Secretary reported that in response to the resolution adopted by the National Research Council, a resolution was adopted by the Council of National Defense on the 28th of February as follows:

RESOLVED: That the Council of National Defense, recognizing that the National Research Council, at the request of the President of the United States, has organized the scientific forces of the country in the interest of national defense and national welfare, requests that the National Research Council coöperate with it in matters pertaining to scientific research for national defense, and, to this end, the Council of National Defense suggests that the National Research Council appoint a committee of not more than three, at least one of whom shall be located in Washington, for the purpose of maintaining active relations with the Director of the Council of National Defense.

The Secretary reported the appointment of a Sub-Committee on Smokeless Powder by the Military Committee. This Sub-Committee is composed of Admiral Griffin, Admiral Earle, Messrs. C. D. Walcott, Comey, and Harry T. Brown.

The fifteenth meeting was held in New York on March 22, 1917. Messrs. Carty, Conklin, Dunn, Hale, and the Secretary were present; also upon invitation Drs. M. T. Bogert and J. M. Clarke.

It was voted to ask Dr. Charles H. Herty to become a member of the Publicity Committee and to take especial charge of chemical publicity in accordance with the general publicity policy of the Publicity Committee of the Council.

Dr. Hale spoke of the urgent need of acting in accordance with the resolution of the Council of National Defense, by appointing a committee of three to work with the Council of National Defense in Washington, of which one member, as requested by the Council of National Defense, shall be in Washington permanently, with headquarters in close connection with the Council of National Defense. After full discussion of the matter it was voted that a committee of three be appointed to coöperate with the Council of National Defense, in accordance with the resolution of that Council, to consist of Dr. C. D. Walcott, Dr. S. W. Stratton, and Dr. R. A. Millikan; that an office be secured in Washington in the Munsey Building as headquarters of the Research Council, and that Dr. Millikan be asked to take charge of it.

Dr. Hale stated that Mr. Tod Ford of Los Angeles is willing to come to Washington and give all his time to the work of the Research Council. It was voted that Mr. Ford be employed as Assistant Secretary in the office of the Research Council in Washington.

Dr. Hale proposed the formation of a volunteer corps of scientific observers to go to Europe to study the development of scientific research in the problems of the war. After discussion it was voted that Dr. Hale be authorized to appoint such persons as he elects to proceed to Europe to study the conditions there brought about by the war, and to take such other steps as he deems advisable to further this general purpose.

Dr. Clarke, Chairman of the Geology Committee, then reported briefly upon the work of his committee. He asked the approval of two sub-committees, one on Camp Sites, another on the Location of Distant Artillery by Seismological Measurements. He was authorized to appoint them, with Dr. Penrose as Chairman of the Sub-committee on Camp Sites, and Dr. Woodworth of that of Seismological Measurements.

Dr. Hale then offered his resignation as Chairman of the Committee on Research in Educational Institutions, and suggested that Dr. R. H. Chittenden be asked to accept the Chairmanship in his stead. The committee accepted with regret Dr. Hale's resignation and approved the appointment of Dr. Chittenden.

In view of the proffered assistance of the American Institute of Consulting Engineers, it was voted that the President of the American Institute of Consulting Engineers be asked to suggest one of its members to be appointed a member of the National Research Council.

CARY T. HUTCHINSON, *Secretary.*

NATIONAL RESEARCH COUNCIL

JOINT MEETING OF THE EXECUTIVE, MILITARY, AND ENGINEERING COMMITTEES

This meeting was held in Washington, on May 3, 1917, at 10 a.m.

There were present: Messrs. John A. Brashear, John J. Carty, Russell H. Chittenden, Gano Dunn, William F. Durand, John R. Freeman, Hollis Godfrey, William C. Gorgas, Robert S. Griffin, George E. Hale, Frank B. Jewett, Van H. Manning, Edgar Marburg, Charles F. Marvin, C. E. Mendenhall, Robert A. Millikan, F. H. Newell, Arthur A. Noyes, Michael I. Pupin, Albert Sauveur, Clayton H. Sharp, C. E. Skinner, George O. Smith, George O. Squier, Lewis B. Stillwell, S. W. Stratton, Ambrose Swasey, David W. Taylor, Chas. D. Young, Charles D. Walcott, and Pope Yeatman. Dr. C. D. Walcott presided.

The meeting was devoted to an extended discussion of the functions to be fulfilled and the work to be undertaken by the Engineering Committee.

FIRST MEETING OF THE ENGINEERING COMMITTEE

This meeting was held in Washington, on May 3, 1917, at 2.10 p.m.

Present: Messrs. Gano Dunn (Chairman), John A. Brashear, John J. Carty, William F. Durand, John R. Freeman, Hollis Godfrey, Frank B. Jewett, Edgar Marburg, F. H. Newell, Michael I. Pupon, Albert Sauveur, Clayton H. Sharp, C. E. Skinner, Lewis B. Stillwell, George F. Swain, Ambrose Swasey, Pope Yeatman, and Charles D. Young, members of the Committee; also G. E. Hale, A. A. Noyes, and R. E. Millikan of the Council.

The chairman introduced the question of the organization of the Engineering Committee. Mr. J. R. Freeman moved that one man be appointed to act as director of the work of the Engineering Committee, who should live in Washington. This motion was carried.

Mr. Swain moved that W. F. Durand be the Washington head of the Engineering Committee, to be known as Vice-Chairman of the Committee, with Gano Dunn remaining as Chairman. This motion was carried, and Mr. Durand accepted the appointment.

It was voted that the Secretary of the National Research Council be appointed Secretary of the Engineering Committee.

The matter of rules and procedure of the Engineering Committee was then brought up, and it was voted that a Committee on Procedure be appointed, with power to adopt rules. The Chairman appointed Messrs. Durand, Young, and Stillwell to be such a committee.

A discussion of various phases of the work of the Committee, participated in by many of the members present, took place.

CARY T. HUTCHINSON, *Secretary.*

NATIONAL RESEARCH COUNCIL

REPORT OF THE ASTRONOMY COMMITTEE

The present report relates only to the scientific needs of Astronomy. Its applications to the possible services that astonomers can render in the war, as a part of the work done by the National Research Council in connection with the Council of National Defense, will be made the subject of a separate study.

The report of the Committee on Astronomy of the Committee of One Hundred (*Science*, **45**, 135) gives the views of a majority of the members of the present Astronomy Committee. It shows the uses that could be made at once of a fund for astronomical research. They need not all be repeated here, but two are of such importance and general application that attention may be called to them.

First, aiding existing southern observatories, or establishing new observing stations to render our knowledge of the southern stars comparable with that of the northern stars. As the resources of northern observatories are far greater than of those south of the Equator, much might be accomplished by taking photographs of the southern stars, and sending them north for measurement, reduction, and perhaps discussion.

Secondly, securing the aid of very large reflectors, both north and south, for the extension of nearly every research to very faint stars. This is especially the case in clusters, or other objects, where photographs with short exposures would furnish material for prolonged study. Experts all over the world might thus be provided with material of the greatest value, while the owners of the telescopes would have the satisfaction of having their photographs discussed by those best qualified to do so. The continuation of the investigation with large reflectors of the radiation of stars and planets is regarded by many as of paramount importance.

Letters are published in *Science*, **41**, 82, giving the needs of twelve leading American astronomers. In almost every case, the demand was for more assistants to aid in extensive routine observations. A relatively very large increase of output could thus be secured. Six of these astronomers are members of the present Committee.

The publication each year of a brief statement of the work done by every American observatory engaged in research, as is now done by European observatories, would aid greatly in forming plans for coöperation.

Examples are given below of large routine investigations needed at the present time. Several of them, such as 2, 4, 8, 17, and 20, are already making excellent progress, according to a definite system. In many cases, plans could best be carried out by small committees of experts. The American Astronomical Society, which, with few exceptions, includes among its members all the leading astronomers of the country, has already appointed several such committees.

For statistical purposes, it is very desirable to include all stars brighter than a given magnitude. This limit is often that for which the desired observations can be obtained. If not, stars of special interest should be included, and in some cases a fixed number of stars of each fainter magnitude. Preference should here be given to the standard regions adopted at Groningen and Harvard.

1. The absolute positions of the stars from observations of the Sun, Mercury, and Venus. These objects are difficult to observe visually, and a trial of photographic methods, either by direct measurement, or by a photographic transit circle, is suggested.

2. Positions of all stars of the ninth magnitude, and brighter, with photographic doublets.

3. Precise positions of standard stars, generally of about the ninth magnitude, needed for the reduction of No. 2. Each star should be observed with transit micrometers, attached to first class meridian circles, on at least three nights at two observatories, if feasible. The aid of national observatories should be secured in this work, since they are best qualified to undertake it. These stars should be selected whenever possible from the list of intermediary stars of the Comité International Permanent. Nos. 2 and 3 should be carried on simultaneously.

4. Proper motions of all stars of the magnitude 7.5, or brighter.

5. Proper motions of stars in spiral, and other, nebulae.

6. Restoration of the stations for determining variations in latitude both visually and photographically.

7. Sufficient measures of all known double stars to determine the relative positions of the components, and if they have perceptible motions to furnish adequate material for an orbit. In the first case, these observations should be made with three telescopes, with a computed probable error not exceeding $\pm 0''.05$. These observations should be repeated every ten years, or, in the case of known binaries of short period, more frequently. For many stars, the material needed already exists.

8. Parallaxes of all stars of the sixth magnitude, and brighter, and of a selected list including variables, stars having large proper motion or parallax, binaries, nebulae, etc. The same stars should be observed with several telescopes to eliminate systematic errors, as far as possible. Some clusters, and stars of Class B, whose parallaxes are presumably small, should therefore be included.

9. Precise positions by modern methods, photography and transit micrometer, of the Moon and major planets.

10. Orbits and ephemerides of asteroids and comets so far as this is not provided for by the Rechen-Institut. The loss of a faint asteroid may sometimes be avoided by promptly computing a circular orbit for it.

11. Spectra of special stars too faint to be classified on existing photographs. They can probably be obtained by a focal plane spectroscope, or objective prism, attached to a large reflector.

12. Peculiarities of spectra, including the relative intensities of lines, especially of those of wave lengths 4216, 4455, etc. Study of lines of great and small wave-lengths. Intensity of light of different wave-lengths.

13. Radial velocities of stars, including all of magnitude 6.0, and brighter, with a computed probable error not exceeding 3 km. Radial velocities of a selected list of stars like those in No. 8.

14. Radial velocities of spectroscopic binaries. Minimum number of good determinations forty, with at least one in each twenty-fourth of the period.

15. Photographic and photovisual magnitudes of a Durchmusterung of all stars of the ninth magnitude, and brighter, on the international scale. It may be best to begin with 40,000 stars, one in each square degree of about the ninth magnitude, or with the standard stars of No. 3.

16. Photographic and photovisual magnitudes of selected lists of faint stars.

17. Photometric, photographic, or visual determinations of the brightness of all variables of long period, or irregular, having a range of four magnitudes, or more, or three magnitudes when of the ninth magnitude, or brighter, at maximum.

18. Light curve of all variables of short period. For the bright stars the photoelectric cell should be used. Dividing the period into twenty-four equal parts, a point on the curve should be determined in each of these parts with a probable error not exceeding ± 0.02 magn. Observations should be made every five years, at phases when the light is changing most rapidly, to determine the variations in the period. In the case of globular clusters and the Magellanic Clouds, photographs with large reflectors are much to be desired to extend the work to the fainter stars.

19. Measures like those of No. 18 of all Algol variables. In addition, at least twenty points should be determined at nearly equal intervals during the primary, and secondary minimum, if any exists.

20. A catalogue of all stars of the magnitude 6.5, and brighter, giving for each the approximate position, proper motion, radial velocity, parallax, magnitude, spectrum, color index, etc. The type to be kept standing and a new edition issued every three years, giving corrected values computed by experts who should maintain a bibliography accessible to all investigators.

EDWARD C. PICKERING, *Chairman,*

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GEORGE C. COMSTOCK,	HENRY N. RUSSELL,
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VOLUME 3 JULY, 1917 NUMBER 7

PROCEEDINGS
OF THE
NATIONAL ACADEMY OF SCIENCES
OF THE
UNITED STATES OF AMERICA

Publication Office: Williams & Wilkins Company, Baltimore
Editorial Office: Massachusetts Institute of Technology, Cambridge
Home Office of the Academy: Washington, D. C.

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PROCEEDINGS
OF THE
NATIONAL ACADEMY OF SCIENCES

Volume 3 JULY 15, 1917 Number 7

THE CAYLEYAN CURVE OF THE QUARTIC

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Communicated by E. H. Moore, May 5, 1917

The quartic curve $(\alpha x)^4 = 0$ determines a correspondence

$$(\alpha x)^2(\alpha y)\alpha_i = 0, \qquad i = 0, 1, 2,$$

in which x is a point on the Hessian curve and y a point of the Steinerian. The locus of lines ξ joining corresponding points x, y is the Cayleyan, known to be of degree 18 in ξ and 12 in the coefficients of the quartic.

The Cayleyan can be expressed in terms of the two contravariants of the quartic, $(s\xi)^4$ $[\equiv \frac{1}{2}|\alpha\beta\xi|^4]$ and $(t\xi)^6$ $[\equiv \frac{1}{6}|\beta\gamma\xi|^2|\gamma\alpha\xi|^2|\alpha\beta\xi|^2]$, and of terms produced by operating with the polars of these on $(\alpha x)^4$. The working out of this depends on a special reference triangle which is always valid for the general quartic. Suppose

$$\begin{aligned}(ax)^4 \equiv\ & ax_0^4 + 4a_1x_0^3x_1 + 4a_2x_0^3x_2 + 6hx_0^2x_1^2 + 12lx_0^2x_1x_2 + 6gx_0^2x_2^2 \\ & + 4b_0x_0x_1^3 + 12mx_0x_1^2x_2 + 12nx_0x_1x_2^2 + 4c_0x_0x_2^3 \\ & + bx_1^4 + 4b_2x_1^3x_2 + 6fx_1^2x_2^2 + 4c_1x_1x_2^3 + cx_2^4.\end{aligned}$$

Since x and y as given above can never coincide for the general quartic, because

$$(\alpha x)^3\alpha_i = 0, \qquad i = 0, 1, 2,$$

is the condition that $(\alpha x)^4$ have a double point, let x, the point of the Hessian, be the reference point $(0, 1, 0)$ and let y, the point of the Steinerian, be $(0, 0, 1)$, so that $x_0 = 0$ is a line of the Cayleyan. Then

$$\begin{aligned}\alpha_1^2\alpha_2\alpha_i &= 0, \qquad i = 0, 1, 2, \\ \text{or} \quad m &= b_2 = f = 0.\end{aligned}$$

This reference scheme is maintained throughout, though more highly specialized as occasion demands. Under it the Hessian becomes

$2n(bh - b_0^2)x_1^5x_0 + 2c_1(bh - b_0^2)x_1^5x_2 +$ lower powers of x_1;
$(s\xi)^4 \equiv bc\xi_0^4 - 4cb_0\xi_0^3\xi_1 + 4(-bc_0 + b_0c_1)\xi_0^3\xi_2 + \ldots\ldots;$
$(t\xi)^6 \equiv -bc_1^2\xi_0^6 + 2(-bcn + bc_0c_1 + 2b_0c_1^2)\xi_0^5\xi_1 + 6bc_1n\xi_0^5\xi_2 + \ldots\ldots$

In the first place, the Cayleyan is known to be on the stationary lines of the quartic, which are the common lines of $(s\xi)^4$ and $(t\xi)^6$. Therefore the Cayleyan must be made up of terms containing either $(s\xi)^4$ or $(t\xi)^6$ at least once.

Now let us see what are the common lines of the Cayleyan and $(s\xi)^4$. To make x_0 a line of the latter requires that $bc = 0$. If $b = 0$, then x_0 is a stationary line of the quartic. If $c = 0$, then not only is x_0 a line of $(s\xi)^4$, but its contact with it is $(0, 0, 1)$, the point of the Steinerian, which has also become a point of the quartic. Therefore quartic, Steinerian, and $(s\xi)^4$ all meet in a point. The Steinerian, a curve of order 12, meets the quartic in 48 points; the 48 corresponding lines together with the 24 stationary lines make up the 72 common lines of the Cayleyan and $(s\xi)^4$. The condition that the polar point of $(s\xi)^4$ lie on $(\alpha x)^4$ is the vanishing of $(s\xi)^3(s\alpha)\,(s'\xi)^3(s'\alpha)(s''\xi)^3(s''\alpha)(s'''\xi)^3(s'''\alpha)$; this, when multiplied by $(t\xi)^6$, is of proper degree for a term of the Cayleyan. It is, then, the only term of the Cayleyan not containing $(s\xi)^4$.

It is now in order to ask for the common lines of the Cayleyan and $(t\xi)^6$. For x_0 to be a line of the latter requires that $bc_1^2 = 0$. Again setting aside the stationary lines, we have $c_1 = 0$. Then it is seen that x_0 has as its contact with $(t\xi)^6$ the point $(0, 1, 0)$; furthermore, it is tangent to the Hessian at the same point. Therefore there are a certain number of lines of the Cayleyan which are also lines of both the Hessian and $(t\xi)^6$, these two curves having contact on these lines. For the terms of the Cayleyan not containing $(t\xi)^6$ it is sufficient to use

$$\begin{aligned}
&\lambda(s\xi)^4.(s'\xi)^3(s'\alpha)(s''\xi)^3(s''\alpha)(s'''\xi)^3(s'''\alpha)(t\xi)^5(t\alpha)\\
&+ (s\xi)^4(s'\xi)^4\,[\sigma(s''\xi)^3(s''\alpha)(s'''\xi)^3(s'''\alpha)(t\xi)^4(t\alpha)^2\\
&\quad + \tau(s''\xi)^3(s''\alpha)(s'''\xi)^2(s'''\alpha)^2(t\xi)^5(t\alpha)]\\
&+ (s\xi)^4(s'\xi)^4(s''\xi)^4\,[\varphi(s'''\xi)^2(s'''\alpha)^2(t\xi)^4(t\alpha)^2 + \mu(s'''\xi)^3(s'''\alpha)(t\xi)^3(t\alpha)^3]\\
&+\nu\,(s\xi)^4(s'\xi)^4(s''\xi)^4(s'''\xi)^4.(t\xi)^2(t\alpha)^4,
\end{aligned}$$

where λ, σ, τ, φ, μ, ν are undetermined coefficients. By requiring the highest power of ξ_0 to vanish when $c_1 = 0$ certain relations on these coefficients are obtained, not enough to solve, however. To the terms given above it is necessary to add terms containing $(t\xi)^6$;

$$\begin{aligned}
&\rho(t\xi)^6\,.\,(s\xi)^3(s\alpha)(s'\xi)^3(s'\alpha)(s''\xi)^3(s''\alpha)(s'''\xi)^3(s'''\alpha)\\
&+ \epsilon(s\xi)^4(s'\xi)^4(s''\xi)^4(t\xi)^6\,.\,(s'''\alpha)^4
\end{aligned}$$

will be found sufficient.

There are certain other lines known to be lines of the Cayleyan. There are 21 points whose polar cubics as to the quartic break up into a conic and a line, which is a fourfold line of the Cayleyan. Let one of the 21 points be $(1, 0, 0)$; then for the polar cubic to contain x_0 as a factor requires that $b_0 = c_0 = n = 0$. Now, using this condition, require that the highest power of ξ_0 in the expression for the Cayleyan vanish. The result will be certain conditions on the undetermined coefficients, but still not enough to solve.

Instead of putting the Cayleyan again on these lines it is easier to proceed at once to the general reference scheme which has been the basis of all the work, when only $m = b_2 = f = 0$, and require that the highest coefficient of ξ_0 vanish. This at once completes the work and furnishes proof of its correctness. The Cayleyan is obtained as

$$\begin{aligned}
&33\,(s\xi)^4\,.\,(s'\xi)^3(s'\alpha)(s''\xi)^3(s''\alpha)(s'''\xi)^3(s'''\alpha)(t\xi)^5(t\alpha)\\
&-\tfrac{22}{3}\,(t\xi)^6\,.\,(s\xi)^3(s\alpha)(s'\xi)^3(s'\alpha)(s''\xi)^3(s''\alpha)(s'''\xi)^3(s'''\alpha)\\
&+(s\xi)^4(s'\xi)^4\,[15(s''\xi)^3(s''\alpha)(s'''\xi)^3(s'''\alpha)(t\xi)^4(t\xi)^2\\
&\quad-57(s''\xi)^3(s''\alpha)(s'''\xi)^2(s'''\alpha)^2(t\xi)^5(t\alpha)\,]\\
&+(s\xi)^4(s'\xi)^4(s''\xi)^4\,[\tfrac{35}{2}\,(s'''\xi)^2(s'''\alpha)^2(t\xi)^4(t\alpha)^2-10(s'''\xi)^3(s'''\alpha)(t\xi)^4(t\alpha)^3]\\
&+\tfrac{65}{18}\,(s\xi)^4(s'\xi)^4(s''\xi)^4(t\xi)^6\,.\,(s\alpha)^4\\
&-\tfrac{5}{3}\,(s\xi)^4(s'\xi)^4(s''\xi)^4(s'''\xi)^4\,.\,(t\xi)^2(t\alpha)^4.
\end{aligned}$$

Since, however, this expression has been obtained by causing a coefficient to vanish, there is the possibility that it gives merely a syzygy and vanishes identically. Therefore it was tested on the special quartic $x_0^4 + x_1^4 + x_2^4$, where the Cayleyan is known to be $\xi_0^2\xi_1^2\xi_2^2$, and found not to vanish.

The stationary lines of the quartic are known to be lines of the Steinerian. From the above form of the Cayleyan it can be shown that the contacts of these lines are the same for the Cayleyan as for the Steinerian, so that the two curves touch. We had also certain common lines of the Cayleyan and Hessian, which were likewise lines of $(t\xi)^6$. These lines can be shown to have the same contact as to the three curves. The Cayleyan and $(t\xi)^6$ have 108 common lines, 24 of which are absorbed by the flexes, leaving 84 to be accounted for here. Because of the contact of the curves each line counts for two common lines; therefore the Cayleyan, Hessian, and $(t\xi)^6$ touch in 42 points.

Certain interesting facts come up under the reference scheme here employed. The polar conic of $(0, 1, 0)$ is

$$hx_0^2 + 2b_0x_0x_1 + bx_1^2 = 0,$$

two lines, which coincide if

$$bh - b_0^2 = 0.$$

But this requires the Hessian to have a double point; therefore the general quartic cannot have a polar conic made up of two coincident lines. Also the polar cubic of (0, 0, 1) is

$$a_2x_0^3 + 3lx_0^2x_1 + 3gx_0^2x_2 + 6nx_0x_1x_2 + 3c_0x_0x_2^2 + 3c_1x_2^2 + cx_2^3 = 0.$$

This can have a cusp only if

$$c_1l - n^2 = 0.$$

This has clearly nothing to do with $b = 0$, the condition that x_0 be a stationary line of the quartic. Therefore the cusps of the Steinerian do not lie on the stationary lines, as might be expected from their number —twenty-four. $n = 0$ is the condition that $x_2 = 0$ be the tangent to the Hessian; then the cusp cannot be obtained by making $c_1 = 0$, for then the Hessian has a double point. Putting $l = 0$ shows that the cusp tangent is also the tangent to the Hessian. Use of $n = 0$ also shows that the polar points of lines of the Cayleyan as to $(t\xi)^6$ lie on the corresponding tangents to the Hessian.

A SEARCH FOR AN EINSTEIN RELATIVITY-GRAVITATIONAL EFFECT IN THE SUN

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Communicated by G. E. Hale, June 5, 1917

From the equivalence principle of generalized relativity Einstein[1] concludes that the propagation of light is influenced by gravitation, and deduces two important consequences that can be subjected to the test of observation; namely, a train of light waves passing close to the edge of the sun is refracted so that the angular distance of a star appearing near the sun is increased by 1″.75, and the Fraunhofer lines are displaced to the red in the solar spectrum by an amount equivalent to a velocity of recession of 0.634 km/sec. The amount depends only on the difference in gravitational potential between the gravitation field in which the radiation originates and the field where it is received. In the case of massive stars with density comparable to that of the sun the line displacement may be large, equivalent to 0.634 km/sec. $M^{2/3} d^{1/3}$, where M and d are in terms of the sun's mass and density.[2] Confirmation of either of these consequences would have not only an important bearing upon the establishment of the relativity principle but also upon the in-

terpretation of astrophysical data. The problem of determining stellar motions in the line of sight, a matter of fundamental importance, would be confronted with difficulties of a high order, depending as it does upon line displacement in stellar, relative to terrestrial spectra. Our knowledge of the motions, pressure, and many other phenomena in the solar atmosphere must be obtained from line displacements in the spectrum, but here it would be possible to apply definite corrections, this would in many cases, however, modify our interpretations.

The question of confirmation may be approached through direct observation of the displacement of a star near the sun[3] but the conditions of observation are beset with great difficulties, statistical study of stellar masses and motions[4] or the determination of displacements of solar lines[5] under conditions that eliminate other possible causes.

Some results of an investigation bearing upon the relativity displacement of lines in the sun's spectrum are here communicated, a full account will appear later in a *Contribution from the Mount Wilson Solar Observatory*.

The substance of the investigation is the behavior of lines in the nitrogen (cyanogen) bands, λ 3883, at the center and at the limb of the sun, these lines owing to their freedom from pressure shift are in so far well fitted for such an investigation, but their compound character in the most important series and the frequent superposition of the lines of different series may introduce disturbing factors. The line density in the bands is high, above ten lines per angstrom. The possible occurrence of blends with metallic lines and the closeness of adjacent lines are important considerations in the selection of lines for observation. As the probability of blends is least for the narrowest lines and the precision of measurement is highest for lines showing no evidence of duplicity and sufficiently separated from adjacent lines,[6] a greater weight is attributed to the lines of group A than to the broader lines of group B in the accompanying table.

The wave-lengths of the lines were measured in the arc and at the center and limb in terms of identical iron standards. The limb-center shifts were found by two independent determinations. The sun-arc displacements at the center were obtained by direct comparison and by three indirect methods. The 'Standard' (R−I) is the mean difference, Rowland minus International for lines whose wave, lengths in the sun and arc are equal, determined from the iron lines in this region by taking account of their sun-arc displacements.

The mean, zero, given by 43 lines for center-arc requires from the relativity point of view a radial movement in the solar atmosphere just

COMPARATIVE SOLAR AND TERRESTRIAL WAVE-LENGTHS OF LINES OF THE NITROGEN (CYANOGEN) BANDS

	GROUP A 25 LINES INTENSITY	GROUP B 18 LINES INTENSITY
At center	00−1	2−4
a. Direct comparison of sun and arc	0.000	+0.0013 A
b. λ at center minus λ in arc	0.000	+0.0026
c. (R−I) for band lines minus "Standard" (R−I)	−0.003	+0.0010
d. (λ limb−λ arc) minus (λ limb−λ center)	−0.001	+0.0008
Mean (Center−Arc)	−0.001	+0.0014
	17 LINES INTENSITY	18 LINES INTENSITY
At limb	00−1	2−4
a. λ at limb minus λ in arc	0.000	+0.0037
b. (λ center−λ arc)+(λ limb−λ center)	0.000	+0.0035
Mean (Limb−Arc)	0.000	+0.0036

balancing the gravitational effect; at the limb where its influence would disappear, a displacement to the red of 0.008 A should be observed. For the lines of highest weight the displacement at the limb is zero, the mean for all lines +0.0018 A. Owing to the probable occurrence of blends in this region, the displacement to the red for the broader lines of group B may be in part at least attributed to this influence, as the effect of blends with metallic lines is to introduce systematic displacement to the red even for lines normally undisplaced at the limb.

The general conclusion from the investigation is that within the limits of error the measurements show no evidence of an effect of the order deduced from the equivalence relativity principle. Two other series of observations at Mount Wilson are at variance with the relativity principle, the equal wave-lengths of the H and K lines of calcium in the arc and at the sun's limb[7] and a center-arc displacement of 0.004 A at λ 6300 for iron lines instead of 0.013 A required by the relativity principle.

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TRIADS OF TRANSFORMATIONS OF CONJUGATE SYSTEMS OF CURVES

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Communicated by E. H. Moore, June 8, 1917

When the rectangular point coordinates x,y,z, of a surface satisfy an equation of the form

$$\frac{\partial^2\theta}{\partial u \partial v} = a\frac{\partial\theta}{\partial u} + b\frac{\partial\theta}{\partial v}, \tag{1}$$

the curves $u = \text{const.}$ $v = \text{const.}$ form a conjugate system. We assume that the parametric system is of this sort throughout this note, and we shall speak of the *net* of parametric curves. Equation (1) is the *point equation* of the net.

If N is such a net, a second net N' of coordinates x', y', z', is given by the quadratures

$$\frac{\partial x'}{\partial u} = h'\frac{\partial x}{\partial u}, \qquad \frac{\partial x'}{\partial v} = l'\frac{\partial x}{\partial v}, \tag{2}$$

provided that h' and l' are functions of u and v subject to the conditions

$$\frac{\partial h}{\partial v} = a\,(l' - h'), \qquad \frac{\partial l'}{\partial u} = b\,(h' - l'). \tag{3}$$

Moreover, each pair of solutions of these equations leads by (2) to a net N', which is such that the tangents at corresponding points M and M' to the curves of the nets are parallel. All nets *parallel* to N are obtained in this way.

If θ_1 is any solution of (1), and θ_1' is the function given by

$$\frac{\partial \theta_1'}{\partial u} = h'\frac{\partial \theta_1}{\partial u}, \qquad \frac{\partial \theta_1'}{\partial v} = l'\frac{\partial \theta_1}{\partial v}, \tag{4}$$

then the functions $x_1^{(1)}$, $y_1^{(1)}$, $z_1^{(1)}$, defined by equations of the form

$$x_1^{(1)} = x - \frac{\theta_1}{\theta_1'}x' \tag{5}$$

are the coordinates of a net $N_1^{(1)}$, so related to N that the lines joining corresponding points M and $M_1^{(1)}$ of these nets form a congruence whose developables meet the surface on which these nets lie in the curves of the nets. We say that two nets so related geometrically are in the relation of a *transformation* T. Parallel nets are in such relation. We

have shown[1] that any transformation T of N into a non-parallel net is given by equations of the form (5).

Let N'' be a second net parallel to N, its coordinates being given by

$$\frac{\partial x''}{\partial u} = h'' \frac{\partial x}{\partial u}, \qquad \frac{\partial x''}{\partial v} = l'' \frac{\partial x}{\partial v}, \tag{6}$$

and let θ_1'' be defined by

$$\frac{\partial \theta_1''}{\partial u} = h'' \frac{\partial \theta_1}{\partial u}, \qquad \frac{\partial \theta_1''}{\partial v} = l'' \frac{\partial \theta_1}{\partial v}, \tag{7}$$

h'' and l'' being a pair of solutions of (3).

Then a second transform $N_1^{(2)}$ has coordinates of the form

$$x_1^{(2)} = x - \frac{\theta_1}{\theta_1''} x''. \tag{8}$$

Since the nets N' and N'' are parallel to one another, and the functions θ_1' and θ_1'' are solutions of the respective point equations for N' and N'' in a relation analogous to (4), a transformation T of N'' is given by

$$\overset{\prime\prime\prime}{x_1^{(1)}} = x'' - \frac{\theta_1''}{\theta_1'} x'. \tag{9}$$

We denote by $\overset{\prime\prime\prime}{N_1^{(1)}}$ the net with these coordinates. By differentiating the expressions (9), we show that the nets $N_1^{(1)}$ and $\overset{\prime\prime\prime}{N_1^{(1)}}$ are parallel. Moreover, it can be shown that the equations

$$x_1^{(2)} = x_1^{(1)} - \frac{\theta_1}{\theta_1''} \overset{\prime\prime\prime}{x_1^{(1)}} \tag{10}$$

are consistent with the above equations, and consequently $N_1^{(2)}$ is a T transform of $N_1^{(1)}$. Hence *if a net is transformed into two nets by means of the same function θ, the new nets are in the relation of a transformation T.* We say that three such nets form a *triad* under transformations T. It can be shown that the relation is entirely reciprocal in the sense that any two are obtainable from the third by transformations involving the same solution of the point equation of the third net. If in particular we take for θ_1 any of the coordinates of N, say z, the nets $N_1^{(1)}$ and $N_1^{(2)}$ lie in the plane $z = 0$. In other words, the developables of the two congruences, obtained by drawing through points of a net N lines parallel to the corresponding radii vectores of two nets parallel to N meet any plane in two nets in the relation of a transformation T. (We postpone to a later time a discussion of transformations of planar nets.)

If θ_1 is any solution of (1), a solution of the point equation of $N_1^{(1)}$ is given by

$$\theta_{12} = \theta_2 - \frac{\theta_1}{\theta_1'}\,\theta_2'. \tag{11}$$

This function and the net $\overset{\prime\prime\prime}{N}{}_1^{(1)}$ parallel to $N_1^{(1)}$ determine a transformation of the latter; moreover, the congruence of the transformation consists of the joins of corresponding points on $N_1^{(1)}$ and $N_1^{(2)}$. We call the transform N_{12} and its point coordinates x_{12}, y_{12}, z_{12}. The solution of the point equation of $\overset{\prime\prime\prime}{N}{}_1^{(1)}$ corresponding to θ_{12} is of the form

$$\theta_{12}''' = \theta_2'' - \frac{\theta_1''}{\theta_1'}\,\theta_2', \tag{12}$$

and consequently we have

$$x_{12} = x_1^{(1)} - \frac{\theta_2\theta_1' - \theta_1\theta_2'}{\theta_2''\theta_1' - \theta_1''\theta_2'}\,x_1^{(1)\prime\prime\prime}. \tag{13}$$

The function θ_2 can be used to determine with the nets N' and N'' two transforms of N, namely $N_2^{(1)}$ and $N_2^{(2)}$, whose coordinates are of the respective forms

$$x_2^{(1)} = x - \frac{\theta_2}{\theta_2'}\,x', \qquad x_2^{(2)} = x - \frac{\theta_2}{\theta_2''}\,x''. \tag{14}$$

Corresponding points of the nets N, $N_1^{(2)}$, and $N_2^{(2)}$ lie on a line, and $N_2^{(2)}$ is a transform of $N_1^{(2)}$ by means of the function

$$\theta_2 - \frac{\theta_1}{\theta_1''}\,\theta_2''. \tag{15}$$

Likewise, corresponding points of the nets $N_1^{(1)}$, $N_1^{(2)}$, N_{12} lie on a line, and N_{12} is a transform of $N_1^{(2)}$ by means of the function

$$\theta_{12} - \frac{\theta_1\theta_{12}''}{\theta_1''}. \tag{16}$$

By means of (11) and (12) we show that the expressions (15) and (16) are equal, and consequently the nets $N_1^{(2)}$, $N_2^{(2)}$, N_{12} form a triad.

Equation (13) is reducible to

$$x_{12} = x + [(\theta_1''\,\theta_2 - \theta_2''\theta_1)x' + (\theta_2^1\theta_1 - \theta_1'\theta_2)x'']/(\theta_1'\theta_2'' - \theta_1''\theta_2') \tag{17}$$

From the symmetry of this expression we see that N and N_{32} are transforms of $N_1^{(1)}$ and $N_2^{(2)}$ in an analogous manner. We say that the

four nets form a *quatern* $(N, N_1^{(1)}, N_2^{(2)}, N_{12})$. This result constitutes a generalization of the theorem of permutability of transformations D_m of isothermic surfaces as established by Bianchi[2]. In like manner we have the quaterns $(N, N_1^{(2)}, N_2^{(1)}, N_{12})$ and $(N_2^{(1)}, N_1^{(1)}, N_2^{(2)}, N_1^{(2)})$. Moreover the six nets can be associated into the four triads $N, N_1^{(1)}, N_1^{(2)}$; $N, N_2^{(1)}, N_2^{(2)}$; $N_1^{(2)}, N_2^{(2)}, N_{12}$; $N_2^{(1)}, N_1^{(2)}, N_{12}$.

When the nets $N_1^{(1)}$ and $N_2^{(2)}$ have been found, the functions θ_1' and θ' are determined to within additive constants. Hence, *if $N_1^{(1)}$ and $N_2^{(2)}$ are two transforms of N, there exist ∞^2 nets N_{12}, each of which forms a quatern with N, $N_1^{(1)}$ and $N_2^{(2)}$; and their determination requires two quadratures.*

The six corresponding points of the nets are the vertices of a complete quadrilateral whose four sides are generic lines of the four congruences which figure in the transformations. On each of these lines there are two focal points, each being the point of contact of the line with the edge of regression of one or other of the two developables as u or v varies. The four points corresponding to the variation of either variable lie on a line, and these two lines are the tangents to the parametric curves on the envelope of the plane of the quadrilateral; moreover, these curves form a net.

Thus far we have used rectangular non-homogenous point coordinates, but in some cases it is advisable to make use of general homogenous coordinates. The four homogenous coordinates x, y, z, w, of a net satisfy an equation of the form.

$$\frac{\partial^2\theta}{\partial u \partial v} = a\frac{\partial\theta}{\partial u} + b\frac{\partial\theta}{\partial v} + c\theta. \tag{18}$$

When two nets N and N_1 are in the relation of a transformation T, the tangents to the curves $v =$ const. at corresponding points M and M_1 of the net meet in a point F_1. Likewise the tangents at M and M_1 to the curves $u =$ const. meet in a point F_2. It is readily seen that as v varies, any point on the tangent to a curve $v =$ const. of a net moves in such a way that the tangent to its path lies in the tangent plane of the net. Similarly for a point on the tangent to $u =$ const., as u varies. Since the line F_1F_2 lies in the tangent planes to both N and N_1, it is tangent to the motion of F_1 as v varies, and to the motion of F_2 as u varies. Hence F_1 and F_2 are the focal points of the congruence of lines of intersection of the planes of the nets. Following Guichard, we say that a congruence whose focal points lie on the tangents to the curves of a net and whose developables correspond to the curves of the net is *harmonic* to the net. It can readily be shown that the homogenous coordinates of a net can

be chosen so that the coordinates of the focal points of a harmonic congruence are of the respective forms

$$\frac{\partial x}{\partial u}, \frac{\partial x}{\partial v}. \tag{19}$$

In this case equation (18) assumes the form (1), so that the choice of coordinates referred to its equivalent to finding a particular solution of (18).

Since the congruence F_1F_2 is harmonic to both N and N_1, it follows that the equations of any transformation T in homogenous coordinates is reducible to the form (2). If the coordinates of N satisfy (18), the equations are of the form

$$\frac{\partial x_1}{\partial u} = h\frac{\partial}{\partial u}\left(\frac{x}{\theta}\right), \qquad \frac{\partial x_1}{\partial v} = l\frac{\partial}{\partial v}\left(\frac{x}{\theta}\right), \tag{20}$$

where now θ is a solution of (18).

When the equations of the transformation are of the form (2), each solution of the point equation of N gives a new transform by means of (5). The equations of the preceding results continue to be true, and parallel nets are replaced by any transforms.

Although these results have been stated in terms of 3-space, they hold for two dimensional spreads in n-space, provided that a congruence is defined as a two parameter family of lines possessing two families of developables.

[1] Eisenhart, *Trans. Amer. Math. Soc., New York*, 18, 1917, (97–124).
[2] Bianchi, *Ann. Mat., Milano*, (Ser. 3), 11, 1905, (93–158).

THE MOLECULAR WEIGHTS OF THE TRIARYLMETHYLS

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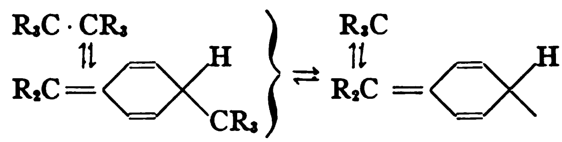
Communicated May 31, 1917

It is now generally accepted that the free radicals of the triphenylmethane series owe their unique unsaturated character to the presence of a trivalent carbon atom in the molecule. In many cases the molecular weight has been found to be double that calculated for the free radical. Nevertheless, even in these cases the presence of a compound with a single unsaturated carbon atom is still recognized, and the assumption is made that there exists, in virtue of partial dissociation, a mobile equilibrium:

$$R_3C-CR_3 \rightleftarrows R_3C+R_3C.$$

It is doubtful whether the matter is quite as simple as it is represented by the above equation. From a variety of experimental evidence[1] the conclusion seems inevitable that both the hexa-arylethanes and the triarylmethyls exist in two tautomeric modifications. It is far more likely that triarylmethyls, at least when in solution, conform to the following scheme:[2]

(I) (II)



(III) (IV)

In other words, there is a tautomeric equilibrium between the benzenoid hexa-arylethane (I) and the quinol (II); also between the benzenoid triarylmethyl (III) wherein the central carbon atom is trivalent and its quinonoid modification (IV) wherein the *para*-carbon atom in the nucleus assumes the trivalent state. This viewpoint is in entire harmony with the peculiar chemical and physical behavior of this class of compounds. It permits us to explain, by the presence of the quinol tautomer (II), the existence of a colored modification of hexa-arylethanes in cases where molecular weight determinations fail to indicate an actually measurable degree of dissociation. And even in those cases where a partial or complete dissociation of the hexa-arylethane is demonstrable, the fact that the solution is colored finds a more reasonable and more concordant explanation in the existence of the quinonoid tautomeric triarylmethyl (IV) than merely in that of the benzenoid (III) alone.[3]

Whether, however, the supplemental hypothesis of tautomerism is accepted or not, the fact remains that we are dealing primarily with a phenomenon where a hydrocarbon dissociates spontaneously into two parts, which parts may, also spontaneously, reassociate with the production of the original substance. What are the factors which influence the degree of dissociation in a given hexa-arylethane? What is the relation between the degree of the dissociation and the nature of the aryl group in the substituted ethane?

Factors Influencing Dissociation.—Obviously, the nature of the solvent, the temperature of the solution, and the concentration of the solute must be the chief factors as regards the extent of dissociation which may be reached by a given hexa-arylethane. With regard to changes in the equilibrium between the dimolecular and the monomolecular modification due to the temperature, it has been found[4] that the molecular weight of triphenylmethyl in naphthalene at 79°–80° is approximately 414, while the average value found with other solvents such

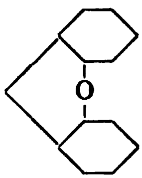
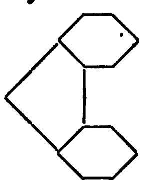
as benzene, nitrobenzene, etc. which freeze at temperatures around 0°–5°, is between 480 and 485. A computation from these molecular weight determinations indicates that triphenylmethyl exists in the monomolecular state to an extent of 17% in naphthalene at 80°, while to considerably less than 5%, if at all, in benzene at 6°. Schlenk and Mair[5] determined the molecular weight of triphenylmethyl in benzene by the ebullioscopic method, and found that at the temperature of the boiling point of benzene, also about 80°, the hydrocarbon is dissociated in the monomolecular phase to the extent of 23.3–29.9% with approximately a 2 % concentration. Schmidlin[6] calls attention to the fact that it is very difficult to obtain an absolutely pure sample of triphenylmethyl and that samples prepared by slightly different methods often differ considerably in purity. Therefore he suggests the possibility that the variation between the molecular weights found by Schlenk with the ebullioscopic method and by Gomberg and Cone with the cryoscopic method may be due to differences in the degree of purity of the samples used. Accordingly, he constructed a special apparatus containing two thermometers, by means of which he could determine the molecular weight in benzene first by the ebullioscopic method and then by the cryoscopic method on one and the same sample. However, the molecular weights of triphenylmethyl found by the two methods showed no appreciable difference in value, and therefore no change in dissociation between the temperatures of 6° and 80° was noticeable.

With regard to the influence of concentration upon the degree of dissociation of hexaphenylethane, an examination of the results obtained by various investigators fails to reveal a concordant influence of this factor. Neither molecular weight determinations nor the application of optical methods[7] have supplied information of a sufficiently decisive nature to warrant us in drawing any conclusion as regards the concentration influences affecting the equilibrium between the dimolecular and the monomolecular triarylmethyl.

Relation Between Dissociation and the Nature of the Aryl Groups.—Although triphenylmethyl, the first and simplest representative of the triarylmethyls, was found to be largely in the dimolecular state, some of its analogs were found to exist to a large extent as monomolecular. Among the first triarylmethyls of this kind was the series containing *p*-biphenyl groups:

$$(C_6H_5)_2C\cdot(C_6H_4\cdot C_6H_5),\quad (C_6H_5)C(C_6H_4\cdot C_6H_5)_2,\quad C(C_6H_4\cdot C_6H_5)_3.$$

Schlenk and his co-workers concluded from the measurements of the molecular weight by the cryoscopic method with benzene as a solvent,

that these compounds are monomolecular to the extent of 15%, 80%, and 100%, respectively.[8] From these results one might infer that the dissociation of the hexa-arylethane into free radicals is greatly favored by the complexity or the weight of the aryl groups,—the dissociation becoming apparently more manifest also in proportion to the number of such groups. But the hypothesis that the dissociation of the hexa-arylethanes is proportional to the complexity of the aryl groups becomes wholly untenable when one compares triphenylmethyl with phenyl-xanthyl, C_6H_5C , which is monomolecular to the extent of 82%.[9] Is the union of the two phenyl groups the paramount influence in this case? If so, why should a substance constitutionally so closely related to the xanthyls as phenyl-biphenylene-methyl, $C_6H_5 \cdot C$ , be completely dimolecular?[10]

It is obvious that from the few facts known to us at present it is difficult, if not wholly impossible, to formulate the relation between the complexity of the aryl groups in the hexa-arylethanes on the one hand and the tendency of these ethanes toward dissociation into free radicals on the other.

The Problem of this Investigation.—In order to obtain some further knowledge as to the factors governing the equilibrium between the di- and the mono-molecular phases of triarylmethyls, we decided to select for our study a related set of compounds. The following triarylmethyls were prepared for this purpose:

1. $(C_6H_5)_3C$ .... Triphenylmethyl.
2. $(C_6H_5)_2C \cdot C_{10}H_7$ .... α-Naphthyl-diphenylmethyl.
3. $C_6H_5 \cdot C{<}^{C_6H_4}_{C_6H_4}{>}O$ .... Phenyl-xanthyl.
4. $CH_3C_6H_4 \cdot C{<}^{C_6H_4}_{C_6H_4}{>}O$ .... *p*-Tolyl-xanthyl.
5. $ClC_6H_4 \cdot C{<}^{C_6H_4}_{C_6H_4}{>}O$ .... *p*-Chlorphenyl-xanthyl.

6. $C_{10}H_7 \cdot C \begin{matrix} \diagup C_6H_4 \diagdown \\ \diagdown C_6H_4 \diagup \end{matrix} O$ .... α-Naphthyl-xanthyl.

7. $C_6H_5 \cdot C \begin{matrix} \diagup C_6H_4 \diagdown \\ \diagdown C_{10}H_6 \diagup \end{matrix} O$ .... Phenyl-pheno-β-naphtho-xanthyl.

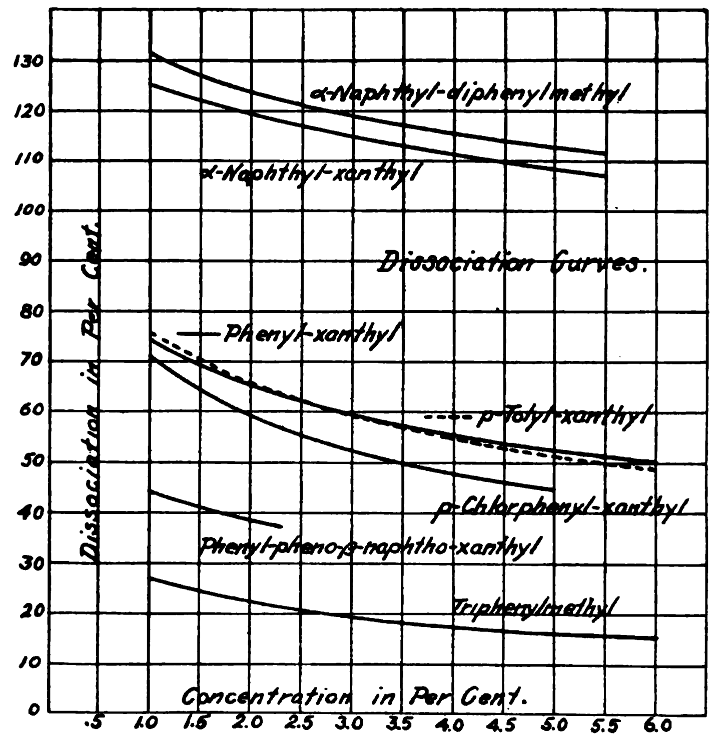
A determination of the molecular state, and consequently of the dissociation phenomenon, of these unsaturated compounds is the more pertinent in view of the limited number of radicals upon which such determinations have been conducted in the past. Further and, if possible, more certain evidence with respect to the dissociation of these hydrocarbons, and consequently with respect to the trivalency of carbon, is most desirable.

Our aim has been to take every possible precaution to insure uniformity and purity of the triarylmethyls to be used. As a check, the capacity for oxygen absorption and for peroxide formation was always resorted to, using part of the very same sample which served for the determination of the molecular weight. This is a very essential and one of the safest criterions for the purity of the products, the reaction being: $2\ R_3C + O_2 = R_3C-O-O-R_3C$. Also it was made certain that during the course of the determination the triarylmethyl suffered no deleterious isomerization due to the effect of the temperature (80°) of the solvent, naphthalene. Each experiment represents an individual preparation and in no case were two determinations of the molecular weight made upon the same sample. Moreover, the sample was never more than one day old, the free radical being prepared in the afternoon, allowed to crystallize over night, isolated the following morning and the molecular weight taken the same afternoon. The molecular weight determinations themselves were carried out in an atmosphere of hydrogen, oxygen being completely excluded from the whole apparatus. The details regarding the preparation of the pure compounds and the method used in the molecular weight determinations will appear shortly in the *Journal of the American Chemical Society*.

The triarylmethyls were studied from two points of view. (1) The molecular weight of every triarylmethyl was determined within a fairly wide range of concentration, from 1% to 6%, in order to determine the effect of concentration upon the degree of dissociation. (2) From a comparison of the degree of dissociation of various trarylmethyls, under the same conditions of solvent, concentration and temperature, it was hoped that something would be learned regarding the

influence of the various aryl groups upon the tendency of the ethane towards dissociation.

Summary of the Results.—In the diagram are represented the curves, each curve based upon a number of molecular weight determinations, expressing the dissociation tendency of the hexa-arylethanes mentioned in this paper. The curves are plotted with the percentage of concentration as abscissas and the dissociation values, in percentages, as ordinates. The values expressing dissociation were



obtained according to the formula $x = \frac{M_t\text{-}M_0}{M_0}$, where x equals the degree of dissociation, M_t represents the theoretical molecular weight of the hexa-arylethane and M_0 the molecular weight actually found.

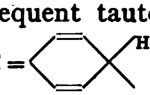
1. In all instances, a steady and gradual increase of the molecular weight is noticeable as one proceeds from a 1% concentration of the radical to that of 6%. The generality of these results, and their uniformity, leave no room for doubt that we are dealing here with a phenomenon of molecular dissociation, wherein the products of dissociation are in equilibrium with the dissociating substance. An

equilibrium of this nature might be expected to shift in favor of dissociation with dilution, as is actually the case with these compounds.

2. When in triphenylmethyl two phenyl groups become joined through an oxygen atom and thus give rise to a xanthone ring, the tendency of the new compound, phenyl-xanthyl, towards dissociation increases to a marked extent, approximately threefold.

3. A phenyl and a *p*-tolyl group are apparently equivalent in their influence for dissociation when linked to a xanthone ring; a *p*-chlorphenyl group is of somewhat lesser influence, as can be seen on comparing the dissociation curves of the three corresponding aryl-xanthyls. This equality of influence of a *p*-tolyl and a phenyl group would not necessarily have been anticipated. Tolyl-diphenylmethyl differs very much from triphenylmethyl, and tri-*p*-tolylmethyl is so unstable that it is apparently impossible to isolate it, owing to the tendency to isomerize or polymerize.

4. An α-naphthyl group when replacing a phenyl group in triphenylmethyl exerts upon the dissociation equilibrium of the compound a very decided influence in favor of the monomolecular phase—α-naphthyl-diphenylmethyl appearing as wholly dissociated. This favorable influence of the naphthyl group is still retained when the group is linked to a xanthone ring, the resulting compound being also dissociated to the extent of 100%. And yet, when the naphthyl group enters as a component in the formation of the xanthone ring itself, it depresses very decidedly the dissociation tendency of the compound, as is evident on comparing the two isomers, α-naphthyl-xanthyl and phenyl-pheno-β-naphtho-xanthyl.

5. It has been established that the triarylmethyls are, in the solid state, almost wholly devoid of color, being, like triphenylmethyl itself, only pale-yellow; but their solutions differ very much, being yellow, orange, brown, red, or green, according to the individual compound. Schlenk has described tribiphenylmethyl, 100% dissociated, as a dark green powder. Whether this difference in color from our completely dissociable radicals is real or only apparent we cannot at present say. The fact that triarylmethyls, colorless when solid, give color only when in solution, lends support to the hypothesis that the triarylmethyls do undergo tautomerization when dissolved. Not dissociation alone into triarylmethyl, but, in addition thereto, the consequent tautomerization of this into its quinonoid modification, $R_3C=$ , constitute a satisfactory explanation of the color phenomenon. Tautomerization commonly occurs, indeed,

only when the compound capable of tautomerization is in the liquid phase, or in solution. And that is just the case with the free radicals under consideration.

6. The two radicals, α-naphthyl-diphenylmethyl and α-naphthyl-xanthyl, were found to give a molecular weight *less* than that calculated for the monomolecular phase. Schlenk and Renning's[11] results show that α-naphthyl-biphenyl-phenylmethyl exhibits the same unusual behavior. We have good reasons to believe that our results are not due to experimental errors, and we took pains to verify them repeatedly. The cause of this unexpected result may possibly lie in a still further dissociation of the triarylmethyl itself, $R\,R'\,R''\,C \rightleftarrows R\,R'C + R''$.

[1] Gomberg, *Berlin, Ber. D. Chem. Ges.*, **40**, 1907, (1860); **42**, 1909, (406); Gomberg and Cone, *Liebigs Ann. Chem. Leipzig.* **370**, 1909, (190); **376**, 1910, (208).

[2] Gomberg, *Berlin, Ber. D. Chem. Ges.* (406); **46**, 1913, (228).

[3] Compare G. N. Lewis, these PROCEEDINGS, **2**, 1916,(588).

[4] Gomberg and Cone, *Berlin, Ber. D. Chem. Ges.*, **37**, 1904, (2037).

[5] Schlenk and Mair, *Liebigs Ann. Chem., Leipzig*, **394**, 1912, (179).

[6] Schmidlin, *Das Triphenylemethyl*, Stuttgart, 1914, (94).

[7] Piccard, *Liebigs Ann. Chem., Leipzig*, **381**, 1911, (347).

[8] Schlenk, *Ibid.*, **372**, 1909, (4); **394**, 1912, (186); *Berlin, Ber. D. chem. Ges.*, **43**, 1910, (1756); Schmidlin and Garcia-Banus, *Ibid.*, **45**, 1912, (3176).

[9] Schlenk and Renning, *Liebigs, Ann. Chem, Leipzig*, 394, 1912, (189).

[10] Schlenk, Herzenstein and Weickel, *Berlin, Ber. D. chem. Ges.*, **43**, 1910, (1754).

[11] Schlenk and Renning, *Liebigs Ann. Chem., Leipzig*, **394**, 1912, (195).

SEX-DETERMINATION AND SEX-DIFFERENTIATION IN MAMMALS

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Read before the Academy, April 17, 1917

The principle of zygotic sex-determination is generally regarded as established for mammals as for other animal groups. The reasons for this are (1) the identity of sex of all individuals derived from a single zygote: e.g., identical twins, quadruplets of armadillos, etc.; (2) the facts of sex-linked inheritance, which demonstrate the inheritance of certain sex factors in a Mendelian way; (3) the dimorphism of spermatozoa in mammals as in other groups with zygotic determination of sex. We must therefore regard sex as *determined*, in the usual sense of the word, at the time of union of the gametes.

The question, however, arises, whether sex-*determination* involves an irreversible tendency to the corresponding sex-*differentiation*, or

whether such differentiation is more or less controllable, or even completely reversible?

Up to a certain stage in the development of mammals there are no morphological evidences of the determined sex. Prior to this stage sex-characters are identical in both kinds of zygotes; the gonad first enters on a phase of male differentiation in both sexes, which subsequently changes to the female direction in the female zygotes only; both male and female sex-ducts arise in each kind of zygote, and by subsequent corelative progressive and retrogressive differentiation the conditions of the appropriate sex are produced; external parts also appear similarly at first in both sexes. The possibilities for complete reversal of the indicated sex-differentiation would therefore seem to lie within this so-called sexually indifferent stage, and to diminish progressively as differentiation proceeds.

There are many indi ations that each zygote, whether determined as male or female, has both tendencies, both sets of sex-factors; in other words that the reactions for male or for female differentiation are both possible for each zygote; but that they are to a considerable extent mutually exclusive in bisexual animals. The initial sex-determination is, therefore, a condition in which there is a quantitative superiority of one or the other tendency or set of factors. The advance of development progressively limits the possible operations of the inferior set of factors, so that, by both positive and negative limitations, reversal of the initial sex-index becomes increasingly more difficult.

It has long been known that the degree of development of the sex-characters that arise after birth is dependent in mammals upon internal secretions of the sex-glands (sex-hormones) circulating in the blood. This is seen in the well-known effects of castration; and partial reversal of sex-differentiation has been secured by implantation of the sex-gland of the opposite sex following castration.[1] But the more fundamental sex-characters, like other fundamental characters, are differentiated in mammals before birth. Such are the type of sex-gland, whether ovary or testis, the type of sex-ducts, whether vasa deferentia or female reproductive tract, and the type of the external organs of reproduction.

The problem of the extent to which sex-differentiation may be reversible carries us back therefore to the sexually indifferent stage, and the question arises whether the sex-characters that develop before birth are, like those arising after, dependent for the degree of their development upon sex-hormones, and whether, like the latter also, they are more or less reversible by action of the sex-hormones of the opposite sex?

The investigation of a remarkable phenomenon in twins of cattle furnishes a positive answer to this question so far as the female is concerned.

The female of two-sexed twins in cattle, commonly known as the free-martin, has long been known to be absolutely sterile as a general rule; however a small percentage of such females is perfectly normal. I have found by a study of the embryonic development that the phenomenon of sterility is due to fusion of the embryonic membranes of the twins, and an anastomosis of the arteries and veins of the female and male associates, but more especially of the arteries, so that there is literal community of blood during foetal life.[2] If the anastomosis of the blood-vessels does not take place, the female is perfectly normal as is usual in the twins or multiple births of all other mammals. In cattle again if the twins be of the same sex both are perfectly normal.

Nature has thus performed here a perfectly controlled experiment, which shows that blood community of foetal life between embryos of different sex causes sterility of the female. This fact can be explained only on the assumption that the foetal blood carries specific sex-hormones, because the only system of the female that is affected is the reproductive system. The male, on the other hand, is normal in all its parts, and this finds explanation in the fact that the sexual differentiation of the male antedates by a little that of the female, and the development of female sex-hormones is probably inhibited from the start.

The time at which the anastomosis of the blood vessels occurs is a question of fundamental importance. Sex-differentiation begins in cattle when the embryo is about 25 mm. long. The evidence at my command indicates strongly that fusion of the embryonic membranes begins at about this time or a little earlier; in a pair of twins 15 mm. long the membranes overlapped but were not yet fused; in another pair, of which one member was 35 mm. and the other 40 mm. in length, the membranes were perfectly fused so that the place of union could no longer be detected. The vascular areas of the two sides overlapped, but the larger vessels did not anastomose; a capillary connection at least between the two sides certainly existed. In another pair 50 mm. long the fusion was perfect and the anastomosis of the blood-vessels also. Study of normal embryos also shows that the conditions precedent to fusion are fully established before the 20 mm. stage; and study of the gonads of the free-martin shows that the development of the ovarian cortex is probably inhibited from the beginning, i.e., from about the 25 mm. stage. There can be no doubt that the blood community dates from about the time of the sexually indifferent stage, but it certainly varies more or less

both with reference to the time of origin and the extent of the vascular anastomosis.

We have hitherto noted only that the free-martin is sterile whenever blood-community with its male twin exists during foetal life, i.e., in about seven-eighths of all cases, and that otherwise it is normal. What is the nature of this sterility? Observers from the time of John Hunter[3] (1786), who have studied the anatomy of free-martins, have all noted the intersexual character of its reproductive system—the internal organs of reproduction are largely male in type, the external female—and the later students of the free-martin have generally regarded it as a modified male on account of the character of the internal organs and for other reasons (Spiegelberg,[4] D. Berry Hart,[5] Bateson,[6] Cole[7]). The gonad is sometimes absent or exceedingly rudimentary, but when well developed it never exhibits any trace of ovarian cortex, and its structure is testis-like, though germ-cells are not formed. The sexual ducts show reduction or absence of the female parts and a graded series of development of the vas deferens. The external parts are usually typically female. We have on the one hand, therefore, failure of development of the internal female reproductive organs, and, on the other hand, the male parts, which usually degenerate, undergo development. Anatomically the free-martin is definitely intersexual, to a variable extent as will be seen.

The writer has studied the anatomy of 22 foetal free-martins ranging in size from 7.5 to 28 cm. The striking results of this examination were (1) The gonads remain rudimentary in size during this period; (2) the female ducts fail to develop; they frequently remain in part as undeveloped rudiments, but in other cases disappear as completely as in the male. (3) The male ducts develop in varying degrees, always more than in the normal female, though rarely to the same extent as in the male. (4) Gubernacula invariably developed in the free-martins and formed peritoneal evaginations exactly as in males. (5) In one of the oldest foetuses the gonads had entered the saccus vaginalis as in the male. (6) In my material the external organs were always typically female.

Miss Chapin[8] made a histological investigation of the gonads of the foetal free-martin under my direction and determined that the ovarian cortex never forms, but that a quite typical albuginea develops over the surface as in the testis of the male. The medullary cords, homologue of the seminiferous tubules of the male, also underwent unusually great development.

A seven weeks old free-martin examined by the writer possessed testes

and vasa deferentia, but no trace of uterus or vagina. The testes had descended and lay beneath the skin in the region of the groin. No scrotum was formed and the external parts were typically female. A free-martin described by Numan in 1844 was even farther transformed in the male direction, and the external parts were also modified.

The various cases can be arranged in a series of increasing male-likeness, but the transformation of the female zygote owing to action of the male sex-hormones does not in the case of the free-martin, in the material at our command, proceed all the way to the normal male condition. It is perhaps worth noting that, if it ever did, we would be unable to detect it, except on a basis of much larger statistics than we possess. But the rarity of the more extreme detectable cases makes it seem very improbable that other cases jump all the way across the gap to the normal male.

It follows from the data that the female zygote must contain factors for both sexes; the primary determination of the female sex must therefore be due to dominance of the female factors over the male. If we think of this as a simple quantitative relation, as Goldschmidt[9] (1916) has done, we can explain the intersexual condition of the free-martin as due to an acceleration or intensification of the male factors of the female zygote by the male hormones. The degree of the effect which is quite variable, as we have seen, would of course be subject to all quantitative variations of the hormone. Thus the case of the free-martin could come under the same general point of view as that of the intersexes of Lymantria according to Goldschmidt with the one exception that the quantitative differences between the male and female factors of the female zygote necessary for the differentiation of female characters, are reduced in the free-martin by internal secretions instead of by variations of potency of the male factors in different varieties as in the intersexual hybrids of Lymantria.

The case of the free-martin shows that a gonad with a primary female determination may form a structure which is morphologically a testis, (cf. Chapin 1917) through suppression of the cortex and over development of the medullary cords and urinogenital union under the influence of male sex-hormones. Lesser degrees of transformation are of course possible, so that it is certain that the gonad of a mammalian female zygote is capable of most, at least, of the series of transformations that may exist between an ovary and a testis. Whether the transformation in the male direction may proceed under such conditions to the production of true spermatocytes and spermatozoa is at least doubtful. Such elements have not hitherto been described for free-martins, if we

except D. Berry Hart's statement concerning the gonads of Hunter free-martins, that "in only one are spermatozoa present." More than six words seem necessary to establish so important an exception.

Regarding other parts of the internal reproductive system we have seen that the free-martins exhibit a graded series of inhibition of the female ducts and of development of the male ducts which may obviously correspond to variable time of onset, intensity, and perhaps duration of action, on the male sex-hormones. The series extends nearly to the normal male limit in exceptional cases. There is indicated a rough parallelism at least between the grade of transformation of the gonad and that of the remainder of the internal reproductive system. The external organs of reproduction are the least liable to modification, but they do not escape in all cases, and may even exhibit considerable transformation in the male direction, if we can accept Numan's case.

The fundamental determining factor in these events is undoubtedly the male sex-hormones as has been argued previously, but the entire causal nexus is by no means clear. We do not know what the results of embryonic castration of the female might be in itself, and hence we are unable to assert definitely in just what positive ways the male hormones act on the female zygote, because the earliest determinable result of such action is the suppression of the ovarian cortex, which must be regarded as practically equivalent to castration. This action at least is due to the male hormones; how much of the subsequent events is due to mere absence of ovarian tissue, and how much to positive action of male sex-hormones is more or less problematical. It is well known that spayed females of certain birds and mammals tend to develop male characters; heifers with cystic degeneration of the ovary also develop certain male characteristics (Pearl and Surface, 1915),[10] so that we must admit in principle the possibility that much of the male development in the free-martin is due to the lack of inhibitions normally furnished by the ovary.

It is also probable that the various parts of the reproductive system have other means of correlation, and act and react on one another in various ways. Certain indications of this are seen in lateral variations, as for instance in one of my cases where a large gonad on one side is associated with a large Wolffian duct, and seminal vesicle, and a much smaller one on the other side with a correspondingly smaller duct and vesicle.

When, therefore, we attribute the free-martin condition to the male hormones we only mean to assert that they are the primary cause, and not that they are the decisive factors in each member of the series of events.

The possibility exists, however, that definitely planned experiments may enable us to regulate time and dosage of hormones better than is done in this experiment of nature; the results of such experiments cannot of course be foreseen. Nor can it be predicted in advance what the results of the inverse experiment might prove to be, i.e., treatment of the male zygote from the beginning of six-differentiation with female hormones. Such experiments will be necessary for the full solution of the stated problem. We can, however, state, confidently on the basis of the present results that sex-determination in mammals is not irreversible predestination, and that with known methods and principles of physiology we can investigate the possible range of reversibility.[11]

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THE CRYSTAL STRUCTURE OF MAGNESIUM

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Communicated by R. A. Millikan, May 29, 1917

Magnesium is assigned by crystallographers to the holohedral class of the hexagonal system, with axial ratio 1.624. The structure given below agrees with this symmetry. The arrangement of atoms is that of hexagonal close packing, the arrangement which equal hard spheres assume when closely packed, except that the structure is shortened by about one-half of one percent in the direction of the hexagonal axis. This is the fourth type of arrangement of atoms in elementary substances thus far observed, viz.: the diamond type, characteristic of diamond, silicon, bismuth and antimony, where each atom is surrounded by four equidistant nearest neighbors; the centered cubic lattice, characteristic of iron, and probably also of nickel and the alkali metals, where each atom is surrounded by eight equidistant nearest neighbors; the face-centered cubic lattice, or closest-packed cubic arrangement,

of which aluminum, copper, silver, and probably gold and lead are examples, where each atom has twelve equidistant nearest neighbors; and the closest-packed hexagonal arrangement described below, of which magnesium is at present the only example, where the number of equidistant nearest neighbors is also twelve, but in a slightly different arrangement from those of the face-centered cubic lattice.

The X-ray analysis was made in two parts. First, single small crystals were mounted with definite orientation on the spectrometer table, and photographed while slowly rotated and exposed to a monochromatic beam of X-rays. This gave the approximate structure. A picture was then taken of magnesium powder, in the manner described in a previous paper (*Phys. Rev.* 9, 85, Jan. 1917), which checked and confirmed the results of the first method.

Three small samples, formed by vacuum distillation, were used for the single photographs. The first was mounted with its basal plane (0001) parallel to the rays, and was rotated about the axis (0001) – $(10\bar{1}0)$ for about 30° on each side of the center. This should give reflection from (0001) and the flatter pyramids $(10\bar{1}3)$, $(10\bar{1}2)$, $(10\bar{1}1)$, etc. The second was mounted so as to rotate about the same axis, but with $10\bar{1}0$ parallel to the rays at the center position. This should give reflection from $(10\bar{1}0)$ and the steeper pyramids $(30\bar{3}1)$, $(20\bar{2}1)$, $(10\bar{1}1)$, etc. The third was rotated about the axis (0001), (1120), with rays parallel to $11\bar{2}0$ at center, so as to give reflection from $(11\bar{2}0)$, $(11\bar{2}1)$, $(11\bar{2}2)$, etc.

The observed lines and spacings are given in table 1.

TABLE 1

Crystal 1			Crystal 2			Crystal 3		
Position of line	Spacing of plane	Plane	Position of line	Spacing of plane	Plane	Position of line	Spacing of plane	Plane
3.10	2.59	0001	2.90	2.75	$10\bar{1}0$	5.02	1.60	$11\bar{2}0$
2.90	2.75	$10\bar{1}0$	3.10	2.59	0001	5.50	1.48	$11\bar{2}1$
3.30	2.44	$10\bar{1}1$	3.30	2.44	$10\bar{1}1$	6.0	1.36	$11\bar{2}2$
4.20	1.90	$10\bar{1}2$	6.05	1.34	$20\bar{2}1$			
5.50	1.48	$10\bar{1}3$	8.92	0.92	$10\bar{1}3(3)$			
6.27	1.30	$20\bar{2}1$	5.9	1.38	$10\bar{1}0(2)$			

The first column gives for each crystal the distance of the observed line from the center, the second the spacing of the corresponding plane, as calculated from this distance, and the third the indices of the plane.

A triangular prism having the spacing $d_{11\bar{2}0} = 1.61$ A and axial ratio 1.624 would have a height $1.624 \times 2d_{11\bar{2}0} = 5.23$ A, which is exactly twice the spacing of the (0001) planes found above, and suggests that the lattice is composed of *two* sets of triangular prisms each of side 3.22A

and height 5.23A, the atoms of either set being in the center of the prisms of the other. The number n of atoms per unit prism is, if ρ is the density, M the mass of an atom of Mg, and a and h represent the side and height of the prism, respectively:

$$n = \frac{\sqrt{3}}{4}\,\frac{a^2h\rho}{M} = 1.03$$

which is equal to 1 within the limit of accuracy of a, h, and ρ, and is correct for the assumed structure.

The second step in the analysis was to check this assumed structure by a photograph taken through finely powdered magnesium, which should show all the lines required by the assumed structure ***and no more.*** Table 2 gives the position of the observed lines and the corresponding spacing

TABLE 2

DISTANCE OF LINE FROM CENTER	ANGLE OF REFLECTION	INTENSITY (ESTIMATED)	SPACING OF PLANE IN ANGSTROMS		INDICES OF PLANE
			Experimental	Theoretical	
cm.					
2.92	7.40°	70.	2.75	2.75	$10\bar{1}0$
				2.59	0001
3.30	8.33	150.0	2.44	2.44	$10\bar{1}1$
4.23	10.74	50.0	1.91	1.90	$10\bar{1}2$
5.03	12.75	70.0	1.61	1.60	$11\bar{2}0$
5.50	13.90	60.0	1.48	1.48	$10\bar{1}3$
				1.38	$10\bar{1}0$ (2)
5.95	15.00	60.0	1.36	1.36	$11\bar{2}2$
6.05	15.30	20.0	1.34	1.34	$20\bar{2}1$
				1.30	0001 (2)
6.65	16.80	10.0	1.23	1.23	$10\bar{1}1$ (2)
6.90	17.40	0.5	1.18	1.18	1014
7.52	19.00	15.0	1.09	1.08	$20\bar{2}3$
7.96	20.10	30.0	1.04	1.05	$21\bar{3}0$
8.10	20.50	0.5	1.02	1.03	$21\bar{3}1$
				1.01	$11\bar{2}4$
8.41	21.3	20.0	0.98	0.97	$21\bar{3}2$
					$10\bar{1}5$
				0.94	$10\bar{1}2$ (2)
8.83	22.4	0.3	0.93	0.92	$10\bar{1}0$ (3)
9.15	23.1	15.0	0.90	0.89	$21\bar{3}3$
9.48	24.0	10.0	0.87	0.87	$30\bar{3}2$
					0001 (3)
9.90	25.1	0.5	0.83	0.83	$10\bar{1}6$
					$20\bar{2}5$
				0.82	$10\bar{1}1$ (3)
					$21\bar{3}4$
				0.80	$11\bar{2}0$ (2)
10.95	27.7	10.0	0.77	0.77	3140

of the planes, together with the theoretical spacing for the lattice described above. The agreement is within the limit of error of the measurements, except that some of the predicted lines are too faint to show. This is to be accounted for by the distribution of electrons in the atoms, and will be discussed in a future paper.

THE STRUCTURE OF HIGH-STANDING ATOLLS

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Communicated June 5, 1917

The structure of high-standing atolls has seldom been studied in detail, and is perhaps seldom sufficiently revealed for close study. Attention is therefore drawn here to only one structural feature, namely the relation of atoll limestones to their supposed foundation of volcanic rocks. According to Darwin's theory of intermittent subsidence, the limestones of atolls should lie unconformably on an unevenly eroded, submountainous volcanic mass, the top of which may be buried to any depth, as in section M of sector L, figure 1: the section of the volcanic

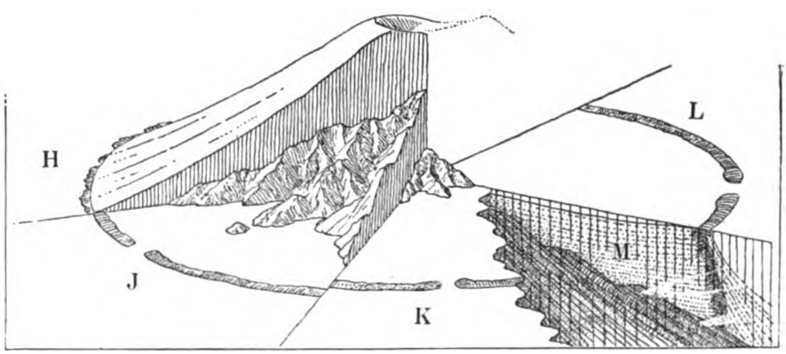

FIG. 1.

foundation here shown resulting from the dissection and progressive subsidence of a volcanic cone, as shown in sectors H, J, K. According to the Glacial-control theory, which is today the only fully formulated competitor of Darwin's theory that deserves consideration here, the limestones of atolls should as a rule unconformably overlie a flat platform of volcanic and calcareous rocks, produced by the following processes: A preglacial volcanic island, sector A, figure 2, is supposed to have stood still so long as to have been worn down to low relief, as in sector B, while a reef plain was built by outgrowth around it: during the Glacial period, when the ocean was lowered about 40 fathoms and

chilled enough to kill the reef-building corals, the island was progressively cut away by the sea, sectors C, D, and eventually reduced to a platform a little below the sea surface, sector E: when the sea rose and warmed in Postglacial time, a reef was built up around the platform margin, and the enclosed area was covered with lagoon deposits, the resulting structure being shown in section on the face of sector F.

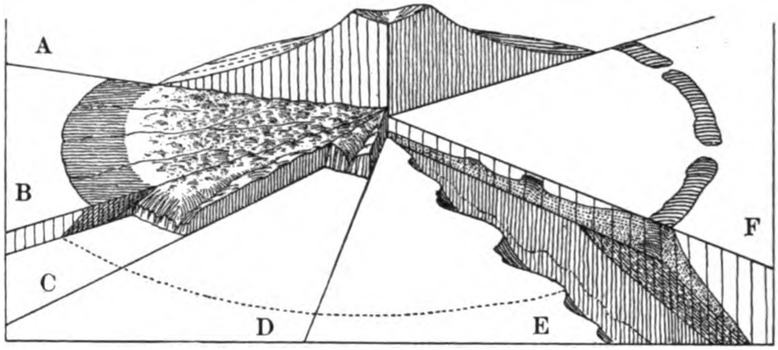

FIG. 2.

Now if such an atoll, sector F, figure 3, be uplifted more than 40 fathoms or 240 feet, as in sector G, and eroded, as in sector H, the central volcanic area of the platform will be sooner or later laid bare, as in sector J. It is evident, however, that no great amount of erosion can take place in Postglacial time; hence it must here be assumed that uplifted atolls which are much dissected were uplifted during the

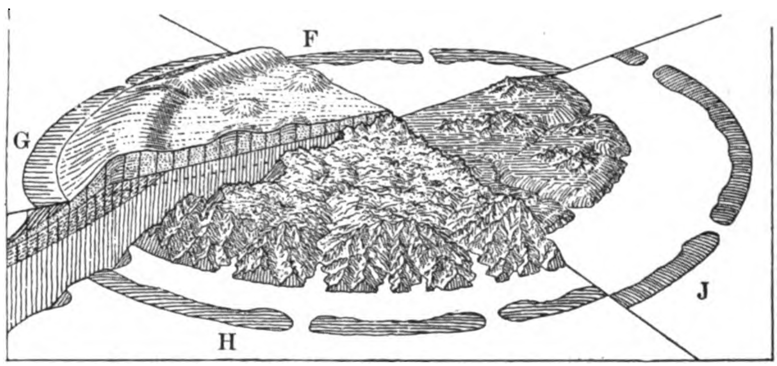

FIG. 3.

Glacial period, and then deeply dissected by subaerial erosion and benched by marginal abrasion during the last Glacial epoch of lowered sea level, so that when the sea finally rose in Postglacial time, an outstanding barrier reef would grow up around the margin of the last abraded marginal bench and enclose a lagoon, the waters of which would enter narrow embayments in the central island, as in sector H. In

case an island were dissected sufficiently to show its abraded platform of central volcanic rocks surmounted by residual limestone hills, as in sector J, a still earlier uplift would be demanded; and in such case, a correspondingly shorter part of the Glacial period would be allowed for the initial abrasion of the volcanic island. The more clearly these various consequences of the theory are conceived, the more closely can the theory be tested when the consequences are confronted with the facts, to which we may now turn.

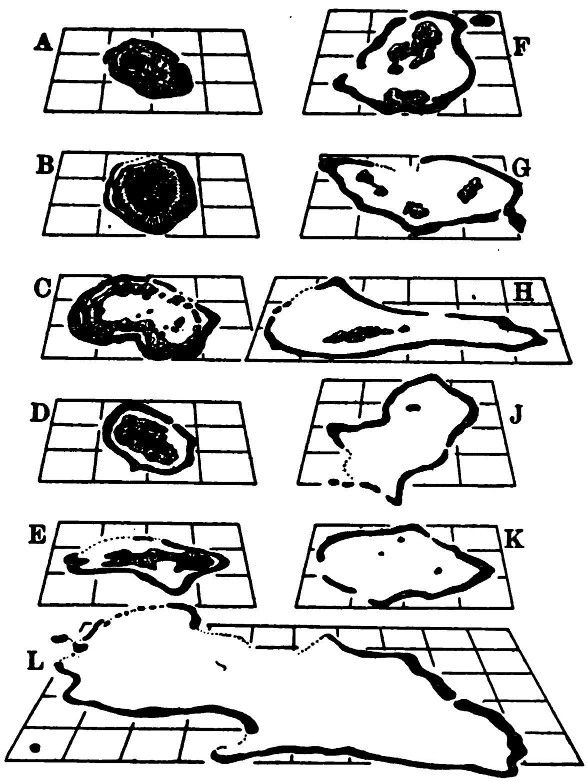

FIG. 4.

A number of uplifted and dissected atolls occur in the Lau group of southeastern Fiji. The route of my Shaler Memorial voyage of 1914 did not, to my regret, lead me to them, but most of them have been described in some detail by Gardiner[1] and Agassiz,[2] from whose reports the following items are taken. The rough outlines of figure 4 are constructed in perspective, with exaggerated height, from Agassiz charts; the squares in the perspective network are 2 nautical miles on a side. The dimensions of the uplifted atolls and of some neighboring sea-

level atolls are presented in the table below, in which the first column gives the letter by which the island, named in the second column, is designated in figure 4; the third column gives the page in Gardiner's report, and the next two columns the page and the plate in Agassiz' report, where descriptions and charts of the atolls may be found. The table is arranged with the best preserved atolls, adjoined by sea-level fringing reefs, as its first members; with dissected limestone islands enclosed by barrier reefs as its middle members; and with two almost-atolls and one true atoll as its final members. Columns 6 and 7 give the dimensions in miles and the height in feet of the uplifted calcareous islands. Columns 8, 9, and 10 give the character of the surrounding sea-level reef, its dimensions, and the depth of its lagoon.

(1) LETTER	(2) NAME	(3) GARDINER	(4) AGASSIZ	(5)	(6) DIMENSIONS	(7) HEIGHT	(8) NEW REEF	(9) DIMENSIONS	(10) DEPTH
	Vatu Vará	462	53	19	1¼ x 1¼	1030	fringe	1½ x 2	
A	Naiau	462	52	20	3½ x 2	580	fringe	4 x 2½	
B	Kambara	463	98	22	3 x 5	320	fringe	3½ x 5½	2
	Wangava	461	66	22	1¼ x 3	290	fringe	4½ x 2	8
	Vanua Vatu	462	121	21	1¼ x 1½	310	close br.	2½ x 3	2
C	Fulanga	457	62	23	5 x 3½	260	fringe-br.	5½ x 4½	4–5
D	Tuvuthá	462	51	20	3½ x 2	800	close br.	4¼ x 3	8–9
E	Namuka	461	57	22	4½ x 1½	260	barrier	7½ x 2½	11–13
F	Ongea	460	60	22	4 x 2	270	barrier	5 x 8	10–13
					1 x 2	300			
G	Yangasa	461	57	22	2 x ½	390	barrier	9 x 5½	16–19
					1 x ½	210			
					½ x ¾	270			
	Aiwa		54	21	2 x ¼	210	barrier	9 x 3	18–23
H	Oneata		56	21	3 x ¾	160	barrier	11 x 2–5	18–20
J	North Argo		125	20	½ x ⅛	80	alm. atoll	5 x 9	18–21
K	Reid		124	20	¼ x ⅛	50	alm. atoll	7 x 6	18–21
L	Great Argo		124	21			atoll	22 x 9	30–36

The first seven islands preserve the form of atolls so well that their emergence must be of recent, postglacial date, and may have been nearly synchronous; but as their altitudes vary greatly, uneven uplift and not a fall of ocean level must be appealed to in accounting for their emergence. It is noteworthy that all these little-dissected islands are surrounded by sea-level reefs of the fringing or close-set barrier type. On the other hand, the five following islands, which lie to the eastward of the preceding seven, do not present the form of atolls; they are of irregular outline in plan and profile; two of them are discontinuous groups of small limestone knobs. If any other origin than uplifted

atolls were available for high-standing oceanic limestone islands, these examples might be ascribed to it; but in the absence of other origin, it seems reasonable to regard them as maturely dissected atolls. They must have been uplifted earlier than their little-dissected neighbors, probably during the Glacial period and not after its close. It is here noteworthy that the surrounding sea-level reefs are relatively distant barriers. Two other examples, North Argo and Reed, take their names, not from the little islands that they enclose, but from the enclosing reefs themselves: they lie farther east than the others and are classed today as almost-atolls; one of the islands in the North Argo lagoon is "stated to be of volcanic origin;" both islands in the Reid lagoon are "probably of elevated limestone." Finally, Great Argo reef is a true atoll, the largest of its kind in Fiji. It is noteworthy that these islands are so distributed as to indicate a westward wave-like progression of a meridional belt of upheaval, so that the earliest uplifted reefs, all lying to the eastward, are at present greatly dissected and somewhat depressed, while the latest uplifted reefs, all lying to the westward, are little dissected; farther west still a number of islands show no signs of uplift, as if the wave of uplift had not yet reached them.

Now although the first ten islands of the table are higher than 240 feet, they do not exhibit any volcanic foundation. The first five of these are, however, so little dissected that the volcanic foundation, if above sea-level, may be concealed by its limestone cover. The absence of a volcanic foundation in the next five argues strongly against the Glacial-control theory; for in Fulanga, although the reef rim is not much dissected, the enclosed area has depths of 4 or 5 fathoms in its lagoon, the bottom of which is thus 290 feet below the rim crest; Tuvuthá, which reaches the exceptional height of 800 feet in its northern part, reveals no volcanic platform at or below a height of 500 feet in its center; Namuka, Ongea, and Yangasa, more dissected and embayed than the hypothetical island of sector H, figure 3, show no volcanic platform, although the vertical measure from their somewhat aggraded lagoon floors to their somewhat worn-down summits ranges from 330 to 500 feet; this is amply sufficient to reveal a volcanic platform if it occurred at the depth of 240 feet below the original reef-level of the now dissected islands. Aiwa and Oneata, small limestone islands in rather large lagoons, although they have undoubtedly lost something of their original height, have vertical measures of 350 and 280 feet between lagoon floor and island top. The three remaining examples are admirable illustrations of Agassiz' theory that some atolls are derived from uplifted and worn-down limestone islands; but the sequence of

forms here given does not, to my reading, support his view that the uplifted limestone islands were not atolls of an earlier generation. Indeed it is a good deal of an assumption that Great Argo reef, a true atoll today, has ever been uplifted, for it contains no limestone islands: the reason for supposing it to have been uplifted is, that limestone islands, mere remnants of formerly larger masses, occur inside of the neighboring barrier reefs. But if Great Argo reef represents an uplifted and worn-down atoll, its uplift must have been relatively early because its erosion is completed; and if its uplift were early, its previous abrasion must according to the Glacial-control theory, have been accomplished in much less than the whole of the Glacial period; yet this is the largest atoll in Fiji.

A sixteenth example might be added, farther north than the others and about on the meridian of the maturely dissected limestone islands, although in its original form before uplift it appears to have been not a true atoll, but an almost-atoll: that is, a reef enclosing a lagoon in which a small volcanic island still survived. This is the group of islands, of which Vanua Mbalavu is the largest, enclosed by the great Exploring reef, some account of which has been given in an earlier article (these PROCEEDINGS, 2, 1916, 471–475). The original sea-level outline of this almost-atoll reef appears to have enclosed a large and irregular lagoon, 15 by 25 miles in diameter, in the western part of which a small volcanic ridge rose in Pliocene or Pleistocene time to a height of 200 or 300 feet: after an uplift of over 600 feet, the limestone plateau was greatly eroded, and reduced for the most part to moderate or small relief, so that in Pleistocene time its larger and higher surviving fragments, partly limestone, partly volcanic, were but a small fraction of the original mass; then the resulting lowland was submerged, and the present barrier reef was built up around its margin; but be it noted that this recent submergence cannot be fully accounted for by the Postglacial rise of ocean level, because the enclosed lagoon floor deepens from 20 fathoms near its western side to over 100 fathoms at its eastern side, thus implying a recent tilting, as Agassiz noted; and this tilting would represent the sinking side of the wave-like upheaval above mentioned. The surviving islands are pertinent in the present connection because several of them show volcanic rocks unconformably covered by eroded limestones, remnants of the uplifted almost-atoll: the contact of the two kinds of rock is not a level platform at a depth of about 240 feet below the highest limestones; on the contrary, the contact exhibits rounded forms and moderate slopes such as characterize volcanic islands maturely dissected by subaerial

erosion; and the occurrence of such forms beneath heavy limestones, 600 feet or more in thickness, clearly demonstrates the submergence of a previously eroded volcanic mass by over 600 feet, while the limestones were forming. Thus not only the recent history of the present barrier reef around Vanua Mbalavu, but also the Pleistocene history of the now dissected almost-atoll, of which Vanua Mbalavu is a remnant, testifies unqualifiedly in favor of Darwin's theory of coral reef and against all other theories. [Since the above was written Foye[3] gives independent evidence of the eastward tilting of Lakemba, which is on about the same meridian as Vanua Mbalavu.]

[1] J. S. Gardiner, *Cambridge, Eng., Proc. Phil. Soc.*, **9**, 1898, (417–503).
[2] A. Agassiz, *Cambridge, Mass., Bull. Mus. Comp. Zool., Harvard Coll.* **33**, 1899, (1–167).
[3] W. G. Foye, *Amer. J. Sci., New Haven*, **43**, 1917, (343–350).

STUDIES OF MAGNITUDE IN STAR CLUSTERS, VII. A METHOD FOR THE DETERMINATION OF THE RELATIVE DISTANCES OF GLOBULAR CLUSTERS

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Communicated by G. E. Hale, June 9, 1917

More than 150 variables for which the light changes are rapid and periodic have been found among the thousand brightest stars in the globular cluster Messier 3. Eighty per cent of them were discovered twenty years ago by Professor Bailey at Harvard,[1] and the remainder three years ago by the writer at Mount Wilson.[2] The light variations of these stars are typical of a large class of variables—the short period Cepheids—some of which are found among the stars in the sky at large, though the far greater majority of those now on record are confined to a few of the globular clusters and to the Magellanic clouds. Wherever found they appear remarkably alike in range of variation, spectral type, color variation, length of period, nature of light changes, and even in the irregularities of the periods and the fluctuations of the light curves.

Recent work with the 60-inch reflector on the variables in Messier 3 is supplemental to the determination of light curves and periods by Bailey,[3] and is incorporated in the general study of magnitudes in clusters primarily for the intercomparison, on the basis of the Mount Wilson scale of magnitudes, of the brightness of variables in this and other globular systems. It is part of an investigation of the magnitudes and colors of all the brighter stars in Messier 3, and follows the methods previously employed.[4]

As much of Bailey's work preceded the establishment of the North Polar Sequence, his values of the magnitudes and ranges of variation may now be standardized. A sample of the revised data is given in table 1. The variables differ very little from each other in any respect, and particularly significant is the striking similarity of the median magnitudes. Bailey derived light curves and periods for 110 variables in this cluster, and 54 of them were chosen as fairly free from uncertainty. The median magnitude (defined as the mean of maximum and minimum) of these 54 stars is 15.49, on the Mount Wilson system, with a probable error of less than a hundredth of a magnitude. The average deviation from this mean for a single star is ±0.07, and the largest deviation is less than two-tenths of a magnitude. If all 110 variables are used, the mean is 15.50±0.006, and the average deviation is ±0.08. The distribution of the residuals follows the law of error as closely as could be expected for a small number of values.

TABLE 1

STAR	PERIOD	BAILEY'S MAGNITUDE		MOUNT WILSON PHOTOGRAPHIC MAGNITUDE			MEDIAN MAGNITUDE
		Maximum	Minimum	Maximum	Minimum	Range	
	days						
9	0.542	14.60	16.76	14.82	16.15	1.33	15.48
10	0.570	14.75	16.70	14.92	16.11	1.19	15.52
27	0.580	15.00	16.68	15.06	16.10	1.04	15.58
34	0.559	15.00	16.70	15.06	16.11	1.05	15.58
38	0.561	14.80	16.83	14.95	16.19	1.24	15.57
40	0.552	14.70	16.83	14.88	16.19	1.31	15.54
49	0.548	14.76	16.76	14.93	16.15	1.22	15.54
63	0.570	14.80	16.62	14.95	16.06	1.11	15.50
80	0.539	14.74	16.70	14.91	16.11	1.20	15.51

Great extremes of absolute brightness are known to exist in Messier 3—nine or ten magnitudes, at least. More than 20,000 stars fainter than these variables have been photographed at Mount Wilson, but no other variables of this or other types have been found among them. The result is confirmed by an examination of long-exposure plates at Harvard. The periodic light variations are apparently confined to a narrow interval of brightness.

Since the deviations of the median magnitudes from their mean are far within the errors of observation, the conclusion is forced upon us that in Messier 3 short-period variation is associated with stars of a very definite intrinsic luminosity. The situation of the variables in a distant globular system necessitates the equality of absolute as well as of apparent magnitudes. Moreover, the work on color, so far as it has

gone, implies that the spectra are all likewise strictly comparable. We conclude, therefore, that in surface brightness and volume, and probably in mass, density, and other physical properties, these 110 stars are almost identical.

Whether the isolated variables of this so-called cluster type also are exactly alike in luminosity, we have at present no means of knowing, other than analogy, because their distances are unknown and their apparent magnitudes differ greatly. To investigate further the possible generality of a law of constant median magnitude, some special studies of the variables in other clusters have been made. The extended discussion will appear in the *Astrophysical Journal*. The results are summarized in the following numbered paragraphs.

1. In Messier 5 Bailey has recently determined the periods and light curves of about 70 variables.[5] Excluding those that are nearer the center than 1′.2, the results for which must be uncertain because of the crowding of images, and omitting also three for which the periods exceed a day, the median magnitude of the remaining 61, referred to Mount Wilson standards, is 15.26±0.01, the average deviation for a single star being ±0.075. Considering only the 30 light curves selected by Bailey as well-determined, the mean median magnitude is 15.25, with an average deviation of ±0.08.

2. In Messier 15 are 51 known variables,[1] but the light curves have not yet been determined. On three plates the extreme magnitude range of the variables has been measured and found to be:

Maximum........................	14.98	Extremes of range.................	1.22
Minimum........................	16.20	Median magnitude..................	15.59

Two peculiarly bright stars, suspected of variability, are excluded. The range of variation is in very good agreement with the mean ranges for Messier 3 and 5, and, although the results are not final, the median magnitude is probably correct within a tenth of a magnitude.

3. In the southern cluster ω Centauri three subclasses of cluster variables are recognized. Treating each separately we have, for the stars whose classification is certain, the results of table 2, which are

TABLE 2

SUB-CLASS	NUMBER VARIABLES	MEAN PERIOD	MAXIMUM MAGNITUDE	RANGE OF VARIATION	MEDIAN MAGNITUDE	AVERAGE DEVIATION
		days				
a..........	33	0.586	12.99	1.11	13.55	±0.09
b..........	15	0.752	13.11	0.87	13.55	±0.10
c..........	28	0.395	13.33	0.56	13.61	±0.09
All........	76				13.57	±0.10

taken from Bailey's tabulation with only slight modifications and additions.[6] Subclass *a*, as the range of variation suggests, is the type that prevails in Messier 3 and 5. Although the stars of the three groups differ from each other in maximum and in range, as well as in period, the median magnitude is the same. The distribution of deviations again follows closely the probability curve. As before the short period variables are restricted to a small interval of brightness which can be represented with high accuracy by a mean median magnitude. The whole interval observed in ω Centauri is six or seven magnitudes, but no fainter variables are found, and the two or three brighter ones are long-period Cepheids. The magnitudes used for this cluster are not referred to the Mount Wilson system, and, therefore, are not strictly comparable with those of the other clusters. They are probably about right, although the uncertainty may be as much as half a magnitude.

4. Ten stars in Messier 2 have been suspected of variation.[1] Eight have been verified on Mount Wilson plates, but as no comparisons with the Pole have been made, only provisional results are available. Measures on three plates give the extreme variation as one and a third magnitudes, agreeing with all that precedes in showing that the short period variables are confined to a definite limit of magnitude.

These results for the variables in five clusters have an obvious application in the determination of relative parallaxes. We need only the hypothesis, apparently reasonable in the light of the foregoing discussion, that the absolute median magnitudes, which appear so constant in each cluster, are actually identical in all systems. The observed differences in the mean values then become sensitive criteria of distance, and the relative parallaxes of these remote systems can be known with an accuracy which will depend only on the precision with which the photographic magnitudes can be determined.

For instance, we find above that the median magnitude for Messier, 3 is 15.49 ± 0.01, and the corresponding value for Messier 5, 15.25 ± 0.01. Possibly there are small systematic errors due to choice of variables, to errors in the maxima, or to remaining errors of zero point, which are not eliminated in taking the difference. But certainly we know the difference in the two median magnitudes within a tenth of a magnitude, and consequently the difference in distance of these remote and nearly equidistant clusters within 40% of its value; while the difference in distance can be known within 7% of its value, if either cluster is compared with ω Centauri. And once we find the absolute median magnitude of such variables—by no means a hopeless task—the actual distances of all clusters with typical variables can be determined within

5%, an accuracy as yet quite unattainable by direct measurement for any stellar object except the nearest stars.

The derivation of a probable value of the absolute luminosity of cluster-type variables will be given in the extended paper. Provisionally we observe that the absolute median magnitude is probably within the limits -0.5 and $+1.5$ (unit of distance, $\pi = 0''.01$), and on that basis the absolute parallax of four of the above clusters is as follows:

Messier 3	π between 0".00006 and 0".00016
Messier 5	π between 0.00007 and 0.00018
Messier 15	π between 0.00006 and 0.00015
ω Centauri	π between 0.00015 and 0.00038

Only a small number of the globular clusters are known to contain short-period variables. The relative distances of the others can be estimated, however, on the basis of a relation, found in the clusters that contain variables, of the median magnitude to the average magnitude of the brightest stars. To derive this relation the photographic magnitudes of all the brighter stars in each cluster were determined, excluding stars more distant from the center than 10′ as possibly not members, and also these within 2′ as too liable to uncertainty of measurement. Of those remaining, the first five in order of brightness were discarded as superposed stars, or as non-typical in luminosity. The mean magnitude of the next 25 stars, a homogeneous group of highly luminous objects, was formed. Thus were obtained the results of table 3, which seem as remarkable and as significant for stellar theories as the phenomenon of constant median magnitude.

TABLE 3

CLUSTER	MEDIAN MAGNITUDE	AVERAGE DEVIATION	NUMBER OF VARIABLES	MEAN MAGNITUDE BRIGHTEST 25 STARS	AVERAGE DEVIATION	MEDIAN MINUS BRIGHTEST	WEIGHT
Messier 3	15.50	±0.08	110	14.14	±0.16	1.35	4
Messier 5	15.26	±0.075	61	13.92	±0.15	1.34	2
Messier 15	15.59		49	14.28	±0.18	1.31	2
Messier 2			8		±0.22	1.40	1
Weighted mean difference						1.35	

In view of these results it is reasonable to believe that, if short-period variables did exist in one of the many globular clusters in which they have not been found, their median magnitudes would average about 1.35 fainter than the average magnitude of the 25 selected brightest

stars. In other words, it is proposed to estimate the distances of clusters lacking typical variables on the basis of their bright stars.

Finally it may be noted that our assumption of the identical luminosity of these variables in the various clusters tacitly implies that the corresponding variables in the galactic system are all of the same absolute magnitude. That leads immediately, and without further hypothesis, to the derivation of highly precise relative parallaxes of the 40 scattered stars of this type, and as some of these variables are extremely faint, the results bear directly on the extent of the general stellar system.

Summary.—(1) The median magnitude of the short-period variables apparently has a rigorously constant value in each globular cluster. (2) These stars also possess essentially identical spectra and color variations. (3) Such phenomena, implying remarkably similar physical conditions for all cluster-type variables, must play an important part in theories of Cepheid variation. (4) The present use of the photographic magnitude observations, however, is to derive accurate relative distances of globular clusters on the basis of the differences, from system to system, of the mean median magnitude. (5) The method extends naturally to all isolated cluster-type variables in the galactic system. (6) By means of a relation between the median magnitude and the magnitudes of the brightest stars in a cluster, the relative parallaxes of the many clusters lacking variables may be estimated with considerable assurance when the magnitudes have been measured. (7) The determination of the absolute luminosity of some of these variables will give immediately absolute individual distances of the clusters and of the forty isolated cluster-type variables of known magnitude and period. (8) A preliminary value of this absolute median magnitude indicates that with one or two exceptions no globular cluster is nearer than thirty thousand light-years ($\pi = 0''.00012$).

[1] Bailey, S. I., *Ann. Obs. Harvard Coll., Cambridge*, **38**, 1902, (1–252), page 2.
[2] Shapley, H., *Mt. Wilson Contrib.*, No. 91; *Astroph. J., Chicago*, **40**, 1914, (443–447).
[3] Bailey, S. I., *Ann. Obs. Harvard Coll., Cambridge*, **78**, Part I, 1913, (1–98).
[4] Shapley, H., *Mt. Wilson Contrib.*, No. 115, 1915, (1–92), pages 8 to 22.
[5] Bailey, S. I., *Ann. Obs. Harvard Coll., Cambridge*, **78**, Part II, 1917, (103–192).
[6] *Ibid.*, **38**, 1902, (1–253).

THE PRINCIPAL AXES OF STELLAR MOTION

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Communicated by E. B. Frost, May 28, 1917

Suppose the stars of known motion transferred to a fixed point O, and allowed to move with their present velocities for a definite length of time. They will have expanded into a cluster, the 'velocity-figure.' According to the older idea that the stars were moving 'at random,' this cluster should be spherical. Since J. C. Kapteyn showed[1] the figure to be elongated, several methods of finding the amount and direction of elongation have been proposed and used. Some of these make use only of the directions of observed proper-motions, leaving the amounts of those motions wholly out of account, thus avoiding the difficulties caused by our ignorance of the distances of most stars; and are based on some hypothesis as to the actual shape of the velocity-figure. The most notable of these are A. S. Eddington's[2] and K. Schwarzschild's.[3] The method here used makes no assumption as to the form of the velocity-figure, and is essentially a process of finding its principal momental axes. It thus belongs to the same *genus* as those used by C. V. L. Charlier,[4] K. W. Gyllenberg,[5] A. S. Eddington and W. E. Hartley,[6] and H. C. Plummer.[7]

Let the coordinates of a star in the velocity-figure, referred to suitable axes through O,—say to the First Point of Aries, to (6h, 0°), and to the North Pole, respectively,—be x', y', z'; the direction cosines of an arbitrarily chosen line be l, m, n; and the projection of the star S upon this line be P. Then the component of motion in the direction l, m, n, is $p = OP = lx' + my' + nz'$. Representing the mean of a quantity by enclosing it in square brackets,

$$[pp] = l^2[x'x'] + m^2[y'y'] + n^2[z'z'] + 2\,lm[x'y'] + 2\,ln[x'z'] + 2\,mn[y'z'] \quad \text{(A)}$$

The axis of preference is defined as the line for which $[pp]$ is a maximum; for the axis of avoidance $[pp]$ is a minimum.

Newcomb, in his paper 'on the Principal Planes toward which the Stars Tend to Crowd,' applies to another problem an essentially similar process, and gives a detailed solution by Lagrange's method for a maximum or minimum of (A), from which the procedure of this investigation is adapted. Putting $A = [x'x']$, $B = [y'y']$, $C = [z'z']$, $D = [x'y']$, $E = [x'z']$, $F = [y'z']$, the maximum, minimax, and minimum

values of $[pp]$ are the three roots of the cubic

$$\begin{vmatrix} (A-\lambda) & D & E \\ D & (B-\lambda) & F \\ E & F & (C-\lambda) \end{vmatrix} = 0 \qquad \text{(B)}$$

and the corresponding values of l, m, n, are derived by inserting these roots successively in the set of equations

$$\begin{aligned} (A-\lambda)l + \quad Dm + \quad En &= 0 \\ Dl + (B-\lambda)m + \quad Fn &= 0 \\ El + \quad Fm + (C-\lambda)n &= 0 \end{aligned} \qquad \text{(C)}$$

The coordinates x', y', z' are referred to the unknown center of gravity of the velocity-figure. We can observe only the velocities x, y, z, relative to the moving sun. But we readily find that

$$\begin{aligned} [x'x'] &= [xx] - [x]\cdot[x] & \qquad [x'y'] &= [xy] - [x]\cdot[y] \\ [y'y'] &= [yy] - [y]\cdot[y] & \qquad [x'z'] &= [xz] - [x]\cdot[z] \\ [z'z'] &= [zz] - [z]\cdot[z] & \qquad [y'z'] &= [yz] - [y]\cdot[z] \end{aligned} \qquad \text{(D)}$$

The quantities $[x]$, $[y]$, $[z]$, are the negatives of the coordinates of solar motion, according to the method of Bravais.

We are limited to the use of the proper-motions alone, or the radial velocities alone, since to combine them requires a knowledge of individual parallaxes which we do not possess. The rectangular components of the proper-motion part (tangential to the celestial sphere) of the stellar motions, resolved as before, may be expressed in terms of the proper-motions μ, μ', and the spherical coordinates of the stars, while μ, μ' may be expressed in terms of x, y, z. Designating the tangential part by subscript l, we readily obtain

$$x_1 = x(1-\cos^2\alpha\cos^2\delta) - y\sin\alpha\cos\alpha\cos^2\delta - z\cos\alpha\sin\alpha\cos\delta,$$

and similarly for y_1 and z_1. From these x, x_1, etc., are found by multiplication. The means of these quantities are found by integration over the sphere. They are, cleared of fractions,

$$\begin{aligned} 3[x_1] &= 2[x] & \qquad 15[x_1y_1] &= 7[xy] \\ 3[y_1] &= 2[y] & \qquad 15[x_1z_1] &= 7[xz] \\ 3[z_1] &= 2[z] & \qquad 15[y_1z_1] &= 7[yz] \end{aligned}$$
$$\begin{aligned} 15[x_1x_1] &= 8[xx] + [yy] + [zz] \\ 15[y_1y_1] &= [xx] + 8[yy] + [zz] \\ 15[z_1z_1] &= [xx] + [yy] + 8[zz] \end{aligned} \qquad \text{(E)}$$

The admissibility of these integrations depends upon certain conditions, which may be stated in the form of three hypotheses:

I. The stars are evenly distributed over the sky.

II. The distribution in distance is the same in different parts of the sky.

III. The motions of the stars are distributed in the same way in all accessible parts of space.

These hypotheses are of course not rigorously true. III is open to objection, but if it is not at least approximately true we have no general problem, merely a number of local problems. After the mean square velocities in the three principal directions are known, failure of II, so far as effect of different mean square parallax in different parts of the sky is concerned, may be taken care of and a new approximation made. The theoretical value of $[x_1x_1] + [y_1y_1] + [z_1z_1]$ can be computed for each region of the sky and compared with the observed values. A difference may be considered as due to the stars being nearer or farther than the average and factors applied accordingly. These approximations converge very rapidly; in fact, the error from the source considered is small, even in the first solution. Errors due to the spread or range of the stars in distance are not corrected by this process, but must usually be negligible. Hypothesis I can be made valid by simply weighting the material proportionally to the area of the sky covered, instead of proportionally to the number of stars.

The material used was the 5943 proper-motions less than 80″ per century from Boss's Preliminary General Catalogue. Of these, 5384 were less than 20″ per century (allowing for solar motion[8]) and were called "S"; the remaining 559 stars of larger proper-motion were called "L." Group S was subdivided according to type of spectrum as follows: B, types Oe5 to B 5, 492 stars; A, B 8 to A 4, 1647; F, A 5 through F, 656; G, 446 stars; K, 1227; M, 223; X, including stars of unknown type—mostly faint—and some of types N, O, and P, 693 stars.

The numerical results appear in the accompanying tables. Table 1 gives, in column 1, the designation of the group; in 2 and 3 the Right Ascension and Declination respectively of the direction in which $[pp]$ is a maximum—the vertex of preferential motion; in 4 and 5, the same for direction of least motion. The third direction, in which $[pp]$ is a minimax, is necessarily at right angles to each of these, hence is not given. The last three columns give the value of $[pp]$ for these three directions. Unit of motion, 1″ per century.

Table 2 gives the solar motion derived at the same time. A and D are the Right Ascension and Declination of the Apex, M the amount of solar motion. The last three columns are given as a means of comparing the various groups. If the means without regard to sign of the

components of motion in the three principal directions be $[u]$, $[v]$, $[w]$, we have

$$[u] = \sqrt{(2\lambda_1/\pi)},\ [v] = \sqrt{(2\lambda_2/\pi)},\ [w] = \sqrt{(2\lambda_3/\pi)}$$

provided the components are distributed according to the error law. They have merely the broad characters of such distribution,—sufficient to justify the process in the same sense that the customary method of finding the 'probable error,' without inquiring too diligently as to whether the errors of observation are actually distributed in the manner assumed in theory, is justified.

TABLE 1

SYSTEMATIC MOTION

GROUP	POLE OF PREFERENCE		POLE OF AVOIDANCE		MEAN SQUARE MOTIONS		
	A_1	D_1	A_2	D_2	λ_1	λ_2	λ_3
B............	100°6	−38°4	180°4	+12°6	4.58	1.94	1.33
A............	92.4	+1.8	182.9	+15.2	25.65	9.15	2.57
F............	93.1	+2.1	185.2	+45.9	58.05	28.78	22.66
G............	93.8	+3.4	185.4	+24.4	32.26	15.81	9.96
K............	93.1	+11.3	191.2	+35.3	29.30	18.71	11.46
M............	94.0	+25.3	205.1	+37.2	28.14	12.58	3.48
X............	83.9	+30.2	192.6	+29.0	21.08	12.72	5.33
S............	91.9	+ 4.2	183.8	+24.9	28.91	14.99	9.89
L............	92.5	+35.3	211.7	+34.5	745.6	261.4	194.7
All............	92.3	+16.9	184.7	+ 8.1	107.3	58.7	31.8

TABLE 2

SOLAR MOTION

GROUP	NUMBER	SOLAR APEX			MEAN MOTIONS		
		A	D	M	[u]/M	[v]/M	[w]/M
B............	492	274°2	+39°3	2″33	0.73	0.47	0.40
A............	1647	266.7	+27.9	4.03	1.00	0.60	0.32
F............	656	262.9	+27.0	6.36	0.96	0.67	0.60
G............	446	256.6	+45.8	2.60	1.74	1.22	0.97
K............	1227	272.5	+38.4	4.02	1.08	0.86	0.67
M............	223	272.2	+45.7	3.45	1.23	0.82	0.43
X............	693	271.4	+42.1	3.06	1.20	0.93	0.60
S............	5384	269.0	+32.1	3.83	1.12	0.81	0.66
L............	559	269.3	+31.5	22.17	1.01	0.61	0.52
All............	5943	269.1	+32.3	5.42	1.53	1.13	0.83

In regard to these results the following points seem especially worth noting:

1. All groups show markedly unequal motions in three principal directions. In all groups the direction of greatest motion, or axis of preference, lies near the plane of the galaxy not far from its intersection with the equator, the axis of avoidance is nearly perpendicular to the galaxy.

Using radial velocities Gyllenberg[5] finds much the same axis of preference, but the other two axes practically interchanged. So do Eddington and Hartley[6] for later types, but their results for early types agree better with table 1. Are these differences real, in the sense of being typical of what may be expected whenever radial velocities and proper-motions are compared? If so, what peculiarity of distribution or of motions in space, out of the many that might easily be suggested, lies back of it? Here is a subject well worth investigation.

2. The separation of the poles of preference into two groups corresponding respectively to 'early' and 'late' types,[9] is confirmed. There seems to be some evidence of progression of the same character within each of the two groups of types, as if with advance of type the 6-hour vertex moved northward. With advance of type there is an increase of stellar as compared with solar motion; that in the preferred direction, however, increasing less than the other two components.

3. The apices of solar motion show the division into two groups already found by several investigators; A and F having apices in smaller R. A. and Decl. than K, M, and X. B seems to resemble the latter group. G is anomalous.

These results agree rather closely with those found by L. Boss by Airy's method,[10] and by the writer by Schwarzschild's method,[9] all three being based upon practically the same material. This agreement will be found to extend even to details.

4. The distribution of stars of type B is so peculiar that to weight the material according to area covered would have been unfair. The integrations which give equations (E) can be made over zones parallel to the galaxy, and the resulting relations used to correct each zone for perspective separately, the material then being combined with weights proportional to the number of stars. The results in the tables were thus found; a check solution by the ordinary method gave for the vertex (122°,–36°).

The vertex of preferential motion is near the antapex of solar motion. This may in part be a manifestation of the tendency, noted above, for the earlier types to have more southerly vertices. In part it is due to

the fact that the stars' 'spread' in distance tends to produce a false elongation of the velocity-figure in the direction apex-antapex. This tendency is stronger the greater the spread, the smaller the velocity-figure relative to solar motion, and the more nearly spherical the figure. The last two causes, and possibly the first, operate strongly in the B stars. There is no appearance of such an effect in the other groups.

5. The separation of groups "S" and "L" was made some years ago, for a different purpose, and according to a method which made allowance for solar motion relative to group S, but not enough for L, and made none for preferential motion. Consequently, while S no doubt represents fairly well the smaller motions, L does not truly represent the large ones. In group "All," while the large motion characters are diluted by large numbers of small motion stars, the former have individually much more weight than the latter; this therefore is probably more representative that L of the large proper-motion stars. A re-division into small and large proper-motions, on principles better adapted to the purpose of this investigation, is in progress.

[1] *London, Rep. Brit Ass.*, 1905, (257).
[2] *London Mon. Not. R. Astr. Soc.*, **67**, 1906, (34).
[3] *Göttingen, Nachr., Math.-Phys. Kl.*, 1907, (614); 1908 (191).
[4] *Medd. Lunds Astr. Obs.*, (Ser. 2), No. 9, 1913.
[5] *Ibid.*, No. 59, and (Ser. 2), No. 13, 1914–15.
[6] *London Mon. Not. R. Astr. Soc.*, **75**, 1915, (521).
[7] *Ibid.*, **76**, 1915, (121).
[8] *Astr. J. Boston*, **612**, 1910, (98).
[9] *Ibid.*, **676**, 1915.
[10] *Ibid.*, **614**, **623–4**, 1910–11.

VOLUME 3 AUGUST, 1917 NUMBER 8

PROCEEDINGS

OF THE

NATIONAL ACADEMY OF SCIENCES

OF THE

UNITED STATES OF AMERICA

Publication Office: Williams & Wilkins Company, Baltimore
Editorial Office: Massachusetts Institute of Technology, Cambridge
Home Office of the Academy: Washington, D. C.

Entered as second-class matter at the Post Office at Baltimore, Md., under the Act of August 24, 1912

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PROCEEDINGS
OF THE
NATIONAL ACADEMY OF SCIENCES

Volume 3 AUGUST 15, 1917 Number 8

RELATION OF PREFERENTIAL MOTION AND OF THE SPECTRAL-CLASS AND MAGNITUDE VELOCITY PROGRESSIONS TO PROPER MOTION

By C. D. Perrine

OBSERVATORIO NACIONAL ARGENTINO, CÓRDOBA

Communicated by E. B. Frost, May 28, 1917

This investigation concerned the relation between the preferential motion or streaming of the stars as interpreted by the ellipsoidal theory, and the size of proper motion.

It is based upon the radial velocities of Campbell's L. O. Catalogs[1] and Adams' Mt. Wilson Catalog,[2] a total of some 1800 stars. The stars, after being cleared of the effect of solarmotion, were divided into two classes according to size of proper motion, the 'large' ones being 0."10 per year and over, while the 'small' ones are less than 0."10, and into three classes by magnitude. The stars of Campbell's catalogs were divided into two groups, those of magnitude 2.9 and brighter and those of 3.0 to the limit of his catalogs which contain few stars fainter than 5½ magnitude. Adams' stars, with but few exceptions, are between magnitudes 5.5 and 6.5, and were treated separately as a third group.

Following the method of Kapteyn and Adams,[3] the radial velocities, without respect to sign, of the stars within 50° of the vertices of the ellipsoid were combined to represent the major axis of the ellipsoid which is designated by ρ_2. In a similar way those between 60° and 90° of the vertices were combined to represent the minor axis ρ_1.

The prolateness obtained for the stars of small μ of class F in Campbell's stars of 3.0 and fainter was 1.91. An examination showed that the average value of μ for these stars was considerably higher than for the other classes with small μ. The experiment was tried of limiting the proper motions of this group to 0."05 which reduced the prolateness to 1.17. None of the other groups were so restricted in the matter of

size of proper motion. The results for the different spectral classes are given in table 1 where the numbers of stars in the groups are in parentheses.

TABLE 1

PROLATENESS FROM RADIAL VELOCITIES

ρ_2/ρ_1

SPECTRAL CLASS	CAMPBELL'S L. O CATALOGS				ADAMS' MT. WILSON CATALOG	
	2.9 and brighter		3.0 and fainter			
	Small μ	Large μ	Small μ	Large μ	Small μ	Large μ
B........	(30) 1.81		(113) 0.96		(81) 0.82	
A........			(127) 1.18	(31) 1.79	(110) 1.23	(9) 1.88
F........			(38) 1.17	(126) 1.63	(18) 1.33	(11) 2.00
G........			(81) 1.31	(41) 1.54	(55) 1.64	(29) 2.88
K........			(226) 1.09	(124) 1.42		
M........			(52) 1.19	(13) 1.96		
K, M........	(20) 1.23	(18) 0.87			(84) 1.17	(31) 1.19
A, F, G........	(18) 4.56	(25) 1.50				

With the exception of the K and M stars of Adams' catalog and those of 2.9 and brighter of the same classes in Campbell's catalogs, a consistently smaller prolateness is shown for the small than for the large proper motions. These exceptions may be only coincidences, but they require further investigation.

The underlying idea in this investigation was that the nearer stars (those of large μ and also the very bright stars) might be subject to different preferential motions from the more distant stars. This was indicated by anomalies in the position of the apex of solar motion yielded by 110 of the stars of magnitude 2.9 and brighter, and in the positions yielded by the different sizes of proper motion. It is to be noted that the 30 bright B stars show a large prolateness and are quite consistent for so small a group—half of the velocities in the regions of the vertices of the ellipsoid are larger than any in the regions at right angles to the axis. The large velocities are found in the region of both vertices. The indications that the very distant stars are not subject to preference of motion for the ellipsoidal axis furnishes a satisfactory explanation of the peculiar behavior of the class B stars.

A dissymmetry has been observed in the velocities of the A, F, and G stars of small μ north and south of the ellipsoidal axis which produces the effect of a considerable prolateness. It is in the nature of smaller average radial velocities for the northern stars than for other parts of the sky. If determinations of prolateness are confined to northern stars

only, values are obtained from the stars both of Campbell's and Adams' catalogs which are nearly the same as those yielded by the generally accepted constants of the ellipsoidal theory.

Evidence has been encountered in this investigation that the increases of radial velocity with spectral class and with decrease of brightness which have been observed, are in reality, largely at least, manifestations of the greater velocities which occur in the stars of large proper motion. When classified with respect to the size of proper motion and relation to the ellipsoidal axis, omitting a very few apparently exceptional velocities, these progressions disappear almost completely as will be seen from tables 2 and 3.

TABLE 2

ACCORDING TO SPECIAL CLASS*

(Small μ)

	ADAMS' MT. WILSON CATALOG			CAMBELL'S L. O. CATALOGS			
				3.0 and fainter		2.9 and brighter	
	ρ_2	ρ_1	Mean ρ	ρ_2	ρ_1	ρ_2	ρ_1
	km.	*km.*	*km.*	*km.*	*km.*	*km.*	*km.*
B..........	(52) 8.6	(29) 10.5	8.2	(60) 7.9	(53) 8.2	(16) 8.9	(14) 5.3
A..........	(49) 9.7	(55) 8.1	8.5	(56) 10.5	(60) 9.8		(3) 4.2
F..........	(6) 10.8	(10) 7.3	9.7	(18) 12.2	(18) 9.2	(4) 12.3	(3) 6.4
G..........	(23) 10.6	(29) 7.8	8.1	(44) 11.4	(32) 8.2		(3) 2.7
K..........	(25) 11.6	(24) 8.1	9.7	(80) 12.5	(120) 10.8	(6) 8.8	(8) 6.2
M..........	(11) 10.0	(13) 11.9	8.8	(13) 12.4	(28) 11.0	(2) 6.1	(1) 1.5

* Rejecting a few velocities of 30 km. and over.

TABLE 3

ACCORDING TO BRIGHTNESS*

	SMALL μ		LARGE μ			
	ρ_2	ρ_1	ρ_2	ρ_2'	ρ_1	ρ_1'
	km.	*km.*	*km.*	*km.*	*km.*	*km.*
2.9 and brighter....	(34) 13.5	(34) 6.2	(13) 15.9	54.8	(26) 13.6	35.8
3 to 5½............	(297) 13.6	(317) 11.8	(96) 26.4	69.3	(212) 17.6	40.2
5½ to 6½...........	(179) 12.4	(169) 10.4	(31) 45.2	70.6	(49) 22.3	39.8

* Omitting two stars in each of the two groups of 2.9 and brighter large μ because of the excessive effect in such small groups of their proper motions. ρ_2' and ρ_1' have been reduced to proper motion unity to make them comparable in that respect.

The conclusion from this investigation that the phenomenon of preferential motion for the ellipsoidal axis is confined to the nearer stars, in connection with the relation between radial velocity and proper motion which has been found by Kapteyn and Adams[3] automatically

leads to the inference that the velocities of the nearer stars are greater than for the distant stars. Such a condition is confirmed by the velocities of 41 stars whose parallaxes have been observed to be 0.″06 or over. Their average radial velocity is 26.9 km. (or 20.7 km. if we omit four stars with velocities of from 73 km. to 98 km.) whereas the average velocity of a large number of stars of similar magnitude and spectral class (including many stars of small μ) which are undoubtedly at a much greater average distance than the 41 stars, may be taken at 15 km. (or 10 km. if limited to small proper motion).

Conclusions.—1. The preference for motion in the direction of the ellipsoidal axis appears to be confined to the stars of larger proper motion and brighter than 3.0 magnitude in all of the spectral classes with the possible exception of K and M. The limit of proper motion below which little or no preference for the ellipsoidal axis is shown, appears to be about 0.″05.

2. The increase in prolateness in the stars of large proper motion can be traced chiefly to the direction of the ellipsoidal axis.

3. This relation to proper motion furnishes a satisfactory explanation of the peculiar behavior of the stars of class B.

4. The radial velocities of the nearer stars are larger than those of the more distant stars.

5. When classified according to size of proper motion, and a few large motions are excluded, there is no certain increase in the radial velocities of the different spectral classes.

6. When the radial velocities are classified according to size of proper motion, there is no certain change with magnitude.

The details of this investigation will be published in the *Astrophysical Journal.*

[1] *Berkeley, Lick Obs., Univ. Cal. Bull.*, 6, 1911, (108); 7, 1913, (20, 113).
[2] *Pasadena, Contrib. Mt. Wilson, Solar Obs.*, No. 105.
[3] These PROCEEDINGS, 1, 1915, (14).

GROWTH OF ISOLATED SPOROPHYTES OF ANTHOCEROS

By Douglas Houghton Campbell

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Communicated, June 19, 1917

In view of the speculations concerning the possible derivation of the ferns from Anthoceros-like forms, it seemed worth while to determine how far the sporophyte is capable of an independent existence when separated from the supporting gametophyte.

The highly developed photosynthetic tissues of the sporophyte of Anthoceros, and its long period of growth, due to the persistence of actively dividing cells at its base, suggest that if the sporophyte were able to absorb water directly, instead of through the mediation of the gametophyte, it might be possible for it to live and develop when separated from the gametophyte, and thus to simulate the behavior of the young fern-sporophyte.

Preliminary experiments were made in the spring of 1916, and more extended ones were made during the winter and spring of 1916–17.

While these experiments were of a somewhat tentative character, and by no means exhaustive, the results were sufficiently interesting to seem worthy of record.

The species chosen for experiment, *Anthoceros Pearsoni* Howe, is a common liverwort in the vicinity of Stanford University. Young sporophytes were noted during the autumn, and plants were brought in at intervals during the late autumn and winter. They were kept in a cool greenhouse where they grew luxuriantly and furnished abundant material for experiment.

The large foot of the sporophyte is so completely grown to the surrounding tissue of the gametophyte, that it is impossible to completely isolate the sporophyte without removing the foot. It was therefore necessary to cut away, as completely as possible, the gametophytic tissues, thus exposing the foot of the sporophyte.

In the first experiments, the isolated sporophytes were placed with the lower part in a nutritive solution (Knop's Solution) but this was not found to be satisfactory, as bacteria developed to such an extent as to quite envelop the submerged part of the sporophyte, whose growth was thus much interfered with, although in some cases they survived for a month or more.

Much more satisfactory results were obtained by planting in sterilized earth. Small flower-pots filled with earth were thoroughly sterilized, and after planting, the flower-pots were placed in vessels filled with boiled tap water, thus avoiding watering from above. Each pot was covered with a tumbler, and in this way the isolated sporophytes were kept in a healthy state for a surprisingly long time.

Sooner or later Oscillatoria and other algal growths appeared, but probably with proper care these might be pretty well eliminated.

For a short time, a slight increase in length was noted, but this was in no case very marked. The most striking effect of the transplanting was apparently a marked hastening of the spore formation. Most of the specimens selected had not yet begun to show spores, and the

young sporophyte throughout was a vivid green. After about a month, most of them showed the darkening of the apical region, indicating the presence of ripe spores, and this darkening soon extended downward until it embraced pretty nearly the whole sporophyte. An examination showed both spores and elaters to be apparently in no way different from those found in normal sporophytes. These spores were observed to germinate freely, and the young gametophytes produced from them were apparently entirely normal.

As it might be argued that the small amount of gametophytic tissue left adhering to the isolated sporophyte might be necessary for the absorption of water, experiments were made in which the sporophyte was pulled out of the calyptra, leaving the foot behind. Although the larger part of these footless sporophytes failed to survive, nevertheless, a number of them lived for two months or more, and behaved very much like the others.

The only case in which there was present anything suggesting a formation of roots from the foot, was one where the bottom of the foot had been exposed by cutting away the underlying tissue of the gametophyte, thus bringing the foot into direct contact with the soil. A couple of short outgrowths, of the superficial cells, resembling rhizoids, were seen, but they were too inconspicuous to be of any great importance. It is hoped that further investigations may furnish more positive results.

This much can be definitely stated. The young sporophyte of *Anthoceros Pearsoni*, separated from its association with the gametophyte, is capable of a limited growth in length, and is able to mature normal spores and elaters from the young sporogenous tissue.

The following data may be of interest.

The greatest elongation noted, after transplanting, was about 3 mm. In most cases it was less than this.

The first planting was made on November 28. Some of these sporophytes were still alive on March 13. The foot had rotted off, but the chromatophores in the upper part were still green, and normal ripe spores had developed.

In the later plantings, many of the sporophytes were still alive when the experiments were concluded, ranging from six weeks to two months from the time of planting.

On January 13, a number of sporophytes, deprived of the foot, and thus completely severed from the gametophyte, were planted. Some of these were still alive on March 15. The cells of the broken end were still alive, but there was no evidence of rhizoid formation. Ripe spores and elaters were developed, as in the other experiments.

THE MESA VERDE TYPES OF PUEBLOS

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Communicated, June 14, 1917

The excavation of a mound in the Mesa Verde National Park, Colorado, by the Smithsonian Institution, at the request of the Interior Department, during the summer of 1916, uncovered a building called Far View House belonging to a type which is morphologically the same as that of adjacent cliff dwellings. In form Far View House differs from its nearest large neighbor, Spruce Tree House; its outline is rectangular while that of a cliff dwelling, like Spruce Tree House, follows the irregular walls of the cave in which it lies. So close is its likeness in other points that we may say that the main difference between the two is that one is constructed in a cave sheltered by an overhanging roof, while the other is built under the open sky, without this protection; both are pure examples of the same type.

The importance of site has been magnified by some archæologists, and it must be confessed that the resemblance of the modern pueblo type to that of a cliff dwelling, is not very close. The accepted belief in an identity of cliff dweller and pueblo, largely determined by legendary and somatological evidences, is supported by architectural features of the Mesa Verde type, which is the purest form of pueblo construction. The former failure of house structure to adequately show this identity was due to the fact that modern pueblos belong to a mixed or highly modified type. Far View House is nearer in time as well as in form to the cliff dwelling, being unchanged by foreign influences. A comparison of it with typical cliff dwellings shows good evidences that community houses erected on sites so different are practically identical in details of construction and practically contemporaneous.

The type of pueblo illustrated by Far View House is now extinct and we have reason to believe that it antedated the beginning of the seventeenth century. It is therefore a true unmodified expression of the aboriginal mind, representing a stage in the development of southwestern architecture preceding the modern type.

The feature that distinguishes the community building of our southwest from other aboriginal dwellings north of Mexico is the arrangement of rooms in stories, one above the other. Of course in its simplest form it has but one story; the multistoried form is characteristic of the highest developed condition of the eastern or pueblo area. It is pronounced in the Mesa Verde type when there are two forms of rooms

structurally unlike and functionally different. Some of these rooms are circular, others rectangular; the former probably ceremonial, the latter secular, probably domiciles. Both kinds of rooms were closely crowded together in a compact mass, and were two or more stories high, the upper rooms having lateral entrances from the roofs of lower terraces; the ground floor chambers generally entered through the roofs. In its simplest form the Mesa Verde type has but one circular room, centrally placed, with rectangular chambers arranged about it. The inhabitants carried on most of their daily occupations on the terraces; the men used the circular rooms for assemblies and ceremonies.

In Far View House there are four of these circular rooms, one of which, the largest, is centrally placed; the rectangular rooms number about fifty. On the south side there is a rectangular court enclosed on three sides by a low wall. A few yards from the southeast angle there is a low mound, the site of the village burial place.

Geographically the Mesa Verde type is widely distributed; its center of distribution was the valley of the San Juan and its tributaries, canyons or mesas of Arizona, Colorado, Utah, and New Mexico. Examples of it are not only numerically most abundant in this region but also are the best constructed. The influence of the type extends far from its center of origin, becoming modified as the distance increases. It appears in the great communal houses of the Chaco Canyon in New Mexico and in the cliff dwellings of Canyon de Tsegi, or Chelly Canyon, in Arizona. The type occurs both isolated and in clustered forms, as at Mummy Lake on the Mesa Verde, and on the La Plata; or united in great consolidated communal buildings, as in the Chaco Canyon and elsewhere.

The position of the kiva in relation to other rooms is the most important feature that separates the Mesa Verde from the modern pueblo type. The main characteristic of the latter type is diffuseness in the arrangement of rooms as compared with the compactness of the northern or more ancient form. The rectangular rooms of the modern type are arranged in rows separated by passageways, concentrated into pyramids or more generally in rectangles enclosing courts. This relation of the circular room has sufficient importance to indicate distinct types; a difference which is still further emphasized if we compare the structure of the roofs and its supports, or the ventilation and other openings in the floors of the Mesa Verde and the modern types.

The modern pueblo type is marked by isolated circular kivas and house masses, as seen in modern pueblos still inhabited and in the historic ruins in that region. We find also, as at Sia and Jemez, and elsewhere, rectangular rooms united with others serving the same purpose.

Two explanations have been advanced to account for this condition: (1) The original external kivas were destroyed by the Spaniards, and secrecy sought by hiding the room used for rites among the rectangular rooms. (2) A habit of performing rites in rectangular chambers in the midst of other rooms has been introduced by foreign additions. The former explanation throws light on the absence of kivas in several Rio Grande pueblos.

From what is said above it appears that the chief structural feature used in separating the two types of pueblos, known as the Mesa Verde and the modern, is the relative position of the circular kiva. The construction of the walls and roofs of circular rooms of the two types is characteristic. To this last feature a few lines should be devoted. The two kinds of circular kivas are distinguished as follows: (1) Those with a vaulted roof indicated by the remains of pilasters on which roof beams were supported and still to be found, even if the roof itself is wanting; and (2) circular kivas the roofs of which were flat, the rafters extending across the top parallel with each other, resting not on pilasters, but on the edge of the wall.

The kivas of Far View House, as in the majority of the kivas of cliff houses in the Mesa Verde, had vaulted roofs; but a few kivas, like those of Cliff Palace, had flat roofs. Accompanying the vaulted roofed circular kivas of Far View House was an elaborate interior construction for supplying fresh air, called the ventilator and deflector. In the flat-roofed kiva these constructions take another form, a description of which would take me too far afield at this time.

Far View House is only one of several types of open-sky buildings on the plateau. There is another more distinctly related in form to cliff dwellings. I refer, of course, to the mysterious structure called Sun Temple, brought to light from a pile of stones by the Smithsonian Institution in the summer of 1915.

In Sun Temple there is a circular kiva surrounded by rooms in the annex, at the west end, but the rooms of the main building surround a central court in which are two isolated circular kivas; there is also a fourth kiva a few feet outside the wall near the southeast corner. In other words, this structure shows in one building a combination of the compact type and the type with separated kivas. In structural details the kivas of Sun Temple resemble the second or flat roofed circular rooms of cliff houses and towers, the distribution of which, in the San Juan culture area, is wider geographically than the vaulted roofed form of Far View House. Sun Temple is a type of its own and must be looked upon as a highly specialized building. The nearest approach to it,

in form, are the 'towers' widely scattered throughout the San Juan culture area, a type imperfectly investigated. Provisionally I will designate this type as the Sun Temple type. Should it, as suspected, turn out on renewed study to be morphologically identical with towers, the term 'tower type' would suffice for both. Awaiting this needed field work, we may summarize by pointing out provisionally that there are three types of prehistoric buildings that have been clearly recognized on the Mesa Verde: (1) The Mesa Verde type; (2) the Sun Temple type; and (3) the tower type. The cliff dwellings and Far View House belong to the first of these.

In order to bring out in clear relief the differences between these prehistoric types and the archaic historic, or modern type, the following statements may aid the student.

A few references may first be made to rectangular chambers commonly called kivas, used by some of the modern pueblos for ceremonial functions. These rooms are not morphologically the same as circular kivas, but rather secular rooms adapted for religious functions. Among the Hopi these rectangular rooms are free from the houses; among some other pueblos embedded in them. The theory that ceremonial rooms of rectangular form are derived from the circular forms is not accepted by the author, but it is recognized that certain clans who once used free circular kivas now use rectangular ones. Clans that used a rectangular kiva at one time were not always too conservative to adopt a circular one, as we see from the evidence given below.

The Hopi rectangular 'kivas'[2] are isolated from house masses in the same way as the circular kivas of the modern type. One of the Hopi pueblos, called Hano, is inhabited by Tewa clans that came from the Rio Grande about 1770. The forms of the kivas of Tewan ruins in their old houses in the east are unknown, but, like Hano, had isolated kivas. Another foreign pueblo, on the Hopi East Mesa, called Sitcomovi, settled by clans from Zuñi, also has two rectangular kivas, situated in its court, separated from the rows of houses. There are no isolated kivas in modern Zuñi.

Unfortunately kivas have not been definitely identified in Hopi ruins, except at Küküttcomo and in a ruin in the Oraibi 'Wash' which, unlike the modern, are circular. Some of the clans use secular square rooms surrounded by living rooms, others have rectangular kivas separated from these rooms; a choice of position ascribed to the configuration of their mesas. In a court of the Hopi ruin, called Payüpki, whose builders were Tanoans who fled from the Rio Grande region in the decade 1680–1690, Victor Mindeleff records two isolated kivas, the

ground plan of each of which was a rectangular form. The Payüpki people returned to the Rio Grande about 1750 and were settled in Sandia and Isleta, both of which now have circular kivas.

The settlements along the Little Colorado, abandoned about the middle of the eighteenth century, had no circular kivas, as far as known, and the same is true of the ancient Zuñi settlements. The present pueblo Zuñi is of comparatively modern construction and its buildings show a comparatively late modern type. The form of the ancient kivas of the Zuñi region and their situation relative to house masses has not been observed, or at least has not been recorded by archæologists. The ceremonial chambers of modern Zuñi are rectangular, surrounded by rooms, a position that may have been chosen for secrecy or may be survivals of those in the Little Colorado settlements, to which some of the old Zuñi towns were related. When excavations are made in the round Zuñi ruins circular kivas may be brought to light, for Mota Padilla appears to refer to a kiva in the middle of the court of a circular ruin called Tzibola (Cibola).

Our knowledge of the forms of building on the Mesa Verde before the development of the pure types above mentioned is vague. It is possible that the earliest houses were not built of stone or other durable material but were subterranean and separated from each other. From these primitive buildings the more advanced types later developed, under the influence of 'cavern' life.

It is important to record that in the area in which the Mummy Lake mounds are now found there are depressions below the general surface suggesting subterranean pits, as if indicative of prepuebloan people whose homes were underground. Like pits have been described at the La Plata pueblo by Mr. Earl Morris, and similar structures near the mouth of the Gila were recorded by the early Spanish travelers as inhabited at the end of the sixteenth century. Evidences are not wanting to support the theory that early inhabitants of the pueblo region not only inhabited caves in the sides of canyons, but also used depressions in the earth, covered with roofs on a level with the surface of the ground. The call is urgent for renewed exploration on the Mesa Verde to enlighten us on the sites and form of prepuebloan huts.

[1] Published by permission of the Secretary of the Smithsonian Institution.

[2] The name kiva is applied by the Hopi to these rectangular rooms. I find no record of similar rectangular isolated kivas among inhabited pueblos on the Rio Grande, although they are universal in Hopi.

A DETERMINATION OF THE RATIO OF THE SPECIFIC HEATS OF HYDROGEN AT 18° AND −190°C.

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Communicated by R. A. Millikan, June 26, 1917

For the purpose of this study of the ratio of the specific heats of hydrogen the Lummer and Pringsheim method has been adapted to a one liter flask with a precision comparable with the extreme precision it has given in large carboys. The method consists essentially in obtaining γ from observation of the cooling consequent upon small adiabatic expansions by way of the ideal gas equation

$$(p_1/p_2)^{\gamma-1} = .(\theta_1/\theta_2)^{\gamma}.$$

The cooling was in this case measured by a thermal element of 0.001 inches copper and constantan wires, lightly brazed at the junction, which were introduced through glass tubes drawn out inside to fine capillaries and spread nearly to the diameter of the flask, the junction being carefully placed at the center. The thermal E.M.F. developed by the expansion was measured by a null method with a Wolff potentiometer, the galvanometer sensibility being such that an equilibrium temperature could be read to 0.0002°. Because of the finite heat capacity of the junction and the surgings incident to the expansion, it was necessary to observe the cooling at a constant interval of time after the expansion, the interval varying from 0.6 to 1.0 seconds in different series of observations. With a small container there is during this time a considerable inflow of heat to the thermojunction by conduction and convection and by radiation, the total inflow being a function of the time and the mounting of the individual junction. Since this inflow is also proportional to the cooling, the error due to it is eliminated by taking a series of observations for different excess pressures and plotting γ as a function of Δp atmosphere, (this must be linear for the expansions used, which did not exceed 0.04 atmosphere) and taking the limiting value of γ for $\Delta p = 0$ as the true γ.

The method was first tested for air with three different junctions which gave lines of quite different slopes, but intercepts

1.4014, 1.4019, and 1.4017.

To this value computed from the ideal gas equation must be added a correction for internal work, 0.0012, as obtained from either the Berthelot

or the Van der Waals equation of state (cf. Partington[3]). The final value 1.4029, is in close accordance with the three values which have been obtained by this method in carboys of 60 to 100 liters: Lummer and Pringsheim,[1] 1.4025; Moody[2] 1.4003; Partington,[3] 1.4032. (Moody's published value has been revised by the addition of the internal work correction and by the removal of the applied radiation correction since it was already included in the intercept.)

Data showing the same degree of uniformity were obtained in hydrogen with four different junctions, in two cases the same junctions as were tested in air. These gave respectively

1.4013, 1.4006, 1.4013, and 1.4017.

There being no theoretical correction in the case of hydrogen, the final value is 1.4012. This value, so closely, in accord with the kinetic theory, is contrary to the only previous evidence of weight, viz., 1.4084, the original determination by Lummer and Pringsheim, and 1.407 computed by Scheel and Heuse[4] from observations on C_p, which point to a quantum effect in hydrogen even at 18°.

The development of the method in a one liter flask opened the possibility of observation with a liquid air bath, at a temperature where determinations by Scheel and Heuse on C_p and by Eucken[5] on C_v have shown hydrogen to be virtually monatomic. By introducing a second low resistance potentiometer into the thermojunction circuit, it was found possible to make the thermometric arrangement quite as sensitive at –190° as at 18°, in spite of the fact that the thermal E.M.F. per degree is only about half as great. Observations are necessarily less precise because of the comparatively rapid change in temperature of the bath and the attendant difficulty in duplicating pressures. The final value of γ with the correction for departure from the ideal gas state added, 1.592 at a mean temperature of 82° A., is however probably correct to 0.005, and corroborates closely the work of Scheel and Heuse.

A full account of the work will appear in the *Physical Review*.

[1] Lummer and Pringsheim, *Ann. Physik, Leipzig*, **64**, (536).
[2] Moody, *Physic. Rev., Ithaca*, **34**, (275).
[3] Partington, *Physik. Zs., Leipzig*, **14**, (969).
[4] Scheel and Heuse, *Am. Physik, Leipzig*, **40**, (473).
[5] Eucken, *Berlin, Sitz Ber. Ak. Wiss.*, 1912, (141).

NOTE ON THE COEFFICIENT OF TOTAL RADIATION OF A UNIFORMLY HEATED ENCLOSURE

By W. W. Coblentz

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Communicated by R. A. Millikan, June 25, 1917

In a recent communication[1] I gave an estimate of the probable value of the coefficient of total radiation of a uniformly heated enclosure, or 'black body' (Stefan-Bolzmann constant).

It was shown that by making corrections for atmospheric absorption, the data obtained by various observers were of the order of

$$\sigma = 5.7 \times 10^{-12} \text{ watt cm.}^{-2} \text{ deg.}^{-4}.$$

My own value was given as

$$\sigma = 5.72 \times 10^{-12} \text{ watt cm.}^{-2} \text{ deg.}^{-4}$$

It was obtained by applying a correction of 1.2% for losses by diffuse reflection from the receiver used in evaluating the radiant power in absolute measure. Nine receivers were used, some of which were covered with lamp black and some with platinum black.

During the past winter further consideration was given (1) to the correction for the loss of energy by diffuse reflection from the receiver and (2) to the question of atmospheric absorption.

In view of the important conclusions arrived at by Millikan[2] as to the value of this constant, as the result of his further investigations of the value of the electron e, it seemed of interest to record herewith recalculations and new experimental verifications of the data on the radiation constant, just mentioned.

In the recalculations a correction of 1.2% was applied for losses by reflection from receivers covered with lamp black (soot) and a correction of 1.7% for losses by reflection from the receivers covered with platinum black. These corrections were determined by direct experiment upon some of the receivers and by comparison of the surfaces of the other receivers with samples of lamp black whose reflection losses had been determined in a previous investigation.[3]

The recalculated value is

$$\sigma = 5.722 = 10^{-12} \pm 0.012 \text{ watt cm.}^{-2} \text{ deg.}^{-4}$$

which is practically the same as previously obtained.

The correction for atmospheric absorption of dry air was determined by observing the transmission through a brass tube 6 cm. in diameter

and 51 cm. long, the ends of which were covered with windows of clear rock salt. This tube was evacuated with an oil pump and then filled with air which had been passed through phosphorous pentoxide. It therefore contained carbon dioxide which causes a small amount of absorption.

The transmission was determined by noting a series of galvanometer deflections caused by black body radiation (800° C.) which was passed through the evacuated tube and focused upon a linear thermopile of bismuth-silver. Immediately thereafter a stopcock was opened and either dried or undried air was permitted to enter, under atmospheric pressure.

Using air containing 9.95 grams of water per cubic meter the absorption amounted to 0.9 %.

Using dry air, the average value of the absorption (3 series of measurements) was 0.09%; which is the magnitude of the errors of observation. In view of the fact that in the measurements of the radiation constant, the column of (dry) air was less than 50 cm. if any correction was to be applied it could hardly be greater than 0.1%.

Millikan's calculations of σ, on the basis of recent determinations of Planck's element of action, h, from photoelectric measurements, and from his redetermination of the electron, e, is $\sigma = 5.72 \times 10^{-12} \pm 0.034$ watt cm. $^{-2}$ deg. $^{-4}$, which is exactly the result I obtained by direct measurement. The only misgiving is that this coincidence is accidental. In conclusion it may therefore be stated that further experiments are in progress in which the whole radiometric apparatus is in a vacuum, the radiator being an enclosure surrounded by molten metal.

[1] Coblentz, W. W., *Washington, Bul. Bur. Standards*, **12**, 1916, (533).
[2] Millikan, R. A., these PROCEEDINGS, **3**, 1916, (231).
[3] Coblentz, W. W., *Washington, Bull. Bur. Standards*, **9**, 1913, (283).

THE DEVELOPMENT OF A SOURCE FOR STANDARD WAVE-LENGTHS AND THE IMPORTANCE OF THEIR FUNDAMENTAL VALUES

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Communicated by G. E. Hale, June 27, 1917

The determination of wave-lengths in International Units forms an important part of the working program of a large number of laboratories. The primary International Unit is the absolute wave-length of the red cadmium line.[1] The secondaries are referred individually to the primary

standard and are thus freed from the systematic errors of the Rowland system.

For the main purposes which the new wave-lengths should serve, namely, the comparison of the wave-lengths in cosmical and terrestial sources, the determination of the constants in series formulae, and the investigation of the effect of changed terrestial conditions, thestandard and derived lines should be accurately reproducible and their wave-lengths should represent what may be considered the fundamental or unperturbed vibration of the emitting centers. Recent work at this observatory has shown, however, that the 6 mm.–6 amp. iron arc adopted as the source for International Secondary Standards,[2] while fulfilling to a moderate degree the first condition, gives for large classes of lines wave-lengths vitiated by pole effect.[3]

The investigation has involved the development of a standard source from which under ordinary working conditions both reproducible and fundamental wave-lengths are obtainable. The methods followed and the tests applied in its development are given in a Contribution from Mount Wilson Solar Observatory where the investigation will be reported in full.

The presence of pole effect in the International arc was shown by direct comparison between its center and the center of the proposed or provisional standard and also by comparing the wave-lengths of the International Secondaries belonging to groups *c* and *d* with their wave-lengths obtained from the new standard by means of the interferometer. The two independent methods gave the same result, namely, a systematic error of +0.006 A in the International wave-lengths of the lines of these two groups.

The importance of this result lies in the systematic character and the magnitude of the error, which is of the order of sun-arc displacements upon which our intimate knowledge of solar conditions must depend. If the solar wave-lengths of such lines are compared with the published International values, the solar lines apparently shift to the violet; but when compared with wave-lengths representing the fundamental vibrations, the same lines are displaced to the red. Such comparisons have already led and must necessarily lead to mistaken interpretations of solar phenomena.[4] The normal behavior of the great majority of iron lines, the stable lines of groups *a* and *b* is displacement to the red in the solar spectrum, whatever form of arc is used as the source. When the International or the 6 mm.–6 amp. Pfund arc is used, the iron lines of groups *c* and *d* are displaced to the violet and those of group *e* abnormally to the red, but with the proposed source the lines of these groups yield

displacements of the same sign and order of magnitude as the stable lines of groups *a* and *b*.

Tests of recently published wave-lengths of calcium and manganese show that they are contaminated by the disturbing influence of pole effect and it appears probable that little of the new work in wave-length determination is sufficiently free from this influence to meet the more and more exacting demands of the present and immediate future, for high accuracy in wave-length measurement.

We recommend that light be taken from a narrow equatorial zone of a 4 to 5 fold enlarged image of an iron arc of the Pfund type 12 mm. long carrying a current of 5 amperes. With such an arc the exposure time for the region λ 5600 to the violet is not excessive; for the region, λ 5600 to λ 6000, it is somewhat long when very high dispersion is required; but in this region the International arc is entirely lacking in stable lines and no element yields a sufficient number of lines of good quality here; from λ 6000 to the red the International Secondaries belong to group *b*. They are free from perturbing influences and for them any form of iron arc may be used as the source.

To obtain dependable wave-lengths of other elements the necessary preliminary is an examination for pole effect. If it is found to be present, a method for its elimination should be worked out and applied before attempting the wave-length measurements. As the method of the elimination depends upon the element, the problem of wave-length determination is no longer one of mere routine but offers opportunities for a real investigation.

[1] *Smithsonian Physical Tables*, p. 172.

[2] *Trans. Int. Union Coöp. Solar Research*, **4**, (59).

[3] St. John, C. E., and Babcock, H. D., *Mt. Wilson Contrib.*, No. 106; *Astroph. J., Chicago*, **42**, 1915, (231–262).

[4] St. John, C. E., *Mt. Wilson Contrib.*, No. 123, pages 11 and 27; *Astroph. J., Chicago*, **44**, 1916, (311–341), pages 321 and 337.

ON THE PRESENCE OF ALBUMOSES IN EXTRACTS OF THE POSTERIOR LOBE OF THE HYPOPHYSIS CEREBRI

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Communicated July 2, 1917

In recent years some very definite statements have been made in respect to the chemical nature of the active principle or principles of the hypophysis cerebri (pituitary gland). Among the claims advanced none are more sharply defined than those published by H. Fühner[1] on

behalf of the chemists of the research laboratory of the Farbwerke-Hoechst Company.

From extracts of the posterior lobe of the hypophysis, which had been freed of coagulable proteids,[2] these investigators obtained a mixture of crystalline sulphates of high physiological activity which was quite unjustifiably named 'Hypophysin.' What misconceptions may arise in connection with the use of this term 'hypophysin'—a designation for an unknown number of substances—may be seen when we read in a treatise on organotherapy that "Hypophysin is the chemically active pure posterior pituitary hormone, marketed as a sulphate."[3]

Further research enabled the chemists referred to to separate their 'hypophysin' into four unnamed crystalline fractions which, in respect to physical and chemical properties, are easily distinguished, the one from the other. These fractions are described in the following words:

1. A colorless, well crystallized sulphate which easily dissolves in water with neutral reaction and is difficultly soluble in alcohol, acetone and ethyl acetate. It is optically active (laevo-rotatory, $[\alpha]_D = -54.02°$) and carbonizes without melting when heated to a high temperature. The Pauly and biuret reactions are positive. With picric acid this substance forms a salt difficultly soluble in water.

2. There was also obtained a substance yielding a well crystallized, colorless sulphate which dissolves in water with faint acid reaction and is likewise difficultly soluble in alcohol, acetone, ethyl acetate, etc. The optical rotatory power of this preparation is $[\alpha]_D = -27.17°$; it decomposes when heated to 198–200°C. and gives the Pauly and biuret reactions. Contrary to the substance described under (1) this compound forms a picrate easily soluble in water. If this substance is brought into contact with alkalies a volatile amine base is at once liberated.

3. A third substance was isolated in the form of a crystalline, faintly yellow sulphate which, to be sure, is present in only very small amount. It is easily soluble in water and methyl alcohol with faintly acid reaction, and difficultly soluble in absolute alcohol, acetone and ethyl acetate. It turns the plane of polarized light to the left; its rotatory power is $[\alpha]_D = -39.25°$. On heating the substance decomposes at 185–186°C. The Pauly and biuret reactions are positive. With picric acid is obtained a salt easily soluble in water.

4. The mother liquor remaining after the fractional precipitation of the three substances just described yields, on cautious evaporation in a vacuum, a brittle, glassy, hygroscopic mass which dissolves easily in water and methyl alcohol, with difficulty in ethyl acetate and acetone. The solution of this substance shows an optical rotatory power of $[\alpha]_D = -21.26°$ and gives the Pauly but not the biuret reaction. Recently it has been found possible to

isolate also from the mother liquors a yellow, crystalline, neutral substance which dissolves easily in water and alcohol, with difficulty in ether, acetone and ethyl acetate. The preparation in aqueous solution rotates the plane of polarized light to the right ($[\alpha]_D = +5.99°$) and gives neither the Pauly nor the biuret reaction. The decomposition temperature is 95 to 96°C. The preparation gives with picric acid a compound difficultly soluble in water.

'Hypophysin' (the mixture of substances) is stated by Fühner to represent the physiological activity of the posterior lobe in respect to blood-pressure, respiration and uterine contractility and the sum of the actions of the four crystalline fractions equals that of the undifferentiated hypophysis.

Fühner's conclusions as to the pharmacological action of the four fractions are as follows:[4]

Fraction 1 has only a slight action on the respiratory apparatus and the uterus, but shows the typical action of 'hypophysin' on blood-pressure.

Fraction 2 has a pronounced action (*ausgeprägte Wirkung*) on blood-pressure, on respiration and on the uterus.

Fraction 3 behaves qualitatively like fraction 2 but has a more marked stimulating action on the uterus.

Fraction 4 (mother liquor and crystalline part) has an action on the uterus equivalent to that of fraction 3 but only a very slight action on blood-pressure and on respiration.

From his experiments Fühner draws the conclusion that the uterus-stimulating power of the hypophysis, which is, practically speaking, the most valuable property of extracts of the gland, resides, not in one, but in various (*verschiedene*) constituents of the organ, the condition being analogous to that found to hold for ergot.

From a chemical point of view, it is especially noteworthy that fractions 1, 2 and 3 give the biuret reaction as well as Pauly's reaction and that all three are laevo-rotatory; fraction 1, physiologically the least active, has the highest rotation, $[\alpha]_D = -54.02°$. It is easily demonstrated, we believe, that one or more albumoses (or polypeptids if the term is preferred) are present in the first three of these fractions and no doubt also in the fourth. We have found, as will be shown in a subsequent paper, that substances of the nature of albumoses can be isolated from many organs, It is these substances, which are themselves inactive, that give the biuret, the Pauly and other reactions of the so-called isolated principles and that account for their laevo-rotation.

The following analysis of one of the commercial preparations of the posterior lobe of the hypophysis is offered in support of our contention

that all those fractions of 'hypophysin' which give the biuret reaction contain albumose. The extract called 'Pituitrin' (Parke, Davis & Co.) was used first because it could be purchased in large quantities (½ oz. bottles).

The contents of 10 bottles, approximately 150 cc., were extracted three times with ether to remove a preservative (chloretone) and then concentrated under an electric fan on the water bath to a volume of 10 to 15 cc. A small amount of flocculent material which separated was removed by filtration, the filtrate was diluted with an equal volume of absolute alcohol and a solution of lead acetate was added, which induced a very slight precipitation. Addition of ammonia to faint alkalinity did not materially increase the precipitate, from which it was concluded that no appreciable amount of native proteid could be present, and that no advantage would accrue from the use of basic lead acetate, as in the procedure which will be described in a subsequent paper. An albumose, especially if of secondary nature, would escape precipitation by ammoniacal lead acetate. The alcoholic filtrate was freed from lead with sulphuric acid and the filtrate from the lead sulphate was concentrated to a small volume (a few cc.) and treated with an equal volume of saturated ammonium sulphate solution. The resulting flocculent precipitate, which was not very abundant, was washed with half-saturated ammonium sulphate solution, dried *in vacuo*, dissolved in water and again precipitated with an equal volume of saturated ammonium sulphate solution. The precipitate was again washed with half-saturated ammonium sulphate solution and treated with an excess of barium hydroxide. After filtering off the barium sulphate, the filtrate was heated on the water bath until all the ammonia had been expelled and the excess of barium was removed with sulphuric acid. The filtered solution was then concentrated to a very small volume and dropped into absolute alcohol, ether being added until no further precipitation was produced. The substance here thrown out, which was small in bulk (0.010 g.), was of less interest to us than the fraction presently to be described. It gave the Millon and biuret reactions, as well as that of Pauly. Knoop's reaction for histidine was negative.

The filtrate from the half-saturation with ammonium sulphate as described above was saturated with finely powdered ammonium sulphate, which caused the appearance of a gummy precipitate so characteristic of albumoses when treated in this manner. The precipitate was filtered off, washed with saturated ammonium sulphate solution, dissolved in water and treated with barium hydroxide in the usual manner. The solution, freed from ammonia and the excess of barium, was concentrated to a very small volume and dropped into absolute alcohol, the precipitation being completed with ether. The substance thus thrown out was collected as completely as possible and dried *in vacuo;* 0.031 g. in 1 cc. of water in a 0.5 dcm. tube showed a rotation of $-1.38°$, whence $[\alpha]_D = -89°$. When the substance was reprecipitated with hot absolute alcohol its specific rotation was found to be $[\alpha]_D = -77.6°$.

As to its properties, the substance must be classed with the secondary albumoses. It has been shown to be precipitated by saturation of its solution with ammonium sulphate. It is non-coagulable on boiling, gives the biuret reaction (hemi-biuret) very beautifully, as also Pauly's reaction, while Knoop's bromine reaction for histidine is entirely negative. This negative result with Knoop's reagent excludes the presence of histidine as an admixture in our substance.[5] Picric acid added to an aqueous solution gives a precipitate. The addition of Millon's reagent gave rise to a slight turbidity, but on boiling the characteristic red color was not obtainable. In this respect there was entire agreement with a secondary albumose which we have isolated from the mucosa of the small intestine. The ninhydrin reaction was positive when made in the usual manner but we are confident that this is due to the fact that adherent amino acids were not entirely removed. To do this would have required several reprecipitations with ammonium sulphate.

The albumose here described was found to have a quite negligible action when tested with the virgin cat's uterus.

A secondary albumose which was prepared by digesting fresh thyroid glands of the pig behaved in every respect like the above substance. Its specific rotation was found to be $[\alpha]_D = -88.1°$, while that of the pituitary albumose varies from $-78°$ to $-89°$.

We come now to the filtrate from the complete salting out with ammonium sulphate. This was freed from ammonium sulphate in the usual manner. Reduced to a small volume, the solution was dropped into absolute alcohol, the precipitation being completed with ether as with the preceding substances. The material thus obtained was readily soluble in water and gave the usual response of pituitary extracts when tested on the isolated uterus of the virgin guinea pig. It also still gave the Pauly and the biuret reactions, though with greatly lessened intensity as compared with the original solution. It fails to give Knoop's reaction for histidine so that we must conclude that this amino acid does not exist as such in any considerable amounts, if at all, in pituitary extracts. The rotation of 0.0381 g. in 1 cc. of water in a 0.5 dcm. tube was found to be $-0.52°$, from which $[\alpha]_D = -27.4°$. We have here a substance which may be compared with respect to physiological activity, rotation and chemical tests with Führer's 'hypophysin' fractions 2 and 3. The solution used in the polarizing tests was saturated with powdered ammonium sulphate and again the characteristic precipitate of albumose was produced, though, naturally, it was not abundant this time. Evidently we have here the remnant of albumose which remained in solution after the first saturation with ammonium sulphate. If we were to apply this salting out to larger quantities of pituitary extract it is possible that more or less of a true peptone would be found in the filtrate. Certainly, there always remains in the filtrate, even after two

saturations, a substance which gives a pink biuret reaction. Only more extended research can show to what extent this is albumose and to what extent peptone.

We believe that there is no mystery attaching to the constituents of pituitary extracts that have been shown to give positive Pauly and biuret reactions *and a negative Knoop's bromine reaction.* These constituents must be classed as albumoses (and even peptones) and the German chemists are to be congratulated if they have obtained them in the form of physiologically active crystalline sulphates, as has been stated on their behalf by Fühner, even though they have failed to recognize their proteid nature.

We have made qualitative tests with other commercial pituitary extracts (Armour's Pituitary Liquid, and Solution Pituitary Extract, Mulford) and have found, as was to be expected, that albumoses can be salted out from all of them. The amount of proteid material present varies considerably in these preparations—one of them (Armour's Pituitary Liquid) appears to have only a trace of that form of proteid (coagulable proteid plus primary albumoses) which gives a precipitate with potassium ferrocyanide and acetic acid and to have practically all its biuret-yielding substance in the form of secondary albumose.

As to the total amount of biuret-yielding material present in the extract analyzed—and by this we mean the substance or substances that give the biuret reaction *immediately at room temperature* (and not, as histidine gives it, after heating)—we believe that we are close to the truth when we say that it cannot be far below 10% in weight of the total solid matter. The dry residue from five bottles of extract (73 cc.), exclusive of chloretone, was found to be 0.415 g. The amount of albumose, primary (?) and secondary, recovered from ten bottles (in the analysis described above), with a dry residue of 0.830 g., was approximately 0.050 g. The losses, at a conservative estimate, could hardly have been less than 0.025 to 0.030 g. On this basis we should have had, in the specimen analyzed, close to 10% of biuret-yielding material.

It may be of interest to state here that when the dry residue of five bottles (0.415 g.) *was heated in a boiling water bath for one hour in 10 cc. of 25% hydrochloric acid the biuret reaction*[6] *disappeared entirely* but the Pauly reaction for histidine was still obtained. The disappearance of the biuret reaction after boiling with hydrochloric acid can only be interpreted as due to hydrolysis of our albumose.

After having completed our examination of the American pituitary preparations we learned that the 'hypophysin' of the Hoechst chemists could be ob-

tained in this country. We accordingly purchased two hundred 1 cc. ampullæ of this product, which is described on the labels as a "sterile solution, 1:1000, of the isolated active substances from the glandula pituitaria." We did not inquire if a preservative is used in its preparation, as the presence of a substance of this character would hardly interfere with the isolation of a proteose.

The residue from ten ampulles, as obtained by evaporation at a low water-bath temperature under an electric fan and subsequent drying over sulphuric acid, amounted to 0.0154 g. The dry residue from the two hundred 1 cc. ampulles would therefore have weighed 0.3080 g. One hundred and ninety cubic centimeters, that is to say, the total quantity of solution with the exception of the 10 cc. used for the estimation of the dry residue, were concentrated on the water bath under the fan to a volume of 2.5 cc.[7] and saturated with finely powdered ammonium sulphate. The characteristic gummy precipitate of salted-out albumose immediately collected on the stirring rod and on the sides of the tube containing the solution. The precipitate was washed with saturated ammonium sulphate solution, decomposed with barium hydroxide and the freed albumose was precipitated as a "sulphate" with absolute alcohol and ether in the manner already described. Dried over sulphuric acid, the albumose thus obtained weighed 0.017 g. The reactions were those already described—a beautiful pink biuret, a positive Pauly and a negative Knoop reaction. Potassium ferrocyanide and acetic acid also failed to give a precipitate, showing that coagulable proteids and primary albumoses were not present.

The ammonium sulphate filtrate from the gummy albumose precipitate still gave a fine pink biuret reaction, as was the case also with all the American preparations under the same conditions. The addition of a drop or two of a very concentrated solution of trichloracetic acid to this filtrate caused an immediate precipitation of gummy droplets. These give the biuret reaction with great intensity and represent a further yield of albumose with a probable admixture of peptone and traces of other substances. It may be stated in this connection that the ammonium sulphate filtrates of our American preparations also give with trichloracetic acid a precipitate which is indistinguishable in its reaction from that obtained with hypophysin. A certain amount of albumose or peptone still remains in these ammonium sulphate filtrates even after the use of trichloracetic acid.

The Hoechst preparation is no doubt a clean product and certainly contains less dry residue than the products prepared in this country. We have seen that the dry residue of ten 1 cc. ampulles of hypophysin was 0.0154 g. The dry residue of ten 1 cc. ampulles of Armour's Pituitary Liquid was 0.0242 g. and in its relatively smaller content in proteoses this prepration more nearly approaches hypophysin than any other examined by us. It is to be understood that we are not criticizing these products because they happen to contain more or less albumose.

This in itself is of no consequence, as this albumose does not appear to be toxic.

The points that we wish to emphasize are these:

1. Carefully prepared commercial extracts of the posterior lobe of the hypophysis contain albumoses.

2. Hypophysin, stated to be a mixture of the "isolated active substances of the pituitary gland," is likewise contaminated with albumoses.

3. All claims in respect to the isolation of pure principles, as made by the Hoechst chemists, must be looked upon, in view of our findings, as being without foundation.

Other considerations also lend support to the last statement.

One who is familiar with the high activity for the virgin uterus of fresh extracts of the hypophysis can only agree with Fenger when he asserts that the as yet unknown constituent of this gland which affects the uterus so powerfully cannot be less potent than β-imidoazolylethylamine, and may be even more powerful. Fenger says that an acidulated methyl alcohol extract of the posterior lobe of the hypophysis, for which no claim to chemical purity can be advanced, "showed a uterine-contracting power somewhat stronger than pure β-I."[3] If we examine the tracings given by Führer in his experiments with the Hoechst products, experiments in which quantities varying from 0.05 to 0.5 mgm. were tested on the guinea pig's uterus in a 100 cc. bath of Locke's solution, it will be seen that these products are much weaker than β-imidoazolylethylamine. Here again is evidence that the crystalline salts of the Hoechst chemists represent mixtures of active and inactive principles and not pure chemical individuals.

Further evidence that Hypophysin does not consist of chemically pure principles is given by the pharmacological tests made with it in this laboratory. Dr. D. I. Macht has kindly compared the oxytocic strength of the preparation with that of Armour's Pituitary Liquid, this having been selected from among the American products because it most nearly approaches Hypophysin in respect to dry matter and a low albumose content. He reports that the Armour product, which makes no pretense of being a pure chemical principle, is "several times more powerful in its action on the virgin uterus of the guinea pig than Hypophysin." There is no reason to assume that the Hypophysin used in Dr. Macht's tests had lost any of its original strength as the labels on the packages give no hint of instability or loss of strength with time.

The question naturally arises whether the albumose or other proteose here shown to be present in all active pituitary extracts is not itself the uterine stimulant. The secondary albumose which was isolated by us

from "Pituitrin" was practically devoid of an oxytocic action, as has already been stated. Investigations on the bio-chemistry of the intestinal and gastric mucosa which we hope soon to publish also lend no support to the theory that pituitary extracts contain an active albumose. We have prepared a water-soluble powder from this mucosa which is highly active for the guinea pig's uterus (1:1,000,000) and for the intestinal strip (1:250,000), which induces a distinct rise of blood-pressure in the cat, and which in respect to its chemical reactions, its behavior towards ammonium sulphate and polarized light, *is indistinguishable from a diluted pituitary extract.* The similar behavior in these several respects of gastric and intestinal 'motiline' solutions and pituitary extracts first led us to suspect that these latter also contain albumoses. Now, in the case of these intestinal preparations, we have had sufficient material on hand for the application of purification processes. *We finally emerged with a secondary albumose which was entirely devoid of oxytocic, pressor, depressor or secretory action.* It is this experience, together with our discovery than an inactive albumose can be prepared from the ordinary pituitary extracts, as already stated, which fortifies us in our belief that the proteoses of the gland have nothing whatever to do with the physiological activity of the organ.

It is not our purpose to consider here the literature[9] pertaining to the 'peristaltic hormones' that are known to occur in almost all, if not all, organs of the body, It is worthy of note, however, that an extract of the gastric or intestinal mucosa can be prepared, as we have already stated, which has a pressor action for the circulation and a marked oxytocic power in a concentration of 1:1,000,000. This powerful action points strongly to the conclusion that here also, as in the case of pituitary extracts, we are dealing with a motiline which, in a state of chemical purity, would be fully as active as β-imidoazolylethylamine. And this again leads us to the supposition that the oxytocic principle (or motiline) of the hypophysis is not a hormone or substance specific to this organ, but is rather a widely distributed substance, everywhere the same, which may have its origin in the various tissues, in the gastric or intestinal mucosa, or which may be absorbed as such from among the products of digestion. We hope that our discovery of the contaminating and difficultly separable proteoses in physiologically active extracts will pave the way to the solution of these problems.

We cannot conclude this communication without adding a few words in regard to the presence of proteoses in the various tissues of the body. Proof of their existence in pituitary extracts has been given and reference has been made to their presence in gastric and intestinal extracts. By

the use of certain methods which will shortly be described in detail we have found that a secondary albumose (to name only a single proteose which is sharply differentiated from all native proteids and primary proteoses) can be isolated in small amounts from all of the cellular tissues of the body thus far examined. Skeletal muscle appears to contain albumose in the smallest amount, gastric and intestinal mucosa contain it even after four days' starvation, *much more during digestion of a meat meal*, while organs like the thyroid gland contain much more, weight for weight, than skeletal muscle. We have not as yet been able to isolate definitely a true proteose of any sort from the plasma of the blood, though able to show that the cellular elements of the blood yield a readily demonstrable amount of albumose.

It was not originally our purpose to study these proteoses or to isolate them from the various tissues, but finding them always present in our final products whenever we attempted to isolate certain 'hormones,' such as the intestinal motiline and secretine, even when our methods of treating the tissues could not have produced them, we were forced to undertake a study of methods for their separation from the hormones. A future communication in these PROCEEDINGS will deal with this question.

Conclusions—1. Secondary albumoses and possibly peptones (or polypeptids if the term is preferred) were found to be present in all of the therapeutically used extracts of the posterior lobe of the hypophysis cerebri that were examined. To what extent the proteose content of the gland may have been increased by autolysis or by processes incidental to the manufacture of the extracts it is impossible for us to state. We believe, nevertheless, that the perfectly fresh, bloodless glands yield proteoses, inasmuch as we have actually isolated such substances from the thyroid gland and other organs when taken from the animal immediately after bleeding it to death.

2. The 'Hypophysin' of the Farbwerke-Hoechst Company is not, as claimed for it, "a solution of the isolated active substances of the pituitary gland" but a mixture of albumoses (and possibly peptones) with varying and unknown amounts of active and inactive constituents of the gland. The albumoses present in 'Hypophysin' account fully for the chemical reactions (such as the biuret and the Pauly reactions and the left-handed rotation) which are stated to characterize the pretended active principles. The albumoses as separated from pituitary extracts are devoid of action upon the uterus. In view of the facts here presented it must be evident that the active principles of the hypophysis cerebri have not yet been isolated as chemical individuals.

[1] Fühner, H., *Zs. ges. exp. Medizin, Berlin,* 1, 1913, (397).

[2] "Vollständig von Eiweiss befreite Auszüge aus den Hinterlappen von Rinderhypophysen." Fühner, *loc. cit.*, p. 399.

[3] Harrower, H. R., *Practical Hormone Therapy,* New York, 1914, p. 460. A Glossary of Terms.

[4] Fühner, *loc cit.*, p. 443.

[5] Cf. Aldrich, T. B., *J. Amer. Chem. Soc., Easton, Pa.,* 38, 1915, (203).

[6] The biuret reaction was made at room temperature. Unfortunately we did not apply heat as is done when making this test for histidine. This substance was no doubt present, having been set free from the albumoses by hydrolysis, as is shown by the positive Pauly test.

[7] This also contained the redissolved residue from the 10 cc. used for the estimation of dry matter.

[8] Fenger, F., *J. Biol. Chem., Baltimore,* 25, 1916, (417).

[9] Cf. Ott, Enriquez and Hallion, Zuelzer, Weiland, Köhler and others.

ON THE RÔLE OF THE THYMUS IN THE PRODUCTION OF TETANY

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Communicated by J. Loeb, June 30, 1917

In a number of experiments on larvae of *Amblystoma punctatum* and *A. opacum* the influence of thymus was studied. It was found that the effect of thymus upon these animals is not exactly similar to its effect on tadpoles of frogs or toads. With regard to this difference the most conspicuous and important effect of the thymus feeding in Salamander larvae is the occurrence of severe tetany in the thymus-fed larvae.

Up to the present, 67 specimens of *A. punctatum* and *A. opacum* were fed exclusively on Thymus after they had reached an age of about six to fourteen days. In each single individual tetany was produced.

For several days previous to the occurrence of the acute attacks, the animals are less active and their appetite is diminished. The acute stage appears in two forms: a mild form and a severe form. The mild form manifests itself by clonic convulsions of the hind limbs and the tail, while the severe form consists in clonic convulsions of the entire system of muscles. In the beginning of the tetany period, the attacks exhibit the characteristics of the mild form, then become severe and towards the end of the entire period again are mild. Each acute attack is followed by a tonic spasm of the entire body, during which the body is stiff for a short time. After several acute attacks the hind limbs become permanently twisted and stiffened and are to some extent paralyzed. Later on the forelegs and in severe cases even the neck and spinal cord become paralyzed.

The severity depends to a great extent on the temperature in which the animals are kept. In low temperature the acute tetany begins later and is of shorter duration than in high temperature In high temperature some animals succumb from the attacks; those which survive metamorphosis never show a complete recovery. In low temperature recovery may be complete and the animals can live after metamorphosis for many months and reach an age of at least fourteen months, even if they are kept on exclusive thymus diet after metamorphosis.

It should be particularly emphasized that no matter in what temperature the animals are kept, acute attacks cease a short time before metamorphosis and never occur in metamorphosed animals.

The symptoms of the attacks caused by thymus in the larvae of *A. punctatum* and *A. opacum* are the same as in the attacks observed in mammals after parathyroidectomy and the theory that they are caused by the same agent as *Tetania thyreopriva* suggests itself.

It would seem then, that thymus contains the substances which cause tetany and excretes them into the body, from which they are removed by the parathyroids. Extirpation of the latter would thus cause tetany.

If this is true, we should expect that thymus-feeding would produce tetany only in such species as have no parathyroids. In fact tetany in thymus-fed tadpoles has never been reported; these animals develop the parathyroids several days after hatching. The Salamander larvae however, have no parathyroids and the occurrence of tetany in thymus-fed larvae of that species corresponds with our expectations.

Furthermore, according to such a theory, thymus-fed animals which suffer from tetany should become free from it as soon as they develop parathyroids. The Salamanders develop parathyroids when they go into metamorphosis, which is actually the time that acute tetany ceased in our larvae.

Although many experiments must still be made to clear the entire problem—some of which are already in progress—for the present, the above theory might be valuable as a working hypothesis.

EVIDENCE OF ASSORTIVE MATING IN A NUDIBRANCH

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Communicated by E. L. Mark, June 11, 1917

In man there is found, according to Pearson and others, a slight but appreciable degree of positive correlation between the members of mating pairs as regards their stature and certain other characters. For Paramecium a similar, but higher, correlation was proved by Pearl (1907) to exist between the lengths of members of conjugating pairs. Jennings (1911) substantiated Pearl's discovery that in Paramecium large individuals are usually found mated with large, small individuals with small, and made more certain the conclusion that this correlation (homogamy) is due to real assortive mating, as Pearl had previously maintained.

This matter of assortive mating, which may have various important implications for evolution, appears not to have been studied in animals other than Paramecium and man. With reference to characters concerning the size of the organism, at least, it should, of course, be possible for assortive mating to take place only when there is available some physical basis for the required process of selection. Hence, although echinoderms and some other marine animals appear to congregate at their times of breeding, and may even be conspicuously disposed in pairs (Orton), it is not to be expected that invertebrates practicing external fertilization would, in general, yield any evidence of assortive mating. Among gastropods, however, the case is different, and notably so with nudibranchs. In the latter animals, which are hermaphroditic, a true copulation of two individuals seems a prerequisite for fertilization of the eggs. In some nudibranchs the male and female genital openings, two or three in number, situated on the right side of the body, are separated by a considerable distance, and the behavior of the animals in copulation shows that it is necessary for the 'male' and 'female' openings of one individual to be brought simultaneously into close relation with the appropriate openings of another (e.g., in Cenia, as described by Pelseneer, 1899).

Other nudibranchs, such as chromodorids, have the reproductive openings concentrated upon a single small papilla; but, in some cases, at least, their behavior during the maneuvers preliminary to actual copulation strongly suggests that here also there is a rather well-defined, though not absolute, mechanical necessity for equality in the sizes of

the individuals forming a successful mating pair. Reciprocal fertilization may be presumed to occur in most of these cases, and is certainly carried out in some instances.

The observation of mating pairs of Chromodoris zebra repeatedly suggested that under natural conditions this species forms copulating pairs of which the individual components closely correspond with each other in general size. Sexually mature specimens of this species range in length from 4 to 18 cm. Differences in the relative sizes of the various individuals are readily detected by the eye, while other dimensions of the animals may be subjected to measurement in the living condition.

One hundred and forty-eight pairs of copulating C. zebra were obtained in the field. The total length (anterior edge of the mantle to posterior termination of the foot) was determined for each specimen, according to a method[2] giving results sufficiently reproduceable for the purposes of statistical treatment. As a check upon this measurement certain other dimensions were ascertained, including the weight and the volume. Each of the methods of evaluating size yields the same qualitative result. Therefore the estimations of total length according to the procedure employed may be relied upon as a criterion of assortive mating with respect to size. In figures 1 and 2, summarizing the observations upon mating pairs, total length measurements are used.

Figure 1 is a regression plot showing the correlation between the lengths of individuals and the average lengths of their mates, as found under natural circumstances. If the correlation were perfect, the observed points would lie upon line "(1)"; if no correlation were to be detected the regression line "(2)" would coincide with m–m'; actually the degree of correlation between the lengths of individuals composing copulating pairs is of about the same magnitude as that found in cultures of Paramecium containing a mixture of pure lines.

Laboratory experiments were carried out with over 400 specimens, embracing about 200 individuals originally obtained in pairs, and an approximately equal number of "single" nudibranchs. About 50 specimens, ranging in length from 4 or 6 to 16 or 18 cm., were placed in each of a number of 9-gallon aquaria supplied with running water. After two days the mating pairs noted in each aquarium were removed, and measured. Data were in this way obtained from 119 pairs, which had come together under such purposely contrived circumstances that true random mating might easily take place, since so many individuals were crowded together in a small space; while if assortive mating is in any degree a real condition of copulation it should still make its influ-

ence sufficiently evident. Measurements of these pairs are plotted in figure 2. The correlation between the members of mating pairs is in fact better than in the previous case (fig. 1); reasons for this will be discussed in a subsequent paper.

A study of the behavior of Chromodoris supports the view that there is exercised an active selection of mating partners. As a rule, two animals greatly differing in size do not successfully copulate. This has

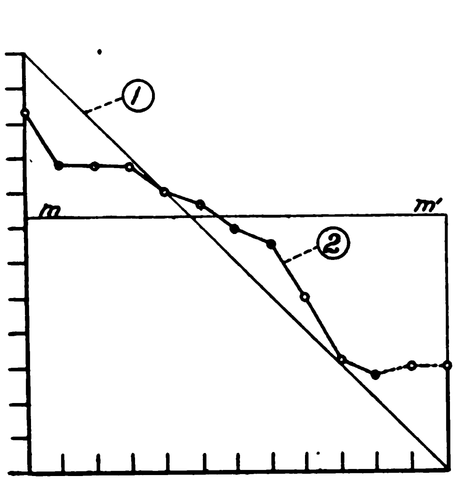

Fig. 1

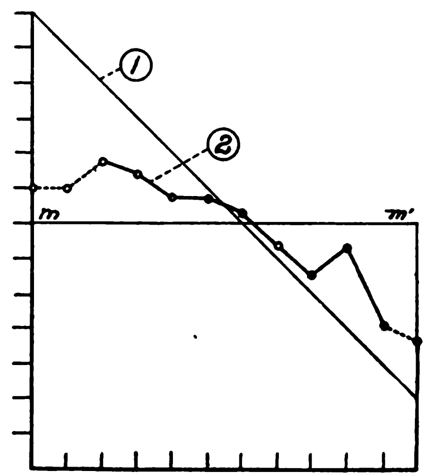

Fig. 2

FIG. 1.—ILLUSTRATING THE CORRELATION IN SIZE BETWEEN THE MEMBERS OF 148 PAIRS OF CHROMODORIS ZEBRA COLLECTED IN THE FIELD

The thin continuous line (1) gives the lengths (as ordinates) for the different classes of individuals in order of decreasing size. The heavy line (2) gives the average lengths of the mates of the individuals of these classes. The unit is 1 cm; m—m' is the mean for all.

FIG. 2.—ILLUSTRATING THE CORRELATION IN SIZE BETWEEN THE MEMBERS OF 119 PAIRS OBTAINED IN LABORATORY EXPERIMENTS

The thin continuous line (1) gives the lengths (as ordinates) for the different classes of individuals in order of decreasing size. The heavy line (2) gives the average lengths of the mates of these classes. The unit is 1 cm; m—m' is the mean for all.

been verified by experiments in which the size of some individuals has been artificially reduced through starvation. The physical basis of assortive mating in Chromodoris is probably found in the relative attitudes assumed by the conjugants and in reactions to tactile (and chemical?) stimuli which determine these attitudes.

Two suggestions may be made regarding the possible significance of

assortive mating in Chromodoris. If the population is composed of a mixture of pure lines, then one effect of this type of copulation may well be, as in Paramecium (Jennings), the prevention of interlinear crossing. Certain generally accepted ideas regarding the life history of nudibranchs may tend to favor this view. The evidence for the presence of pure lines in the Chromodoris stock is, however, entirely inferential. It would, indeed, be almost impossible to obtain good evidence upon this point, unless, possibly, through a study of the rate of segmentation of the eggs; but the eggs of C. zebra are not well adapted for this work, and it is very doubtful if such evidence could be made conclusive.

Another, and, I believe, at present better founded, suggestion concerning the effect of assortive mating is based upon the fact that the size of the egg-masses, and the number of eggs in each ribbon, as well, probably, as the number of egg masses deposited by each animal during a single season, increase directly with the size of the individual. On grounds of physiological economy—remembering that mutual fertilization is involved, and remembering also that each animal deposits a number of egg-masses at each spawning season—it may be argued that the mating of large individuals is an influence tending to increase the number of larvae beyond that which would result from random pairing. In some other nudibranchs assortive mating, if it occurs, may have a different, or an additional, significance.

Summary.—Mating pairs of the nudibranch Chromodoris zebra are found to exhibit a rather high degree of correlation between the sizes of the two members. This is due to assortive mating, which may constitute an important influence tending to increase the numbers of larvae.

[1] Contributions from the Bermuda Biological Station for Research, No. 70.

[2] It was necessary to remove the animals from the water and place them, dorsal surface downward, upon a glass plate.

CORAL REEFS OF TUTUILA, WITH REFERENCE TO THE MURRAY AGASSIZ SOLUTION THEORY

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Communicated June, 22, 1917

Tutuila, Samoa, is a purely volcanic island without elevated coral reefs or limestones. It is surrounded by a recent fringing reef which forms a mere veneer over the modern off-shore marine platform, and extends a short distance seaward, its precipitous outer edge being from

5 to about 20 fathoms deep. In former times, the island may have been submerged about 20 fathoms below its present level, this being suggested by the fairly uniform depth of about 20 fathoms off the mouths of the harbors of the northern coast, while these harbors themselves have the appearance of drowned valleys.

However this may be, the latest movement of the island has been an emergence of about 8 feet above present high-tide level; for a platform about 8 feet above high tide juts out to seaward from the base of practically every promontory. The shores are strongly cliffed, some of the sea-cliffs being 500 feet high.

Being volcanic and densely forested from summit to shore, it was thought that rain-water falling upon the island might become so augmented in acidity as to dissolve the shoreward parts of the surrounding coral reef. This, however, is untrue. The rain-water is indeed acid, ranging from 0.126 to 0.678×10^{-6}. However, the streams and springs of the island are usually slightly *alkaline*, due to bicarbonates; the average of 11 of the principal streams and 6 springs being 7.19 PH., or 0.645×10^{-7} hydrogen-ion concentration, and the range from 0.25×10^{-6} to 0.38×10^{-7}.

An analysis of the water from Faagalu stream below the water-falls was made by Prof. Alexander H. Phillips, and shows a high percentage of chlorine, and bicarbonates and very little ammonia, or nitrates. The calcium, magnesium sodium and potassium are more than sufficient to hold the bicarbonates, HCO_3, in an ionic state.

The following is an abstract of Professor Phillip's analysis of Faagalu stream water.

Sample taken April 14, 1917

	Parts per million		*Parts per million*
Free Ammonia	0.036	Magnesium (Mg)	2.20
Albuminoid ammonia	0.028	Sodium (Na)	6.19
Nitrogen in nitrites	none	Potassium (K)	2.16
Nitrogen in nitrates	0.04	Bicarbonate (HCO_3)	9.60
Oxygen consumed	2.05	Sulphate (SO_4)	4.06
Total solids	70.06	Chlorine (Cl)	12.00
Silica (SiO_2)	26.55	Nitrate (NO_3)	.17
Iron(Fe_2O_3) are Alumina (Al_2O_3)	0.95	Phosphate (P_2O_5)	.026
Calcium	3.03	Hydrogen ion concentration of the water 0.5×10^{-7}.	

It is evident that the calcium is too great in proportion to the sodium to have been derived from the salt spray, and must therefore have come from the rocks or the decaying vegetation of the Island.

In Oahu, Hawaiian Islands, also, the streams are alkaline, for Dr.

C. Montague Cooke, Jr. of Honolulu, kindly transported me in his automobile to eight of the larger streams and springs, between Palolo and Monanalua valleys, and these were all alkaline, ranging from 7.1 to 8.12 PH.; the average being 7.34 PH. or 0.457×10^{-7}. The higher alkalinity of the streams of Oahu may be due to the presence of elevated limestones, these being absent from Tutuila. Thus the surface waters draining off from Tutuila, and Oahu, being alkaline, cannot dissolve limestones by reason of their 'acidity,' and the Murray-Agassiz theory of solution of the shoreward parts of reef flats by fresh water is not supported.

The openings in coral reefs opposite the mouths of streams are due to the fact that corals cannot survive and have never grown, in these places, due to silt and dilution in time of flood. This was proven at Tutuila by placing 26 specimens of 12 species of Acropora, Pavona, Psammocora, Porites, Pocillopora, Fungia, and Coeloseris, 150 feet from the mouth of Pago Pago brook in a place where the bottom is covered with fine brown volcanic mud and no corals are found. The salinity in this situation was observed to range between 31.38 to 25.48; that of the open sea about being 34.83. Yet the corals survived in this diluted water, for fifteen days, although the Acroporas did not expand. However all but one species were killed by the freshet due to the torrential shower of 4.3 inches on April 12, 1917, which reduced the salinity to 0.93, becoming only 9.25 at the end of 24 hours. All the corals died with the exception of 2 out of 3 specimens of massive *Porites* which withstood the dilution and silt but with apparent injury. This species of *Porites* lives nearer to streams mouths and closer to the shore than does any other coral of Samoa.

It is apparently uninjured by being placed for an hour in water of a constant temperature of 36° C., which would be fatal to all the *Acropora, Pocillopora* and dominant off-shore corals of the reefs.

On the reef flats of both Murray Island, Australia, and Tutuila, Samoa, coral heads are most densely clustered in relatively quiet water about 150 to 200 feet shoreward from the region wherein the surges die out in ordinary weather.

The greatest variety of species of corals are, however, found just where the surges die out in ordinary weather.

In both Murray Island, Australia, and Tutuila, Samoa, four genera constitute over 90% of the coral heads of the reef flats. Thus:

NAME OF CORAL	PERCENTAGE OF CORAL HEADS	
	Murray Island, Australia	Tutuila, Samoa
	per cent	*per cent*
Porites	38	47.4
Acropora	18	33.6
Pocillopora	10	4.01
Psammocora	Very rare	10.0
Seriatopora	25	0.0
Totals	91	95.1

It was found that when shallow reef-flats are impounded and cut off for about an hour from the open ocean at low tide, the water quickly becomes nearly twice as alkaline in places as in the open ocean, while in other places it becomes abnormally reduced in alkalinity. The increase in alkalinity is caused by the photo-synthetic action of plant cells within the corals and by sea weeds.

Rail falling directly into the sea has far more effect in reducing the alkalinity of the surface water than has stream water pouring outward from the shore. Yet the torrential rain of 7.8 inches in about five hours on March 19 reduced the surface alkalinity only from 8.20 to 8.18 PH., and the salinity from 34.69 to 30.46 off the landing-stage of Blacklock's wharf near the inner end of Pago Pago Harbor, and the alkalinity of the harbor remained about 34.33 despite the average rainfall of more than one inch per day from January 1 to April 17.

The scouring of sand from their floors by currents is a potent factor in deepening the shoreward parts of the reef-flats, and may result in changing a fringing reef into a barrier reef. The lithothamnion ridge lying along the seaward edge withstands this process of disintegration for by growing, it resupplies such loss and maintains itself about 6 inches above low tide level. The corals growing over the shoreward parts of the reef-flats also tend to replace the lost limestone and some reefs may thus maintain themselves as fringing reefs, as at Aua, Pago Pago Harbor, while others as at Black's Bay, Tutuila, become deepened near shore so as to change into barrier reefs. At Aua the current over the reef-flat ranges from 20 to 62.8 feet per minute, and is an effective transporter of coral sand; spilling it into deep water off the northern edge of the reef. This scouring process is doubtless facilitated by the holothurians which are well known to be sand swallowers. There is on the average one of these animals for every 8.6 square feet of reef flat off Aua, Pago Pago Harbor.

It is our hope to return to Samoa in 1918 to remeasure and re-weigh corals, and thus determine their growth rate, and to bore through the coral reef, study the question of the existence or non-existence of a submerged marine platform, and evaluate the sand-carrying ability of currents over the reef flats, and of solution due to holothurians. The results may then be published by the Carnegie Institution of Washington.

It is a pleasure to express our gratitude to Hon. Josephus Daniels, Secretary of the Navy; and also to His Excellency, Governor John M. Poyer, Commander U. S. N., and his officers who did all in their power to facilitate our studies.

NATIONAL RESEARCH COUNCIL

SUGGESTIONS RELATING TO THE NEW NATIONAL ARMY BY THE ANTHROPOLOGY COMMITTEE OF THE NATIONAL RESEARCH COUNCIL

The recruiting of a large army from the diversified elements of the national population must present certain contingencies in which Physical Anthropology may be of much practical service, and at the same time should afford many opportunities by the utilization of which this and related branches of science may greatly benefit.

1. Examination of Recruits.—The examination of recruits for admission into the new Army will include certain observations which, if properly systematized and made by simple, accurate, standardized instruments, should prove of great statistical value. Unhappily the methods followed today and the instruments by which the measurements are taken lack in uniformity as well as in accuracy. Unless a few necessary improvements are made in both, the great body of data derived from the examination of a million or more men cannot be utilized by science with full confidence; and as properly revised regulations would not increase, but actually diminish the burdens of the examiners, the Committee on Anthropology takes the liberty of urging immediate consideration of the question of revision. Suggested modifications of the present examination blanks and specifications for simple outfits of instruments, together with directions for the examiners in making measurements and physical observations have already been submitted by the Committee to the National Research Council.

In this as well as in other recommendations which the Committee has made the utmost care was taken not to add to, but rather to reduce the burdens of the medical examiners and the medical service of the army. No additional tasks at this time unless of the highest practical importance would be justifiable or feasible.

2. *Modification of Stature Requirements.*—The present minimum requirements of stature, in any branch of the army or the navy, is 5 feet, 4 inches. In the case of mountain artillery it is 5 feet 8 inches.

The minimum for the English infantry and some other branches of the service prior to the present war was 5 feet, 2 inches, and it has since been reduced. On the Continent the minimum differs with the nationalities, but is as a rule lower than that of the United States. In many of these nationalities the average height of the adult male does not reach, barely equals, or only slightly surpasses the minimum requirement for the soldier of the United States. Many of these nationalities are well represented in this country. They include the Italians, Greeks, French, Mexicans, Spanish, Swiss, the Russian and Austrian Jews, many of the Slavs, the Magyars, Roumanians, Lithuanians, and even Germans. Should the present minimum in stature for the United States Army and Navy be rigidly adhered to, from one-fourth to one-half of the men belonging to or descending from the nationalities mentioned would be excluded by this rule alone, thus resulting in serious disadvantages, the chief among which would be that of placing a disproportionate burden in the formation of the army on the naturally taller native American.

In view of the above facts, and as small stature in a large majority of cases signifies normal fluctuation and not any weakness or degeneration, as has been repeatedly proved by the 'bantam' regiments of England and other short stature troops of European countries, the Committee recommends that the minimum stature requirement for the new United States army be reduced, for all branches of the service, to 60 or at most 62 inches; and that corresponding with this, the minimum weight requirement be reduced from 128 to 120 pounds.

3. *Further Anthropometric Work for Statistical and Scientific Purposes at the Concentration Camps.*—The sixteen or more concentration camps will afford a unique opportunity for anthropometric observations, one object of which would be to obtain data regarding the normal physical conditions of the American people of different descent, admixture, education, social class, occupation, and environment.

To utilize these opportunities the minimum requirement would be the selection of six of the camps representing the northeast, southeast, the northern middle states, the south, the southwest, and the northwest; and placing in each a specially trained young medical officer who would devote his time to the anthropometric work.

The investigations, reduced to the minimum, would consist of the measurement of stature and height sitting; of the three principal dimensions of the head; of two diameters of the face; of two diameters of the chest; and of more precise observations on the color of the skin, eyes, and hair than are practicable at the recruiting stations.

The training and equipment of the six medical officers would be undertaken by the Department of Anthropology of the United States National Museum.

The data procured would be elaborated without cost by the Statistical Staff of the Prudential Insurance Company of America. Publication of the results would be facilitated as far as possible by the Smithsonian Institution.

The total period of the investigations could be limited to six months if found advisable. The officers required for conducting the observations should be appointed by the medical departments of the army and the navy. Their special training at the United States National Museum would require one month.

4. *Material for Future Scientific Research.*—The organization of the new army will afford important opportunities for additional scientific research and the collection of data and specimens which should be utilized as far as practicable. Efforts in this direction were made in the Northern Army of the United States during the Civil War; the results are embodied in three volumes of data by Baxter and Gould, and in the collections of the Army Medical Museum. According to available information, more or less extended scientific researches are being conducted and illustrative collections made at the present time in connection with nearly all the armies of Europe.

The United States Army will include not only men of many nationalities, but also those of different races, such as Indian, Negro, Filipino, and possibly Japanese and Chinese. Even under the best hygienic conditions and without actual participation in war a certain proportion of these must be expected to become ill and die in hospitals. The bodies of such dead cannot, as in peace, be transported hundreds or thousands of miles, perhaps, to their friends, but must be cremated or buried in the vicinity of the hospitals. These bodies offer a valuable opportunity for postmortem determinations, such as the cause of death as found at autopsy, the weight of the different internal organs, etc., and also for assembling specimens which would be of the utmost value to future pathological, anatomical and anthropological investigation. The Army Medical Museum and the United States National Museum would gladly take charge of the preparation and distribution of such material.

For the above purposes it is requisite that in each of the more important hospitals one member of the medical staff, preferably a pathologist or an assistant pathologist, be designated to gather needed records and specimens; and it is earnestly recommended that such a detail be made immediately on the establishment of each large army or navy hospital.

W. H. Holmes, *Chairman*,
C. B. Davenport,
F. L. Hoffman,
G. M. Kober,
Ales Hrdlicka,
Madison Grant,
E. A. Hooton,
T. A. Williams.

NATIONAL RESEARCH COUNCIL

FIRST REPORT OF COMMITTEE ON BOTANY

The first work of this Committee was to make a census of competent investigators and of the problems under way. Botanists were also asked to suggest additional problems bearing upon public welfare, and numerous suggestions were received. In the main these are long-time problems, very important to continue, but not expected to yield immediate practical results.

When the United States entered the war, a large number of emergency problems arose. The larger number of these had to do with the available sources of plant materials and products. Many of them were questions asking for information, rather than problems needing investigation. Dr. E. M. East, a member of the Committee, was asked to take charge of this phase of the work. Finally it seemed best to establish a Special Committee on Raw Products, with Dr. East as Chairman, which is in close co-operation with the Committee on Botany. The Commercial Museum of Philadelphia, with its unusual facilities as a source of information in reference to raw products, is also working in co-operation with this Special Committee.

The number and nature of the questions in reference to forest materials and products soon made it evident that this group of problems should be provided for in a special way, and accordingly Prof. Irving W. Bailey of the Bussey Institution was asked to take charge of this phase of the work. These problems involve such an overlapping of Botany and Forestry, and many of them are of such practical importance that it has been recommended that a Sub-Committee on Forestry be organized, which shall bring into close co-operation the Forestry Service and the Committee on Botany.

Problems Undertaken.—Among the emergency problems under way, the following have progressed far enough to be encouraging.

1. Dehydration of vegetables. A method to preserve flavor.

2. Dehydration of fruits. A method being developed chiefly in the western fruit area as a measure of conservation.

3. Sources of rubber. A problem well toward solution.

4. Improvement of sugar beets. A breeding problem undertaken by several investigators and well advanced.

5. Disease-resistant cereals. A long-time problem but advanced far enough to expect that some disease-resistant strains will soon be available.

6. Drought-resistant cereals, especially corn and sorghum.

7. Native drug plants. Information in reference to native plants from which needed drugs may be obtained is in charge of Dr. Henry Kraemer of the Philadelphia College of Pharmacy. A great many medicinal plants have been suggested, but comparatively few have been tested.

8. A substitute for cotton in the manufacture of explosives. It is necessary for this substitute to consist of nearly pure cellulose, and to be easily obtainable in vast quantities.

9. The food reserves in certain plants.

Co-operation.—Co-operative relations have been established with the Committee on Agriculture of the National Research Council, the U. S. Department of Agriculture, the Botanical Division of the Carnegie Institution, research laboratories of universities, agricultural experiment stations, botanic gardens, the Council on Pharmacy and Chemistry of the American Medical Association, the Cereal Conference, and the Association of Seedsmen. The purpose of this co-operation is the exchange of information in reference to problems under way, and in reference to pressing problems that have been encountered; and also the sharing of facilities for investigation. It should be said that information in reference to problems under way is not intended as a notice of preemption, the feeling being that several attacks upon a problem may bring speedier results.

Additional Activities.—Efforts are being made to:

1. Effect contact with manufacturers who may need information as to plant materials and products, and who may not know the most speedy way of obtaining it.
2. Effect contact with farmers so that they may receive and apply the information and advice that is accumulating.
3. Make some of the results commercially available, without specially favoring any commercial organization. Some of these results would ordinarily involve patents or trade secrets.

JOHN M. COULTER, *Chairman.*

NATIONAL RESEARCH COUNCIL

MEETINGS OF THE EXECUTIVE COMMITTEE

The sixteenth meeting of the Executive Committee was held at the offices of the Council in New York City, April 4, 1917. Messrs. Chittenden, Dunn Pupin, and the Secretary were present.

The Secretary announced the appointment of Mr. Lewis B. Stillwell to the Council. It was voted that Mr. Stillwell be appointed a member of the Engineering Committee.

The Secretary also announced the appointment of Dr. Joseph S. Ames and of Dr. Hollis Godfrey to the Council; the resignation of Dr. Hale as Chairman of the Committee on Research in Educational Institutions, and the acceptance by Dr. Chittenden of that chairmanship.

Mr. Dunn reported a gift of five thousand dollars from Mr. Edward D. Adams to the Engineering Foundation.

Dr. Chittenden presented a letter from certain members of the staff of the Sheffield Scientific School, bringing up several very practical questions on the metallurgy of brass, particularly for the use of the Army and Navy. After discussion, the letter was referred to the Secretary, to take the matter up with General Crozier, and possibly with others.

The seventeenth meeting of the Executive Committee was held at the offices of the Council in Washington, April 11, 1917. Messrs. Hale, Stratton, and the Secretary were present. Dr. Walcott was consulted by telephone and approved the proceedings. There was also present Dr. MacCallum, of the Canadian Research Council.

Professor R. W. Wood, of Johns Hopkins University and Professor Arthur Gordon Webster, of Clark University, were nominated members of the Council.

A Food Committee was authorized, Dr. Hale to name the Chairman and the personnel of the Committee.

Dr. Hale announced the appointment of Professor H. H. Donaldson of the Wistar Institute, as Chairman of the Anatomy Committee. This appointment carries with it appointment to the Council.

A Committee on Psychology was authorized, Dr. Hale to name the Chairman, who will be a member of the Council.

Dr. Millikan was elected a Vice-Chairman of the Council.

The eighteenth meeting of the Executive Committee was held at the offices of the Council in Washington, April 16, 1917. Messrs. Carty, Chittenden, Conklin, Hale, Noyes, Pearl, Walcott, and the Secretary were present.

This meeting was called at the suggestion of Dr. Noyes to discuss the proposed relations of the National Research Council with the State Research Committees of Councils, which may be appointed in different states of the Union. The immediate occasion was the appointment of a State Research Committee in California. It was voted to telegraph to the State Research Committee of California, an expression of a desire to co-operate with them, and to follow this telegram with a letter of explanation.

It was voted to request the President of the National Academy of Sciences to appoint Mr. John R. Freeman, and Professor John C. Merriam members of the Council.

The action of the Military Committee in appointing a Noxious Gas Committee was approved.

An Optical Glass Committee was authorized, with Dr. Millikan as Chairman.

Mr. John R. Freeman was appointed a member of the Engineering Committee.

The nineteenth meeting of the Executive Committee was held at the offices of the Council in Washington, May 3, 1917. Messrs. Carty, Dunn, Hale, Noyes, Pupin, Stratton, and the Secretary were present.

Dr. Pupin spoke of the urgent necessity for further information on the status of the submarine detection investigation. He urged that all members of the Council who are working on the matter be informed regarding the tests which have already been made and that opportunity be given to take

part in preliminary tests, in order to learn the conditions surrounding the problem. Dr. Carty spoke of the work that the Western Electric Company and the General Electric Company are doing in connection with the Submarine Signalling Company.

The twentieth meeting of the Executive Committee was held at the offices of the Council in Washington, May 24, 1917. Messrs. Dunn, Hale, Millikan, Stratton, Vaughan and Walcott, were present. Messrs. Durand, Mendenhall and Pegram were also present upon invitation. Dr. Bogert, after election as a member of the Executive Committee, was also present.

It was decided that Dr. Millikan, in view of his position as Vice-chairman of the Council, should be regarded as an ex-officio member of the Executive Committee. The vacancy in the Committee thereby created was filled by the election of Dr. M. T. Bogert, Chairman of the Chemistry Committee.

Upon motion it was voted that all sub-committees of the Council be appointed by the Executive Committee upon recommendation of the Chairmen of Committees.

Report was made upon the present difficulty in obtaining release from England of Austrian and German scientific journals addressed to societies and individuals in the United States. The Chairman thereupon was requested to present the facts to the State Department with an expression of the desire of the National Research Council to have steps taken to secure the release and uninterrupted transmission of such periodicals, if this can be done without conflict with other obligations.

Dr. Stratton, Chairman of the Committee on Census of Research, reported progress in the distribution of Census Blanks, stating that for this purpose institutions in the United States had been classified as follows: 1. Educational; 2. Industrial; 3. Other than Educational and Industrial. The necessary expenses of the work of the Committee were considered and the Chairman further reported that within a few days the question of indexing and rendering available the information now being received would be considered.

Upon motion it was voted that the appointment of the following Mathematics Committee be confirmed, with the understanding that three additional members be added to this Committee upon nomination by the National Academy of Sciences· Professor E. H. Moore, Chairman; Messrs. E. W. Brown, F. R. Moulton, R. S. Woodward, E. B. Wilson, George D. Birkhoff.

Upon motion of Dr. Millikan the appointment of a Sub-committee on Visibility, of the Physics Committee, was confirmed. This Sub-committee consists of Dr. Herbert E. Ives, Chairman, and Mr. Irving G. Priest.

The Chairman of the Council reported that Dr. W. H. Howell had been appointed as Vice-chairman of the Physiology Committee to serve as Chairman of the Committee during the absence of Dr. Cannon.

Dr. Victor C. Vaughan, Chairman of the Committee on Medicine and Hygiene, reported that he had appointed a Sub-committee on Psychiatry

under the chairmanship of Dr. Stewart Paton, with the following membership: Messrs. Pearce Bailey, Albert M. Barrett, Henry A. Cotton, Charles L. Dana, Walter E. Fernald, Adolf Meyer, Thomas W. Salmon, Elmer E. Southard, Roy M. VanWart, William A. White. Upon motion it was voted that the appointment of this Psychiatry Committee be confirmed.

Dr. Vaughan also reported that the Secretary of the Interior has permitted Dr. Franz at St. Elizabeth's Hospital to train men for rehabilitation work. He also stated that Dr. Murlin has been asked to make observations and report upon the food and rations which are issued to soldiers in training at Plattsburg.

Upon motion it was voted that a Sub-committee on the Sterilization of Water, with Dr. Phelps as Chairman, be appointed.

Dr. Vaughan further reported that he had been asked to serve as Chairman of the Research Committee of the General Medical Board of the Council of National Defense. He stated that the Surgeon General has consented that recruits in concentration camps be examined by experts instead of by individual medical officers, involving the necessity for the appointment of an Examining Board of the Army. Upon motion it was voted that the Chairmen of the Anatomy, Psychology, Psychiatry, and other related committees, be recommended for appointment on such an Examining Board, together with Colonel Bushnell, who is already a member.

The Chairman was requested to appoint a committee to report on cooperation between the National Research Council and the Research Committees appointed by the various states. The following committee was appointed for this purpose: Dr. C. E. Mendenhall, Chairman, Messrs. Bogert, Durand, W. B. Hale, and Vaughan.

It was voted that the sum of $100 be allotted to the Psychology Committee.

Upon motion of Dr. Bogert the appointment of a Potash Committee, with Dr. J. D. Pennock, General Manager of the Solvay Process Company of Syracuse, N. Y., as Chairman, was authorized. Recommendations were submitted with regard to the membership of this committee.

The Chairman of the Council read a letter from Dr. Ames, Chairman of the Foreign Service Committee, and reported upon the activities and recommendations of this committee.

The Chairman of the Council reported the organization and appointment of a special New York Committee on Submarine Warfare, the Chairman of which is Dr. Nicholas Murray Butler, and the Executive Secretary, Dr. G. B. Pegram of Columbia University. Upon motion, this action was approved and confirmed.

The suggestion was made that the letterhead of the Council be changed to read "National Research Council, Acting as the Department of Science and Research of the Council of National Defense." This question was referred to the Chairman of the Council with power.

The Chairman reported the resignation of Mr. Tod Ford as Assistant Secretary of the Council, due to his acceptance as a member of the Lafayette Squadron in France. Mr. Dunn reported the election of Mr. Walter M. Gilbert as Assistant Secretary of the Engineering Foundation and that the Carnegie Institution of Washington had courteously detailed him for such work. Mr. Gilbert was thereupon elected as Assistant Secretary of the National Research Council.

Upon motion it was voted, subject to change, that subsequent meetings of the Executive Committee be held weekly on Thursday at 9 a.m.

The twenty-first meeting of the Executive Committee was held at the offices of the Council in Washington, May 28, 1917. In the absence of the Chairman of the Executive Committee, the meeting was called to order by the Chairman of the Council. Messrs. Hale, Millikan, Stratton, Vaughan, Walcott, and Welch were present. Mr. Manning, Chairman of the Noxious Gas Committee, and Messrs. Mendenhall and Durand were also present upon invitation.

Mr. Manning read a report of the Noxious Gas Committee, making recommendations for the establishment of a laboratory for the study of problems related to this subject.

Dr. Millikan presented a report of the Optical Glass Committee, stating that recommendations had been made to the General Munitions Board, had received its approval, and in turn had been recommended to the Council of National Defense.

Upon motion of Dr. Walcott the action of the Optical Glass Committee was approved and confirmed.

The Chairman of the Council stated that a French Scientific Mission would soon reach the United States and a discussion followed regarding its reception and means for its entertainment.

The twenty-second meeting of the Executive Committee was held at the offices of the Council in Washington, May 31, 1917. In the absence of the Chairman of the Committee, the meeting was called to order by the Vice-Chairman (Dr. Millikan) of the Council. Messrs. Bogert, Chittenden, Millikan, Pearl, Pupin, Walcott, and Welch were present. Messrs. Durand and Mendenhall were also present by invitation. Dr. Hale entered the meeting before its close.

The Vice-Chairman (Dr. Millikan) of the Council reported:

That a sum of $5000 has been donated by Mr. Cleveland H. Dodge for the general purposes of the Council. and that an additional sum of $100 has been received from another source; and that these funds have been placed in the hands of the Treasurer of the National Academy of Sciences for the use of the Council.

That the President of the National Academy of Sciences has appointed the following additional members of the Council:

Dr. ARTHUR L. DAY, Geophysical Laboratory, Washington, D. C.
Professor CHARLES E. MENDENHALL, Munsey Building, Washington, D. C.
Mr. F. H. NEWELL, University of Illinois, Urbana, Illinois.
Dr. STEWART PATON, Princeton, New Jersey.

After discussion, upon motion, it was

RESOLVED, That all bills for services, supplies, and incidental expenses pertaining to the work of the National Research Council be approved by the Chairman or by a Vice-Chairman of the Council and forwarded to the Treasurer of the National Academy of Sciences or to the Engineering Foundation for payment from funds available for the use of the Council.

Upon motion it was also voted that similar approval be, and hereby is, given for the payment of bills from funds which may be available for the use of the Council at any time through any other organization.

The Vice-Chairman read a letter from the Chairman of the Chemistry Committee with respect to the advisability of the appointment of a Sub-Committee on the Chemistry of Forest Products. Upon motion, Dr. John E. Teeple was appointed Chairman of such a Committee.

The Chairman of the Council suggested that it may become advisable to appoint a special committee on nautical instruments. Upon motion, the Chairman was authorized to appoint such a committee if it may prove advisable to do so after consultation with members of the General Munitions Board and of the Shipping Board.

It was decided upon motion, to have embossed the new letterhead to be used on stationery for the general offices of the Council and for its committees. The appointment of a special committee to consider other questions of detail with respect to the office business of the Council, was approved.

The twenty-third meeting of the Executive Committee was held at the offices of the Council in Washington, June 21, 1917, and was called to order by the Chairman of the Council. Messrs. Bogert, Hale and Stratton were present. Messrs. Durand and Mendenhall were also present upon invitation.

The Chairman reported:

That Mr. Edward D. Adams has donated the sum of $5000 to the Engineering Foundation for the use of the National Research Council.

That Mr. Martin A. Ryerson has donated the sum of $1000 for the use of the Council and this sum has been deposited to the credit of the Council in the Riggs National Bank, Washington.

That the Carnegie Corporation of New York at its meeting of June 4, 1917 authorized a grant of $50,000 to the Carnegie Institution of Washington for purposes of the National Research Council, with the understanding that disbursements on account of this donation will be made at the discretion of the President of the Institution.

That Dr. Day, Mr. Mendenhall, Mr. Newell, and Dr. Paton have accepted appointment as members of the Council.

That at the request of the General Munitions Board, a special Committee on Navigation and Nautical Instruments has been appointed with Dr. L. A. Bauer, Director of the

Department of Terrestrial Magnetism of the Carnegie Institution of Washington, as Chairman, and the following as additional members:

Commander J. S. DODDRIDGE, U. S. Naval Observatory, representing the Bureau of Navigation.

Mr. ROY Y. FERNER, representing the U. S. Bureau of Standards.

Mr. R. L. FARIS, representing the U. S. Coast and Geodetic Survey.

It is probable that other members of the Committee will be appointed.

Upon motion it was then

RESOLVED, That the Executive Committee of the National Research Council desires to express to the trustees of the Carnegie Corporation of New York its appreciation of the generous and timely assistance which has been provided for purposes of the Council by means of an appropriation of $50,000 to the Carnegie Institution of Washington to be disbursed through said Institution for such purposes.

Consideration was given to the question of the organization of the Council, and of representation in its membership of various scientific activities. Upon motion, the Chairman was requested to appoint a special committee to consider all such questions and report thereon at a subsequent meeting of the Committee. Dr. Bogert was named as the Chairman of this Special Committee and requested to confer with the Chairman of the Council with regard to its membership.

The Chairman presented letters from the Chairman of the Botany Committee and from Dr. E. M. East of the Bussey Institution relative to the appointment of a special committee on Botanical Raw Products. He also presented a letter from Dr. Earle H. Clapp, Chairman of the Committee on American Forest Research of the Society of American Foresters, urging the appointment of a special committee on forestry. After discussion, the Special Committee of the Council on Organization and Representation, of which Dr. Bogert has been appointed Chairman, was requested to give early consideration to questions raised by the suggestions and recommendations just submitted, with particular reference to the relation of work on forest products to other activities of the Council.

Upon recommendation of the Chairman of the Anthropology Committee, Dr. Charles B. Davenport, Cold Spring Harbor, New York, was appointed a member of this Committee in place of Dr. Tom A. Williams, who has been detailed for foreign service.

Dr. Mendenhall submitted the following report of the Committee of the Council appointed to consider questions of relations to State Councils of Defense:

The Committee appointed to consider the question of the organization of state research committees and their relation to the National Research Council reports as follows:

In the opinion of the Committee the matter of the usefulness and value of state research committees is in the experimental stage at present, so that it is undesirable for the National Research Council to take the positive attitude of urging the formation of such committees. It seems best to allow developments to take place in accordance with local needs, but to take

the precaution of outlining a form of procedure to recommend to the state councils of defense in case the organization of research committees is decided upon.

The Committee therefore recommends that a letter be sent to the proper officer of all the state councils of defense which have not already organized research committees, embodying the points considered below, and that a letter offering the complete co-operation and assistance of the National Research Council be sent to those state councils which have already organized research committees.

The National Research Council has carefully considered the question of the general organization of State Committees or Councils of Research, and has come to the conclusion that such organization should be determined by local needs and conditions. Local causes, such as the development or investigation of natural resources or the proper development of the use of research methods in industries, or the correlation of industries with research laboratories already existing at educational institutions, may make it very desirable to organize such state research committees. In fact, the present emergency offers, in some respects, an unusual opportunity for improving the correlation of industry and research, and it is highly desirable that those concerned with this development should not only consider it with reference to the emergency but should also plan for the permanent continuance of any research committees which may be formed.

If such a committee is decided upon, the National Research Council is desirous of co-operating in every possible way and suggests the following scheme of organization: Committees should be organized under the State Council of Defense (or similar official body) and at least their general expenses provided for by the state: they should be representative of the universities and other leading educational institutions through members chosen from their scientific and engineering research departments and also of those industries in which applied science plays the most important part. In choosing the personnel of the Committee in accordance with this general scheme, it is recommended that it include some representation from the committees on research of the universities of the state. Such a State Research Committee would be chiefly active through subcommittees assigned to consider particular questions, but should have a permanent secretary to handle correspondence and through whom close contact could be maintained with the National Research Council.

Upon motion, this report was adopted, and the recommendations contained therein approved.

CARY T. HUTCHINSON, *Secretary.*

NOTICES OF BIOGRAPHICAL MEMOIRS

The following biographical memoirs has been published by the Academy since the last notices of such memoirs appeared in the December, 1916, number of the PROCEEDINGS.

ALFRED MARSHALL MAYER (1836–1897). By ALFRED G. MAYER and ROBERT S. WOODWARD. *Biographical Memoirs of the National Academy,* **8,** pp. 243–272.

This Memoir follows the life-work of Alfred M. Mayer according to the outline: Ancestry, Early Self-education, Friendship with Joseph Henry, Successive College Positions, varied scientific interests and publications, 'Mayer's Law for Human Audition', Interest in Natural History, Magnetic Studies, Foreign Visits, Inactive Middle Period, and Fruitful Close of Life. A bibliography of 76 titles concludes the Memoir.

EDWARD SINGLETON HOLDEN (1846–1914). By W. W. CAMPBELL. *Biographical Memoirs of the National Academy*, **8**, pp. 347–372.

This Memoir recounts the life-work of Edward S. Holden: Early Education, West Point and Army Service, Appointment at the U. S. Naval Observatory, the Great Nebula in Orion, Popular Articles and Textbooks, Librarian in U. S. Naval Observatory, Work at the University of Wisconsin, Solar Eclipse of May 6, 1883, Transfer to the Lick Observatory, and Up-building of its Staff, Photography of the Moon, Librarian of the U. S. Military Academy at West Point. The numerous titles of his contributions cover 15 pages.

VOLUME 3 SEPTEMBER, 1917 NUMBER 9

PROCEEDINGS

OF THE

NATIONAL ACADEMY OF SCIENCES

OF THE

UNITED STATES OF AMERICA

Publication Office: Williams & Wilkins Company, Baltimore
Editorial Office: Massachusetts Institute of Technology, Cambridge
Home Office of the Academy: Washington, D. C.

Entered as second-class matter at the Post Office at Baltimore, Md., under the Act of August 24, 1912

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Volume 3 SEPTEMBER 15, 1917 Number 9

HELIOTROPIC ANIMALS AS PHOTOMETERS ON THE BASIS OF THE VALIDITY OF THE BUNSEN-ROSCOE LAW FOR HELIOTROPIC REACTIONS

By Jacques Loeb and John H. Northrop

ROCKEFELLER INSTITUTE FOR MEDICAL RESEARCH, NEW YORK CITY

Communicated, July 25, 1917

While it was customary to express animal instincts in terms of human behavior, one of us many years ago began to replace this anthropomorphic method by the objective and quantitative methods of the physicist. These methods were most easily applicable in the case of those instincts familiar to every layman in which animals were apparently attracted or repelled by light. Loeb[1] was able to express the effect of light in these cases in the following terms: Certain animals are compelled automatically to orient their body in such a way that symmetrical elements of their photosensitive surface are struck by light of the same intensity. In that case the tension and energy production in the symmetrical muscles of both sides of the body are equal and there is no reason for the animal to deviate from this direction of its motion. If, however, the symmetrical photosensitive elements (e.g., the eyes) receive unequal illumination the tension or energy production of the symmetrical muscles is no longer the same and the animal is automatically turned until its orientation is again such that symmetrical photosensitive elements receive the same amount of light.

It was obvious from the observations that this reaction was a function of the constant intensity of light and Loeb assumed that it was a photochemical effect and that the function was probably the law of Bunsen and Roscoe, whereby the effect equals the product $i\,t$, where i is the intensity of light and t the duration of illumination.[2]

The proof for the correctness of this view was furnished for the heliotropic curvatures of the hydroid *Eudendrium* by Loeb and Ewald[3] and by Loeb and Wasteneys.[4] The authors could show that if the intensity of light was lowered the time required to call forth the heliotropic curvatures of the polyps had to be increased in such a way as to keep the product $i\ t$ constant.

A second proof was furnished by Ewald[5] who showed that when constant illumination was replaced by an intermittent one the same effect could only be produced when the product of time of exposure and intensity of intermittent light was the same as that of a constant light. Ewald worked on the orientation of the eye of *Daphnia* to light. This crustacean turns its eye to the light and when the eye is under the influence of two lights of equal intensity the eye is turned in a direction at right angles to the line connecting the two lights. By keeping the one light constant, the other intermittent (through rotating a disk with a sector cut out in front of it) Ewald found that the two lights acted in an equal way when the product $i\ t$ in both cases was equal.

The experiments of *Eudendrium* as well as Ewald's experiments are tedious and it seemed desirable to have a simpler method for the verification of this law. Bradley M. Patten[6] in working on the heliotropic reactions of the larva of the blowfly (which is negatively heliotropic) determined the path of the animals under the influence of two different sources of light striking the animals simultaneously. Theoretically the animal should creep in such a direction that the intensity of illumination on both sides of its photosensitive elements should be equal, and Patten could prove that for each ratio of the two sources the path was a definite one. By rotating a wheel with a sector cut out before one source of light and cutting down the intensity of the other by a slit Patten could also show that indeed the heliotropic effect is determined by the product of intensity into duration of illumination.

"Using the apparatus described, one of the beams of light was cut down by a diaphragm and the other by an episcotister, so that the light coming from one side was a steady beam of low intensity, and that from the opposite side an intermittent beam in which bright flashes alternated with darkness. The apertures in the sector wheel were adjusted so that the amount of light from each source was equal for a unit time. It has already been established that when the larvae are subjected to equal steady beams of light from opposite directions the aggregate response is almost precisely at right angles to the line connecting the sources of light. The average angular deflection of 200 trails at equality was only 0.09°, when the degrees represented a distance of

but 1.5 mm. If the Bunsen-Roscoe law holds for the phototactic response of the larvae, they should orient perpendicularly to the rays of light when subjected to the action of steady and intermittent lights of equal energy per second. The experimental results based on 136 trails made under these conditions show an average angular deflection of but 0.07° from the perpendicular. These results seem to show that in the blowfly larva the phototactic reaction follows the Bunsen-Roscoe energy law."[7]

It seemed desirable to extend the proof for Loeb's theory of animal heliotropism and especially for the validity of the Bunsen-Roscoe energy law to other forms of animals, and we selected for the purpose the reactions of the larvae of the barnacle which were already utilized by Groom and Loeb[6] in their early experiments on the transformation of positively heliotropic animals into negative ones and vice versa.

These larvae move in a straight line towards or away from a single source of light, and when two lights of equal intensity are given they move in a line at right angles to the line connecting the two lights. These animals are small and can be obtained in large numbers. They were made to collect in the corner of a dish with a little sea water and were then sucked up into a pipette which was blackened with the exception of the opening. When such a pipette is put into a glass dish with parallel walls whose bottom is black (by putting paraffin blackened with lamp black at the bottom of the dish) the larvae will flow out in a fine stream and swim when they are positively heliotropic in a straight line towards the source of light. They thus form a rather narrow white trail on the dark bottom and it is possible to measure the angle of this trail with the line connecting the two lights. In this way in each observation the trail of thousands of individuals is measured. By using one constant and one intermittent source of light and comparing the results with those obtained by two constant lights we can test the validity of the Bunsen-Roscoe law.

The method of the experiments was as follows: *a b c d* (fig. 1) is a square dish of optical glass with blackened bottom and containing a layer of sea water. *A* and *B* are two lights, the intensity of which is determined by a Lummer-Brodhun contrast photometer. In front of each light is a screen with a round hole permitting a beam of light to go to the dish. The lights and the dish *a b c d* are so adjusted that the two beams of light striking the sides *a b* and *b c* at right angles cross each other in the middle of the dish. The light *A* is fixed while the light *B* is movable on an optical bench. The experiment is made in a dark room and the lights *A* and *B* are enclosed in a box. At the begin-

ning of the experiments the pipette is filled with a dense suspension of larvae in sea water and then put with its point touching the bottom of the dish. The animals flow out in a fine stream which is narrow at the opening of the pipette and widens slightly, owing probably to the negative stereotropism of the animals. A glass plate (fig. 2) *h i k l*, which has a strong red line *n o* and a fine parallel line *p g* (cut with a diamond), is then put on the dish and so adjusted that *p g* is in the middle of the stream *f g* of the animals. Then the angle α which *p g* makes with the perpendicular from *A* on *a b* is measured. This per-

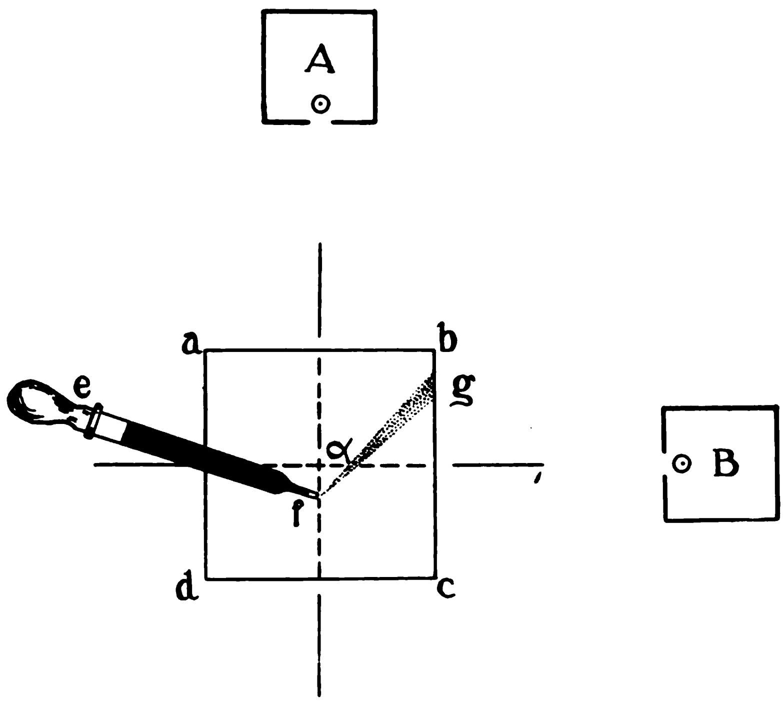

FIG. 1.

pendicular is marked in the form of a red line on the black base on which the glass vessel *a b c d* stands. The angle α is measured with a goniometer. When the lights are equal in intensity α should be 45°; if the two lights have different intensities and if *A* be the stronger light α should become smaller with increasing difference in intensity. The individual measurements vary comparatively little, as long as the difference in the intensity of the two lights is not too great; for this reason our observations do not go beyond a wider ratio of the two lights than 10:1. Table 1 gives the results. *A* is always the stronger light.

Each table is the average of from 40 to 60 individual observations, each being the average of the path of many thousands of animals.

In the next series of experiments an opaque rotating disk with one sector cut out was placed before light B. In one set of experiments the sector cut out was 90°. The rate of rotation (by an electric motor) was 1,500 to 2,500 revolutions per minute. The other light was constant and its position was chosen on the assumption of the validity of the Bunsen-Roscoe law for these cases. Thus when the two lights without sector were equal at a given distance of A, by putting the 90° sector before B, it was assumed that the ratio of effects would be the same as if, with constant light, B had been placed at the double distance and the ratio of intensities of the two lights had been 4:1. Going on such a calculation we should expect the same values for α as in table 1.

As one sees the observed values are slightly smaller but practically identical with the values obtained when the two lights are constant. The deviation is probably due to the fact that the photochemical efficiency of an intermittent light is a trifle less than that calculated on the basis of the Bunsen-Roscoe law.[9]

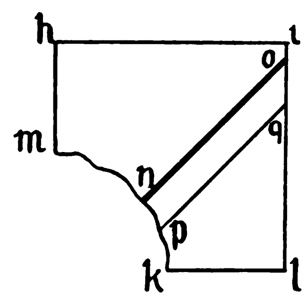

FIG. 2.

We carried out some experiments with a sector of 144°. When the efficiency of both lights was equal on the assumption of the validity of the Bunsen-Roscoe law α was found to be 44.9° (instead of 45°), and for the ratio 2:1 α was found to be 38.8°. The values are, within the limits of error, identical with the values in tables 1 and 2.

TABLE 1

	VALUE OF α FOR DIFFERENT RATIOS OF INTENSITIES OF THE TWO LIGHTS			
Ratio of the two lights	1:1	2:1	4:1	10:1
Value of α (direction of path)	45.6°	40°	34.4°	28.8°

TABLE 2

	VALUE OF α WHEN ONE LIGHT IS INTERMITTENT (90° SECTOR) AND THE OTHER CONSTANT AND THE EFFICIENCY OF THE TWO LIGHTS IS CALCULATED ON THE BASIS OF THE VALIDITY OF THE BUNSEN-ROSCOE PHOTOCHEMICAL LAW		
Ration of the two lights	1:1	2:1	4:1
Value of α	44.2°	38.3°	34.1°

Summary.—The paper gives some new quantitative experiments proving that the 'instinctive' motions of animals to light are phenomena of automatic orientation (heliotropism) and a function of the constant intensity of light; the exact expression of the function being the Bunsen-Roscoe law of photochemical action.

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[9] Parker, G. H., and Patten, B. M., *Amer. J. Physiol.*, **31**, 1912–13, (22); Abney, W. de W., *Report 59th Meeting, British Assoc. Adv. Sc.*, 1899, (481); *Treatise on photography*, 10th ed. London, 1907; Englisch, E., *Arch. wiss. Phot., Halle*, **1**, 1899, (117).

THE APPEARANCE OF REVERSE MUTATIONS IN THE BAR-EYED RACE OF *DROSOPHILA* UNDER EXPERIMENTAL CONTROL

By H. G. May

DEPARTMENT OF ZOOLOGY, UNIVERSITY OF ILLINOIS

Communicated by T. H. Morgan, July 10, 1917

During some experiments in selection for higher and lower facet numbers in the bar-eyed race of *Drosophila ampelophila* I obtained six full-eyed males and five heterozygous females from the stock bottles and the selected lines.

In appearance these flies could not be distinguished from normal full-eyed males and heterozygous females. Three males and three females were mated with bar-eyed flies and gave the offspring anticipated from such normal flies. In two cases the males gave only bar-eyed male offspring and heterozygous female offspring. In the third case the offspring were not examined until twenty-four days after the mating had been made and as the result of the hatching of individuals from the second generation bar-eyed females and full-eyed males were also present. The offspring of the heterozygous females in each case consisted of bar-eyed and full-eyed males and bar-eyed and heterozygous females. Some of the offspring of a full-eyed male were interbred and produced full-eyed males and bar-eyed females as well as bar-eyed males

and heterozygous females. In appearance and behavior, therefore these flies could not be distinguished from normal full-eyed males and heterozygous females.

The possibility that the flies were due to contamination is not absolutely excluded, but the probability is very low. In handling food and flies the usual precautions were used. No larvae or flies appeared in the food jars. Both vestigial-winged and long-winged races were handled, but no contamination of one with the other appeared. Three full-eyed males and one heterozygous female appeared in the vestigial race and all had vestigial wings; the others appeared in the long-winged race, and all had long wings. The fact that all females were heterozygous is a very strong argument against the probability of contamination. In case of contamination the females should, at least in the majority of cases, have been full-eyed; but no full-eyed females appeared. In the face of this evidence it is almost necessary to conclude that these flies appeared by reverse mutation and not by contamination.

The appearance of such reverse mutations can not readily be explained on the basis of the presence and absence theory nor on the theory of association and disjunction, but it is not difficult to explain on the theory of chemical change. If a chemical change in the constitution of some substance, probably in the chromosomes, produced the bar-eyed mutant, then a reversion of that chemical change would produce the original substance and so bring about the reappearance of the original character, the full eye.

The data upon which this report is based together with a more detailed discussion will be published in the near future in the report on the selection experiments.

THE PART PLAYED BY ALCYONARIA IN THE FORMATION OF SOME PACIFIC CORAL REEFS

By Lewis R. Cary

DEPARTMENT OF BIOLOGY, PRINCETON UNIVERSITY

Communicated by A. G. Mayer, June 22, 1917

Following up my studies on the coral reefs of the Tortugas Islands, in which it was found that in this particular region the alcyonaria contribute more limestone to the reefs in a given time than do the stony corals, a similar study of the coral reefs was undertaken, under the auspices of the Carnegie Institution of Washington, at the Island of Tutuila, American Samoa.

All of the coral reefs about Tutuila are fringing reefs, with the exception of the large reef at Nueli (Blacks Bay on the charts), which appears to be changing from a fringing into a barrier reef. Along most of the north shore the reefs are restricted to the bays, or harbors, while on the south side of the island the reefs are continuous from bay to bay around many of the headlands.

In Pago Pago harbor, which is bordered with coral reefs broken only where one of the numerous streams has its entrance, there exists all of the possible conditions of reef environment varying from those at the entrance of the harbor where the surf beats incessantly to those at its inner end where, even in severe storms, the water is only slightly agitated. Marked gradations in the salinity of the water and in the amount of sediment which it bears are also found at different points within this area so that the relationship of reef building organisms to these factors can be readily observed.

Six species only of alcyonaria were found on the reefs at Tutuila. Of this number two occur so rarely that they are unimportant factors as reef builders. All six species belong to the *Alcyonaceae.*

Two of the more abundant species contribute to reef formation only through the setting free of their spicules at the death of the colony. The others, *Alcyoneum rigidum* and *A. confertum*, form at the base of the colony a corallium-like mass of limestone composed of closely cemented spicules, upon which the living tissues are bourn. This rock is continuously added to as the colony grows outward and upward, the living tissues disintegrating about its base as expansion takes place. In this way heads composed of spicule rock are formed which extend to a height of 4 feet (up to low tide mark) above the general reef flat, and which have a circumference as great as 20 feet.

The distribution of the alcyonaria on the Pago Pago reefs depends on a number of factors. Very few specimens are found on the horizontal surfaces of the exposed reefs. On the nearly vertical faces of these reefs, however, *Alcyonium rigidum* often covers large areas forming a nearly continuous carpet. This species occurs on the faces and in the deeper holes of practically all of the reefs about Tutuila, but on the horizontal surfaces of only those reefs which while protected from heavy breakers, are traversed by strong currents which provide for good aeràtion, normal temperature of the water and against the silting up of the colonies.

A. confertum, which is next in importance as a reef builder, is the most resistant of the four species and is consequently the most widely distributed.

A. glaucum requires much the same environmental conditions as *A.*

rigidum but is commonly found in slightly less favorable conditions than the latter by which it is, on account of its habits of growth, easily crowded from the most favorable locations.

The fourth species *A. flexilae* is by far the most delicately branched and is also incapable of retracting its branches. It is consequently restricted to very quiet waters in the most protected locations on the reefs.

To determine the proportion of alcyonaria in different locations on the Pago Pago reefs clearly marked lines were run across a number of the reefs fiom the shore to their outer edges. Along these lines squares 25 feet on a side were laid off and the area occupied by alcyonaria esti-

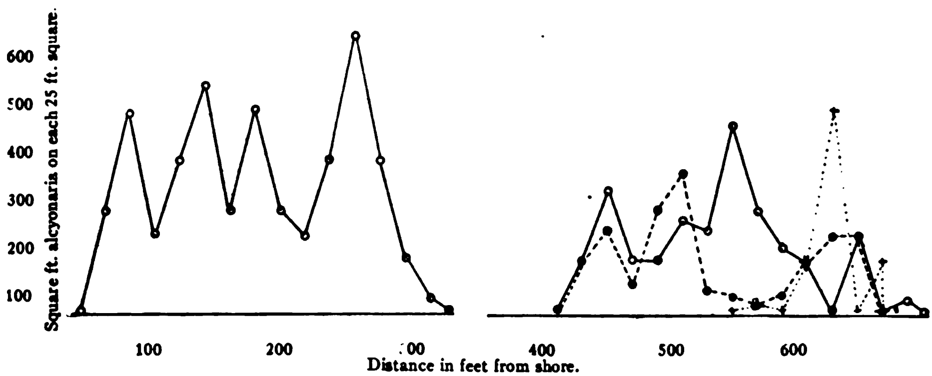

FIG. 1 FIG. 2

FIG. 1. Showing the distribution of the alcyonaria across the Utelei reef Pago Pago harbor, Samoa. The figures along the ordinate represent the distance in feet from shore, those along the abscissa the area in square feet covered by alcyonaria on each square the sides of which were 25 feet in length.

FIG. 2. Showing the distribution of three species of alcyonaria along a line the total alcyonaria of which is shown in figure 1.

Distribution as shown in the previous figure. Alcyonium glaucum, o————; Alcyonium confertum, ● - - - - - -; Alcyonium rigidum, + ...

mated for each square. Figure 1 shows the conditions on a reef along the south side of a cove at Utellei village which is situated on the west side of Pago Pago harbor about three-quarters of a mile from its entrance.

On account of the configuration of the shore line at this place a strong current sets across the reef area traversed by the outer half of this line, while over the inner portion the currents are very weak. The depth at extreme low tide varies from 6 to 15 inches over all of this reef except the lithothamnuium ridge which is exposed for a width of from 50 to 75 feet.

Figure 2 shows the area occupied by each one of the three species occurring along this line. The thickness of the rock formed from spicules

over these areas varies from a mere veneer to at least 2 feet. Along the outer vertical face of the reef on the opposite (north) side of this same cove many barren areas were found to be covered with a surface layer of spicule rock from 1 to 12 inches in thickness. This layer extends back into many subterranean caverns in the reef for a distance of several feet, and when added to the area of the reef face now covered with living alcyonaria constitutes an almost complete covering of spicule rock over the entire reef face for more than one-third of a mile from the head of the cove.

While these observations have made it clear that on certain of the pacific reefs the alcyonaria are important coral forming agents their relative importance can be determined only after borings have been made through some reefs to determine whether or not the present conditions are transient or have been maintained over long periods during the up-building of the reefs.

OBSERVATIONS UPON THE ALKALINITY OF THE SURFACE WATER OF THE TROPICAL PACIFIC

By Alfred Goldsborough Mayer

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Communicated, June 22, 1917

On a voyage from San Francisco, California, to Honolulu and thence to Pago Pago, Samoa; and also upon the return over the same route, we made daily observations of the hydrogen-ion concentration of the surface water, using for this purpose a set of thymolsulphonephthalein tubes standardized and prepared by Prof. J. F. McClendon, and kindly presented to us for this purpose.

It was found that in the mid-Pacific, N.N.E. of Samoa, the surface water at or near the equator was cooler, and less alkaline than 5°–10° north or south of this region. This fact will appear upon inspection of the tables at the end of this paper. It seems that the water of the equator at 24°.9 C. is so low in alkalinity as to be comparable in this respect with the water of only 15°C. about 300 miles off the mouth of San Francisco Harbor, California.

The low alkalinity of the water near the equator was usually although not invariably associated with a decided easterly set opposite in direction to the prevailing westerly surface drift of the tropical Pacific.

This suggests that counter currents at the surface in the tropical Pacific may be regions wherein the cold bottom water is rising to the surface; and that this cold water has not yet had time to come into

equilibrium with the carbon dioxide of the atmosphere, and thus still retains some of the relative acidity associated with its former low temperature.

Similarly, we would expect cold currents drifting into warmer regions to retain their relative acidity to a greater degree than is warranted by their augmenting temperature; and this expectation appeared to be justified by the very low alkalinity of 0.141×10^{-7} shown by the water at 10°.5 C., 54 miles W.S.W. of Golden Gate, San Francisco, on May 1, 1917.

No conclusions should be drawn from such meagre observations, but if future studies should demonstrate that low alkalinity is usually associated with easterly set of surface currents over the tropical oceans, the fact may become of importance to navigation owing to the ease and rapidity with which colorimetric tests of the alkalinity of sea water may be made by using a graded series of thymolsulphonephthalein tubes in the manner suggested by McClendon. In response to a request from Professor McClendon, tests were made of the carbon dioxide of the atmosphere at noon each day, but these showed that there is apparently no definite relation between the CO_2 tension of the air and the local alkalinity of the surface water.

The CO_2 tension was very variable and ranged from about 0.00045 to 0.00025 of an atmosphere, the average of 22 determinations being about 0.00035. There is also no definite relation between the salinity and the hydrogen-ion concentration of the ocean water, as will appear from the following tables.

The cold current which moves southward along the coast of California, is of low salinity being about 33.6 or 33.7 on an average whereas the salinity of the water between 1000 miles off the Californian coast and the Hawaiian Islands is about 35.

An elaborate study of the salinity and temperature of the water off the California coast has been made by George F. McEwen, *University of California Publications, Zoology*, **15,** 1916, (255–356, Plates 1–38), and the presence of an upwelling of cold water from the depths is clearly indicated along the California Coast as a result of studies by Michael and McEwen.

TABLE 1

TEMPERATURE AND ALKALINITY OF SEA WATER AT THE SURFACE FROM SAN FRANCISCO, CALIFORNIA, TO HONOLULU; AND THENCE TO PAGO PAGO, SAMOA, FEBRUARY 21 TO MARCH 5, 1917, ON S. S. *Sierra*, CAPTAIN JOHN J. K. KOUGHAN

DATE 1917	TEMPERATURE OF AIR °C.	TEMPERATURE OF SEA WATER, °C.	HYDROGEN-ION CONCENTRATION OF SEA WATER	PH OF SEA WATER	LATITUDE	LONGITUDE	MILES FROM PORT	BAROMETER IN INCHES	REMARKS
San Francisco to Honolulu									
February 21, noon	16°.6	12°.3	0.89×10^{-8}	8.05	36° 05′ N.	128° 38′ W.	302	29.79	Wind from west; rough sea
February 22, noon	15°.4	15°.4	0.676×10^{-8}	8.17	33° 32′ N.	135° 09′ W.	657	29.88	Rough; overcast. Wind from S-S. E.
February 23, noon	16°.5	17°.6	0.63×10^{-8}	8.2	30° 53′ N.	140° 53′ W.	988	29.53	Squally; rain, rough. Wind from W.N.W.
February 24, noon	18°.3	20°.1	0.589×10^{-8}	8.23	27° 49′ N.	146° 28′ W.	1334	30.01	Overcast, N.N.W. wind; moderate sea
February 25, noon	21°.2	21°.6	0.563×10^{-8}	8.25	24° 22′ N.	152° 19′ W.	1712	30.05	S.W. breeze; overcast, moderate sea
February 26, 10 a.m.	23°.4	23°.6	0.563×10^{-8}	8.25	21° 25′ N.	157° 25′ W.	2050		Clear; nearly calm; breeze from S.E., 30 miles off Honolulu
From Honolulu to Pago Pago, Samoa									
February 27, noon	24°.7	24°.2	0.563×10^{-8}	8.25	17° 52′ N.	159° 13′ W.	220	30.04	Clear, light trade wind from N.N.E.
February 28, noon	25°.5	25°.4	0.563×10^{-8}	8.25	12° 10′ N.	161° 17′ W.	588	29.97	Clear. Strong E.N.E. trade wind
March 1, noon	26°.3	26°.1	0.589×10^{-8}	8.23	6° 35′ N.	163° 21′ W.	940	29.84	Clear; rough; wind from E.N.E.
March 1, 5:20 p.m.	26°.2	25°.9	0.63×10^{-8}	8.2	5° 13′ N.	163° 40′ W.	1021		Strong current to east. Wind from N.E.
March 2, noon	24°.7	24°.2	0.676×10^{-8}	8.17	1° 07′ N.	165° 30′ W.	1293	29.78	Current to the N.W. all day; fair E. breeze; smooth.
March 2, 4.45 p.m.	24°.5	24°.85	0.656×10^{-8}	8.18	Equator	165° 50′ W.	1364		Current to the N.W. all day; fair E. breeze; smooth.
March 3, noon	26°.3	26°.4	0.63×10^{-3}	8.2	4° 35′ S.	167° 24′ W.	1653	29.79	Light breeze from east; clear
March 3, 5 p.m.	26°.4	26°.8	0.595×10^{-8}	8.225	5° 49′ S.	167° 45′ W.	1728		Light breeze from east; clear
March 4, noon	27°.4	28°.0	0.563×10^{-8}	8.25	10° 14′ S.	169° 14′ W.	2010	29.81	Clear, light breeze from east
March 4, 5 p.m.	27°.4	27°.9	0.563×10^{-8}	8.25	11° 29′ S.	169° 36′ W.	2085		Clear, light breeze from east
March 5, 6 a.m.	26°.8	27°.3	0.563×10^{-8}	8.25	14° 09′ S.	170° 31′ W.	2262		About 10 miles N. of Tutuila, Samoa

TABLE 2

TEMPERATURE, ALKALINITY, AND SALINITY OF SEA WATER AT THE SURFACE FROM PAGO PAGO, SAMOA, TO HONOLULU AND THENCE TO SAN FRANCISCO, CALIFORNIA, APRIL 19 TO MAY 1, 1917, ON S.S. *Ventura*, CAPTAIN J. H. DAWSON

DATE, 1917	TEMPERATURE OF AIR, °C.	TEMPERATURE OF SEA, °C.	HYDROGEN-ION CONCENTRATION OF SEA WATER	PH OF SEA WATER	SALINITY OF SEA WATER	LATITUDE	LONGITUDE	MILES FROM NEAREST PORT	BAROMETER	DIRECTION OF CURRENT	WEATHER
Pago Pago, Samoa, to Honolulu, Hawaiian Islands											
April 19, noon	25°.4	27°.8	0.575×10^{-8}	8.24	35.10	10° 15′ S.	169° 03′ W.	262	29.87		Light breeze from N.E.; showers. Raining when sample was taken
April 20, noon	26°.95	26°.75	0.708×10^{-8}	8.15	35.41	5° 10′ S.	167° 00′ W.	591	29.84	To the east	N.E. breeze; clear weather
April 21, noon	25°.4	24°.95	0.759×10^{-8}	8.12	35.26	Equator	165° 05′ W.	919	29.80	Strong current to the east	E.N.E. breeze; clear weather
April 22, noon	26°.4	25°.9	0.795×10^{-8}	8.1	35.05	5° 42′ N.	163° 12′ W.	1281	29.82	No current	Strong breeze from N.E.; moderate sea
April 23, noon	25°.4	25°.35	0.603×10^{-8}	8.22	34.58	11° 19′ N.	161° 05′ W.	1641	29.90	No current	Moderate breeze from N.E.; clear
April 23, 5.00 p.m.	25°.3	25°.5	0.588×10^{-8}	8.23		12° 29′ N.	160° 39′ W.	1723		No current	Moderate breeze from N.E.; clear
April 24, 7.30 a.m.	24°.7	24°.55	0.588×10^{-8}	8.23+		15° 58′ N.	159° 46′ W.	1932		Set to the west	Breeze from N.E.; clear; moderate sea
April 24, noon	24°.2	24°.5	0.617×10^{-8}	8.21	34.58	17° 03′ N′.	159° 23′ W.	1999	29.96	To the east	Clear E.N.E. breeze; moderate current
April 24, 5.30 p.m.	23°.5	24°.05	0.582×10^{-8}	8.235		18° 33′ N.	159° 00′ W.	2081		Set to the west	Clear; breeze from the east
April 25, 5.00 a.m.	21°.1	23°.75	0.617×10^{-8}	8.21				15 miles S. of Honolulu		Set strongly to the east	Calm, clear all day

TABLE 2—*Continued*

DATE, 1917	TEMPERATURE OF AIR, ° C.	TEMPERATURE OF SEA, ° C.	HYDROGEN-ION CONCENTRATION OF SEA WATER	PH OF SEA WATER	SALINITY OF SEA WATER	LATITUDE	LONGITUDE	MILES FROM NEAREST PORT	BAROMETER	DIRECTION OF CURRENT	WEATHER
Honolulu to San Francisco, California											
April 25, 6 30 p.m.	22°.4	23°.9	0.63×10^{-8}	8.2	34.94	5 miles off the S.E. Point of Oahu			29.93	Easterly set; current moving N.E.?	Calm, clear all day
April 26, noon	22°.6	25°.6	0.582×10^{-8}	8.21	34.79	23° 51′ N.	153° 44′ W.	281	29.94	Easterly set; current moving N.E.?	Breeze from the east; clear smooth sea
April 27, noon	21°.9	21°.6	0.588×10^{-8}	8.23+	35.23	27° 18′ N.	147° 52′ W.	662	29.95	Current southerly	Breeze from the east; overcast; moderate sea
April 28, noon	19°.4	19°.8	0.63×10^{-8}	8.2	35.05	30° 29′ N.	141° 50′ W.	1033	30.02	No current	Breeze from the east; clear; moderate sea
April 29, noon	16°.6	16°.6	0.795×10^{-8}	8.1	33.89	33° 39′ N.	135° 34′ W.	1405	30.13	Northerly current	Clear, nearly calm
April 30, noon	16°.1	14°.3	0.795×10^{-8}	8.1	33.17	36° 00′ N.	128° 59′ W.	1760	30.15	No current	Overcast. No rain. Nearly calm. Water blue as in mid-Pacific
May 1, 7.00 a.m.	12°.7	10°.5	0.141×10^{-7}	7.85	33.33	54 miles off Golden Gate, San Francisco Harbor		2036		Southerly current	Calm; clear; slight breeze from the west. Water dark greenish-brown to olive.

THE EFFECT OF TEMPERATURE ON LINKAGE IN THE SECOND CHROMOSOME OF DROSOPHILA

By Harold H. Plough

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Communicated by T. H. Morgan, July 5, 1917

Some recent data have shown that certain influences affect the percentage of crossing over. Bridges[1] (1915) noted that the age of the mother altered the result in the second chromosome, and Sturtevant[2] (1917) has recently discovered no less than three definite Mendelian factors which influence the strength of linkage in parts of the second and of the third chromosomes. For two years I have been studying the effect of changes in the environment on the percentage of crossing over in *Drosophila melanogaster* (*ampelophila*). It has been found that very striking effects are produced by differences in temperature for the second chromosome. A full report of this work will appear in a forthcoming number of the *Journal of Experimental Zoölogy.*

Tests of the effect of temperature on the percentage of crossing over were made in the following way. Virgin females from the stock collected originally at Falmouth, Mass., were mated to males homozygous for the second chromosome factors for black body color, purple eyes and curved wings. The pairs were placed successively in two or more sets of bottles for three or four days each. The bottles of the first set were kept at room temperature—about 22°C—and each of the other sets at one of the temperatures to be tested. The normal females heterozygous for black purple curved which hatched from each set were then back crossed to males of the original mutant stock and the offspring allowed to hatch at room temperature. The offspring (F_2) of the back cross show the genetic constitution of the (F_1) eggs, and enable one to calculate the percentage of crossing over between black and purple, and purple and curved. The percentages for the shorter region—black to purple—from a number of experiments involving more than 35,000 flies are shown in the curve given in figure 1. The value at 22°C is the average of the controls for all of the different sets. At lower and higher temperatures—5°C. and 35°C.—no fertile offspring hatched.

The curve is of considerable interest since it shows that both high and low temperatures produce an *increase* in the percentage of crossing over, i.e., a reduction in the strength of linkage. It is plain that the process does not follow van't Hoff's law, as do most physiological processes.[3] The phenomenon therefore involves apparently some change in the physical state of the colloidal substratum of the cells.

Further investigation has demonstrated that the increase in the percentage of crossing over due to high or low temperature applied during the development of the female parent is maintained for only six or seven days after she begins to lay. At the end of this period the percentage drops to the same level as the control. It has also been found that the high or low temperature can be applied to adult females with similar results. After the flies are exposed to the new temperature, an interval elapses during which 225 to 275 eggs are laid which do not show the effect of the new temperature. The percentage then jumps suddenly to the high point where it remains for approximately the length of time of the exposure. A curve illustrating this point is shown in figure 2. The mothers of both series were sisters hatched at room temperature.

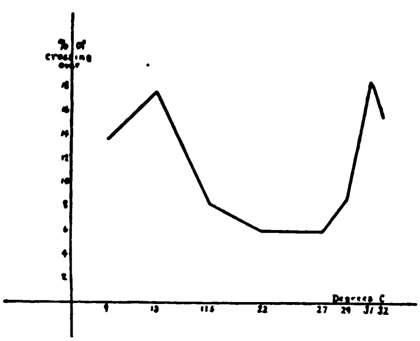

FIG. 1

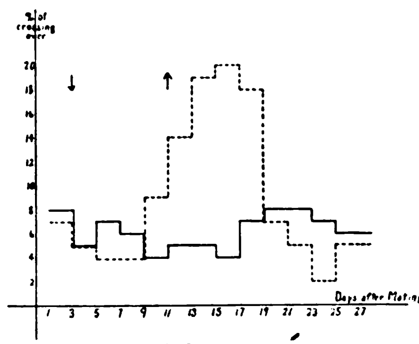

FIG. 2

They were mated and placed in phials which were changed regularly at two day intervals. From the third to the eleventh day the pairs of one series were exposed to a temperature of 31.5°C while the others were continued at room temperature. The percentage of crossing over for the black to purple region is shown in the curve. After an interval of eight days following the beginning of the treatment (i.e., beginning on the same day that the flies were returned to normal temperature) the treated series showed an increase of more than 100% in the amount of crossing over among their offspring. This high ratio was maintained for eight days, after which it returned at once to the control value.

The facts apparently indicate that temperature affects the amount of crossing over at a definite stage in the oogenesis. Exposures to high temperatures for one, two or four day periods make it appear that a consecutive exposure of nearly two days is required to produce any effect at all. Cytological examination has shown that shortly after hatching about 140 eggs have usually passed the last oogonial division. Controls show that eggs are laid at the rate of about 50 a day or 100

in two days. This number of eggs therefore has not received the exposure necessary to produce the change in crossing over. The culmination of the two days exposure is to be expected in those eggs so situated that 125 to 175 eggs will be laid before them. Such an interpretation makes it extremely likely that the change in the amount of crossing over is finally affected in the earliest oocytes, that is, at the beginning of the growth period. The above evidence on the time of applying the new temperature and the time when the change in crossing over occurs, suggests that the crossing over process takes place in the stage when the chromosomes of Drosophila are known to be finely drawn out threads.

The decrease in the strength of linkage caused by temperature in no way weakens the chromosome interpretation of linkage. It rather adds to it considerable support, for it localizes the process of crossing over at a period in oogenesis when twisting between homologous threads seems possible. The evidence positively establishes the fact that crossing over does not take place during the early oogonial divisions, and makes it extremely improbable that it occurs at so late a stage in the growth period as the thick thread stage favored by Janssens as the chiasmatype.

[1] Bridges, C. B., *J. Exp. Zoöl., Wistar Inst., Philadelphia*, 19, No. 1, July, 1915.
[2] Sturtevant, A. H., these PROCEEDINGS, 3, 1917. (555–558).
[3] Cf. Snyder, C. D., *Amer. J. Physiol.*, 22, 1908, (309).

GENETIC FACTORS AFFECTING THE STRENGTH OF LINKAGE IN DROSOPHILA

By A. H. Sturtevant

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Communicated by T. H. Morgan, July 5, 1917

In September, 1913, a wild female Drosophila of a stock from Liverpool, Nova Scotia, was crossed to a male bearing the second chromosome mutant characters vestigial and speck. A single daughter of this mating was tested, and gave no crossovers among 99 offspring, though vestigial and speck usually show about 37% crossing over. This strain has since been bred in very large numbers, and the experiments are being continued; but it has seemed advisable to report briefly on some of the results obtained.[1]

It has become clear that the original result was due to something in the second chromosome derived from the Nova Scotia female. Two of her granddaughters and all of her later descendants that were known, from linkage, to have received the second chromosome in question gave

similar results; but those descendants known not to have received it gave the 'usual' result. This was true whether the chromosome came from the mother (as above) or from the father. And when other second chromosome genes were substituted for vestigial and speck, unusual ratios still appeared; but for any combination the result was relatively constant, and the combined 'unusual' results give as consistent a scheme as the 'usual' results. The first column of table 1 shows the percentage of crossing over in the ordinary females for various combinations of second chromosome genes.[3] The second column gives similar data for tests of females that carry a Nova Scotia chromosome. Figures 1 and 2 are chromosome maps constructed on the basis of these two columns.

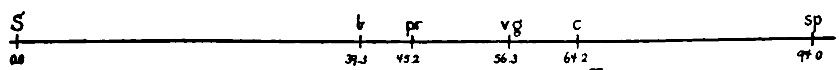

Fig. 1

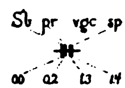

Fig. 2

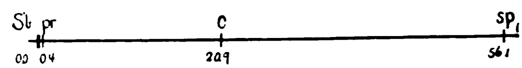

Fig. 3

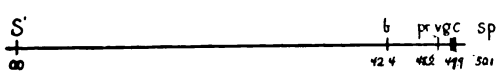

Fig. 4

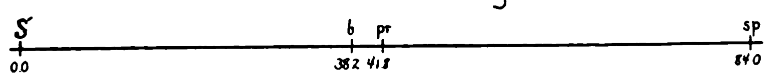

Fig. 5

Fig. 6

A large number of these experiments have been made with females having one Nova Scotia chromosome, and its mate bearing the mutant genes black, purple, and curved. Numerous tests have been made in order to determine the nature of the crossovers produced. It has been found that those crossovers that receive the part of the original Nova Scotia chromosome lying to the left of the purple locus still give unusual results and transmit the peculiarity to all of their descendants that receive this piece. The results produced by such females are not, however, the same as those produced when all the Nova Scotia chromosome

is present, as is shown in column 3 of table 1 and in figure 3. When the other piece of the Nova Scotia chromosome, lying to the right of purple (and, in many experiments, that part of it that is also to the left of speck) is tested, a still different result is obtained, as shown in column 4 and figure 4.

TABLE 1

GENES	PERCENTAGE OF CROSSING OVER					
	Normal	$\frac{C_{IIl}\ C_{IIr}}{}$	$\frac{C_{IIl}}{}$	$\frac{C_{IIr}}{}$	$\frac{C_{IIr}}{C_{IIr}}$	$\frac{C_{IIl}\ C_{IIr}}{C_{IIr}}$
Star black	39.3	0.0	0.0	42.4	38.2	0.3*
Star purple	40.2	0.0	×	45.4	40.6	0.3*
Star curved	46.5	×	25.9	47.3	×	×
Star speck	48.3	0.4	49.0	46.0	49.0	47.4
Black purple	5.9	0.2	0.4	5.8	3.6	0.1
Black vestigial	16.9	1.2	×	8.9	×	×
Black curved	22.7	1.3	25.6	7.5	×	×
Black speck	49.3	1.1	48.8	9.3	43.5	47.0
Purple vestigial	11.1	1.2	×	0.6	×	×
Purple curved	19.0	1.1	20.5	1.7	×	×
Purple speck	46.5	1.2	47.4	2.8	42.2	47.4
Vestigial speck	36.7	0.2	×	0.0	×	×
Curved speck	29.8	0.1	35.2	0.2	×	×
†Total length	94.0	1.4	56.1	50.1	84.0	47.8

* These few (6) crossovers are doubtful. None of them were tested; and there is apt to be a small percentage of error in classifying Star flies.

† Calculated as Star black + black purple + purple curved + purple speck in all but the last two columns, where purple speck is used. In no case further corrected for double crossing over.

It follows from these results that the original Nova Scotia chromosome contained at least two factors causing reduced crossing over, and each affecting chiefly the region in which it lies. We may call these two factors 'C_{IIl}' and 'C_{IIr}' (C for crossing over, after Muller,[2] II to distinguish them from similar factors affecting other chromosomes, *l* and *r* for left hand and right hand).

By appropriate matings it has been possible to obtain females homozygous for C_{IIr}, and a surprising result has been observed: the effect produced by C_{IIr} when heterozygous disappears when it becomes homozygous (see columns 5 and 6, figures 5 and 6). There can be no doubt of this result, as it has been obtained repeatedly, and has often been checked by tests of the offspring. It has also been paralleled more recently by results obtained by Muller[3] (1916) and by me[4] with C_{III}. Similar tests of homozygous C_{IIl} have not been possible since no chromo-

some has yet been obtained that contains both that factor and any mutant factor within its "sphere of influence."

Backcross tests have shown that males of all the above types with respect to $C_{II}l$ and C_{III} resemble "normal" males in giving no crossovers at all.

Included in the above tables are a considerable number of data involving three or more loci at once; and these agree with the table in showing clearly that the linear order of the factors established for the usual second chromosome is unchanged by the factors under discussion. The amount of crossing over is altered, often markedly, and not usually proportionately in different regions; but the factors keep their same sequence. This result serves to emphasize the importance of considering the distances on chromosome maps as only diagrammatic, not as representing actually proportionate distances between the genes, although actual distance is evidently an important factor influencing the end result. It does not, I think, in any way weaken the case for the chromosome hypothesis, but merely shows, together with the results of Bridges[5] (1915) and Plough[6] (1917), that any chromosome map is available for purposes of numerical prediction only when the conditions under which it was made are duplicated.

[1] Some of the early results were reported at the 1913 meeting of the American Naturalists, and brief references have been published by me (1915) and by Muller (1916).

[2] Much of this data has not hitherto been published. It has been collected mainly by Dr. C. B. Bridges, to whom I am indebted for permission to use it.

[3] Muller, H. J., *Amer. Nat., Lancaster, Pa.*, **50**, 1916, (193, 284, 350, 421).

[4] Sturtevant, A. H., *Zs. Abst. Vererb.*, **13**, 1915, (234).

[5] Bridges, C. B., *J. Exp. Zool., Wistar Inst. Philadelphia*, **19**, 1915, (1).

[6] Plough, H. H., these PROCEEDINGS, **3**, 1917, (553–555).

FURTHER EVIDENCE ON THE CONCENTRATION OF THE STARS TOWARD THE GALAXY

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Communicated by G. E. Hale, July 9, 1917

In a previous communication[1] attention was directed to the striking difference in the results for the distribution of the stars with respect to the galactic plane found by Kapteyn[2] and by Chapman and Melotte.[3] An analysis of the counts of stars on photographs of 88 Selected Areas, made at Mount Wilson with the 60-inch reflector, gave preliminary values for the variation in the totals to magnitude 17.5 which agree well with those of Kapteyn.

Further confirmatory evidence of the large value of the galactic condensation for the faint stars, which is so striking a feature of Kapteyn's conclusions, is available from two different sources. The first is a series of counts made at Groningen on photographs of 54 Selected Areas taken with the Bruce telescope of the Harvard station at Arequipa in Peru. The results for these including 127,315 stars, are given by Kapteyn in his *First and Second Reports on the Progress of the Plan of Selected Areas.*[4] The second source of data for a comparison is the collection of counts recently published by Turner for nearly 600,000 stars in zones of the *Astrographic Catalogue.*[5]

The durchmusterung photographs of the southern Selected Areas extend from +62°. to −76° galactic latitude, and are well distributed throughout this interval. The exposure time was two hours. We assume that the different plates are comparable, in limiting magnitude, and a reference to Professor Kapteyn's tabulation[6] indicates that the agreement in this particular is satisfactory. The calculated limits of brightness given by him show considerable irregularity, but it should be noted that these have been derived from the observed density in each field, and are thus affected by local variations in the distribution of the stars, as well as by changes in atmospheric transparency and in the photographic conditions of exposure and development. For example, area No. 110, galactic latitude +1°, lies between the two branches of the Milky Way, and is known to include an abnormally small number of stars. This accounts for the unusually bright limit of 13.2 given by Kapteyn.

The photographs of areas in southern latitudes are richer in stars than those north of the Galaxy; but since the evidence from other sources as to the reality of this difference is conflicting, no distinction has been made between northern and southern latitudes in forming the mean curve connecting latitude with stellar density (number of stars per square degree to the limiting magnitude m, denoted by N_m).

After the counts had been arranged in the order of increasing latitude, overlapping means for groups of ten areas were formed, with the results shown in the first two columns of table 1. These define a smooth curve whose ordinates and corresponding abscissae for equidistant intervals are in the third and fourth columns of the table. The value in parentheses is an extrapolation. The fifth column contains the densities from Kapteyn's table in *Groningen Publication*, No. 18, p. 54, for magnitude 16.0, which is adopted as the mean limit for the whole series of plates. In the column following are the densities from the Mount Wilson photographs of 88 northern Selected Areas. These are the values given in

these PROCEEDINGS, **3**, 1917, 219, after the application of a correction of -0.10 to the logarithms to reduce them to the limiting magnitude of the durchmusterung plates.

The significant feature of the table is the relation between the three series of results shown by the differences in the last two columns. These reveal no important systematic effect and show that the increase in stellar density with decreasing latitude is substantially the same for all three investigations. The differences MW–DM are noteworthy in that all of the Mount Wilson photographs are of regions on or north of the celestial equator, while the durchmusterung areas are on or south of the equator, with only seven regions in common.

The average value for the ratio of the number of stars at 5° latitude to the number at 80° is 23, with deviations of -2, $+4$, and -2 for DM, Kapteyn, and MW, respectively. This result refers to magnitude 16.0 on the scale of *Groningen Publication* No. 18. On the international photographic scale the limit would be approximately 17.0.

The counts for the Astrographic zones, which constitute a very important collection of data, have been discussed with detail in an article that will appear in the *Astrophysical Journal*. The salient features only need be touched upon here. The zones are ten in number, each 2° wide, and well distributed in declination from $+62°$ to $-65°$. The counts extend throughout the entire 24 hours of right ascension, so that each zone includes a wide range of galactic latitude. We have here, for each hour of right ascension, not only the total number of stars to the faintest limit of the *Catalogue*, but also the totals to at least one, and usually two, other limits from one to three magnitudes brighter. We assume that each limit is constant for any given zone; although there are obvious irregularities, their influence is pretty thoroughly eliminated by the method of discussion, and it is improbable that the mean distribution is affected to any important degree.

A curve connecting latitude and density was drawn, just as in the case of the durchmusterung counts above, for each separate limit of each zone. From these curves were read the stellar densities for each 10° latitude. With these and their respective latitudes as arguments, corresponding magnitudes were interpolated from Kapteyn's distribution table in *Groningen Publication* No. 18. The mean of the magnitudes thus found for any curve was adopted as the limiting magnitude for that curve and for the series of counts to which it corresponds. The process is exactly that described in these PROCEEDINGS, **3**, 1917, 217, and there used for the determination of the limiting magnitude, on the scale of Kapteyn's table, of the Mount Wilson photographs of the Selected Areas.

The next step involved the segregation of the data for equidistant values of galactic latitude. The 0° ordinate for each of the original curves was plotted against the limiting magnitude just found for the curve to which it belongs. These points define a new curve expressing the variation of stellar density with magnitude for regions in 0° latitude. Similar curves were also derived for 10°, 20°, etc., by plotting the appropriate ordinates of the original curves against their limiting magnitudes

TABLE 1

Comparison of Durchmusterung Results with Kapteyn and Mount Wilson

Mean Latitude	Observed Log N_m	Galactic Latitude	Log N_m			Kapteyn minus DM	MW minus DM
			DM	Kapteyn	MW		
3°	3.99	5°	3.97	4.04	4.03	+0.07	+0.05
10	3.90	15	3.79	3.74	3.76	−0.05	−0.03
16	3.75	25	3.50	3.47	3.43	−0.03	−0.07
23	3.63	35	3.22	3.23	3.16	+0.01	−0.06
30	3.32	45	3.00	3.04	3.00	+0.04	0.00
37	3.17	55	2.84	2.89	2.88	+0.05	+0.04
47	2.98	65	2.74	2.74	2.79	0.00	+0.05
60	2.79	80	(2.65)	2.60	2.69	(−0.05)	(+0.04)

TABLE 2

Differences in Log N_m from Astrographic Zones and from Kapteyn's Table of Distribution

(Unit = 0.01)

Magnitude	Galactic Latitude								
	0°	10°	20°	30°	40°	50°	60°	70°	80°
8.5	−3	0	0	+2	+2	+1	+1	+1	0
9.0	−3	0	0	+1	0	−2	−2	−1	−2
9.5	−1	+1	+3	+2	0	−3	−3	−3	−1
10.0	0	+1	+3	+3	0	−2	−3	−2	−1
10.5	−1	+1	+4	+3	0	−2	−4	−2	+1
11.0	−3	0	+4	+3	−1	−2	−3	−2	−1
11.5	−3	−1	+4	+4	+1	−1	−1	0	−1
12.0	−4	−1	+3	+4	+1	0	0	+1	−2
12.5	−5	−1	+4	+5	+3	+2	+2	+1	−1

as before. From the new curves were read the densities corresponding to each half-magnitude interval from 8.5 to 12.5. The results, when tabulated, constitute a distribution table analogous to that of Kapteyn, though restricted in its range of brightness.

Our interest is naturally in the agreement of these two tables. Their differences (Astrographic zones *minus* Kapteyn), in units of the second decimal, are shown in table 2. Though more or less systematic, the

deviations are very small—in most cases within the uncertainty which necessarily affects the discussion of the data. To this extent, therefore, we may look upon them as supplying a very satisfactory confirmation of Kapteyn's results. This fact is further illustrated by the following series of values for the galactic condensation (ratio of number of stars at 5° to number at 80°).

Limiting Magnitude	8.5	9.5	10.5	11.5	12.5	16.0
Kapteyn	2.6	3.1	3.8	4.9	6.7	27
Astrographic Zones	2.4	3.2	3.8	4.7	6.2	....
Arequipa, Selected Areas		...	....	....	....	21
Mt. Wilson, Selected Areas		...	....	....	....	21

It is important to note that the use of Kapteyn's density table for the determination of the limiting magnitudes of the counts has not artificially forced an agreement with his values of the galactic concentration. The use of a limit thus derived for any zone in no wise affects the rate at which the density observed within that zone increases with decreasing latitude. The results are referred to Kapteyn's scale of magnitudes, but this influences only indirectly the value of the condensation for any specified limit.

A comparison of the counts for the different zones with the mean distribution table whose derivation has been outlined reveals large deviations in individual cases. To what extent these represent local variations in stellar density cannot now be determined. Professor Turner has been led to believe[7] that the Astrographic data give evidence of a spiral of obscuration encircling the sky, in which the ratio of the number of faint stars to bright stars is abnormally low. But his discussion seems not to include the influence of the increase in galactic condensation with increasing magnitude, which appears so clearly in the numbers appended to the preceding paragraph. When this has been duly allowed for, the fluctuations in the ratio of faint stars to bright stars assume an accidental character; doubtless they correspond in part to actual variations in density, but differences in observing conditions must also have contributed much in producing the irregularities.

[1] Seares, F. H., these PROCEEDINGS, **3**, 1917, (217–222).

[2] Kapteyn, J. C., *Groningen, Pub. Astr. Lab.*, No. 18, 1908, (1–54).

[3] Chapman, B. A., and Melotte, P. J., *London, Mem. R. Astr. Soc.*, **60**, 1914, (145–173).

[4] Published by the Astronomical Laboratory at Groningen, 1911, (1–34).

[5] Turner, H. H., Numerous articles in *London, Mon. Not. R. Astr. Soc.*, **72, 75, 76, 77,** 1909–1916.

[6] Kapteyn, J. C., *First and Second Reports, Groningen*, 1911, (1–34), p. 15.

[7] Turner, H. H., London, *Mon. Not. R. Astr. Soc.*, **76,** 1915, (149–157), p. 152.

THEORETICAL RELATIONS IN THE INTERFEROMETRY OF SMALL ANGLES[1]

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Communicated July 25, 1917

In addition to the sides of the ray parallelogram (base, b) and the radius of rotation R, we shall have to consider the following angles or angular increment: $\Delta\alpha$ the angular rotation of the paired mirrors, $\Delta\theta$ the corresponding angular displacement of the fringes, ΔN the linear displacement of the micrometer mirror (in a direction normal to its face), and $\Delta\varphi$ the angle subtended by two consecutive fringes. If n is the order of the fringe we may write

$$\Delta\varphi = \Delta\theta/\Delta n \tag{1}$$

Moreover if i is the angle of incidence (45°) of the impinging beams at the mirrors and λ the wave length in question,

$$2 \cos i \, \Delta N/\Delta n \doteq \lambda, \tag{2}$$

or on substitution,

$$\Delta\theta/\Delta N = \Delta\varphi/(\Delta N/\Delta n) = 2 \cos i\text{-}\Delta\varphi/\lambda \tag{3}$$

Again if s is the angle at the apex of the distance triangle on the base b,

$$\Delta s = 2\Delta\alpha \tag{4}$$

and

$$\Delta\alpha = \Delta N \cos i/R \tag{5}$$

and since the distance $d = b/2\Delta s = b/2\Delta\alpha$, from (5)

$$d = bR/2\Delta N \cos i = F/\Delta N \tag{6}$$

so that the sensitiveness is from (6),

$$\delta d = (2d^2 \cos i/bR) \, \delta\,(\Delta N) = \delta\,(\Delta N)/Fd^2 \tag{7}$$

If μ is the index of refraction, e the effective thickness of the plates, i.e., the difference of effective thickness of the two half silvered plates through which the beams pass, we may write as in the colors of thin plates (since the respective beams pass each plate but once)

$$n\lambda = e\mu \cos r - 2N \cos i \tag{8}$$

if r is the angle of refraction corresponding to the incidence, i. If n, i, r, alone vary while e, μ, λ, N, are fixed and since $\sin i = \mu \sin r$, i.e., if the eye travels through the field of the telescope from left to right,

$$\Delta\varphi = di/dn = \lambda/(2N \sin i - e \tan r \cos i) \tag{9}$$

so that $\Delta\varphi$ depends inversely on e and N.

When the spectrum ellipses are centered, $N = N_0$, a condition necessary for the occurrence of achromatic fringes, where,

$$2N_0 = e\ (\mu \cos r + 2B/\lambda^2 \cos r)/\cos i \qquad (10)$$

if $\lambda d\mu/d\lambda = 2B/\lambda^2$ is adequate. Equation (10) may now be inserted in equation (9) and the coefficient of e, viz., the long parenthesis containing circular functions evaluated for $i = 45°$, $\lambda = 6 \times 10^{-5}$, $\mu = 1.55$, $2B/\lambda^2 = .026$. Its value is slightly greater than 1. Hence we may write approximately but with much greater convenience

$$\Delta\varphi = \lambda/e \qquad (11)$$

and we thus obtain the breadth of the fringes for different values of the parameter e, roughly. It would, of course, be easy to compute the accurate value of i. This equation placed in the above equations (3), (5), (6) gives in succession,

$$\Delta\theta = \frac{2\Delta\varphi\, \Delta N \cos i}{\lambda} = 2\frac{\cos i}{e}\Delta N \qquad (12)$$

$$\Delta\theta = \frac{2R}{\lambda}\Delta\varphi\,\Delta\alpha = \frac{2R}{e}\Delta\alpha \qquad (13)$$

$$d = \frac{bR}{2\Delta N \cos i} = \frac{bR}{e\Delta\theta} \qquad (14)$$

so that the measurement of the long distance d depends ultimately on the area, $2bR$, of the ray parallelogram, the differential thickness of paired glass plates, e, and the displacement $\Delta\theta$ of the achromatic fringes.

From equation (14) we obtain the sensitiveness by differentiation, or

$$\delta d = \frac{d^2 e}{bR}\delta\,(\Delta\theta) \qquad (15)$$

Let the angle $\Delta\theta$ or its variation be measured in a telescope of length L, and provided with an ocular micrometer, so that the angle $\Delta\theta = x/L$, x being the linear magnitude measured on this micrometer. Hence,

$$\delta d = \frac{d^2 e}{bRL}\delta x \qquad (16)$$

and if we introduce moderate estimates, d = kilometer = 10^5 cm., $e = 10^{-2}$ cm., $b = 200$ cm., $R = 10$ cm., $L = 50$ cm., $\delta x = 10^{-2}$ cm., then $\delta d = 10$ cm., or d should be measurable to 10 cm. at a kilometer, so far as the interferometer only is concerned. It may be noticed that in equations (11) to (14), e is variable and equal to $\lambda/\Delta\varphi$. If the plate halfsilvers traversed by the interfering beams are not equally thick

optic plate (or in general), a full compensation is secured by rotating the mirror nearest the telescope, provided it is the thinner of the pair. In such a case the system of mirrors will not be parallel, however, when the fringes are infinite in size (circles) and the zero position of the micrometer must be independently found, as one of the constants of the apparatus.

[1] See these PROCEEDINGS, 3, June, 1917, 412, 432, 436.
[2] From a Report to the Carnegie Institution, of Washington, D. C.

INTER-PERIODIC CORRELATION IN THE EGG PRODUCTION OF THE DOMESTIC FOWL

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Communicated by C. B. Davenport, July 13, 1917

In no animal organism except man has the investigation of the interrelationship of the various morphological and physiological characteristics of the individual by means of the modern methods of statistical analysis applied to large masses of quantitatively recorded observations been carried out on a more extensive scale than in the domestic fowl, to which Pearl and Surface, and others associated with them, have devoted their attention for a number of years.

Notwithstanding the many problems dealt with in this series of investigations, our knowledge of the interrelationships of characters of economic importance in this organism is still far from complete. Data from breeds different from those used by Pearl and Surface are particularly needed for purposes of comparison.

In an earlier paper[1] we discussed the correlation between the concentration of yellow pigment in the somatic tissues and egg production in the White Leghorn fowl. In the present investigation we have considered the correlations between the egg production of various periods.

The intensity of the correlation between the number of eggs laid in the several individual months and the total egg production of the year as a whole is shown by the solid dots in figure 1. A curve in excellent agreement was found for the previous year.

Economically these coefficients are of the greatest importance since they make possible the selection of groups of birds of high annual egg production from the trap nest records of individual months. Biologically they are in some degree spurious because of the fact that the cor-

relation is determined in each instance between the yield of an individual month and the yield of the entire year, which is made up of the yields of all the individual months. If the source of the partial spuriousness of these constants be removed by correlating between the records of the individual months and the total production of the remaining eleven months of the year, a better biological measure of the interdependence of egg laying activity in two periods is obtained. These are the results represented by the position of the circles on the ordinates for the individual months in figure 1. There is, as shown by the shaded area, a material reduction from twelve to eleven months by excluding the month used as the first variable in determining the correlation.

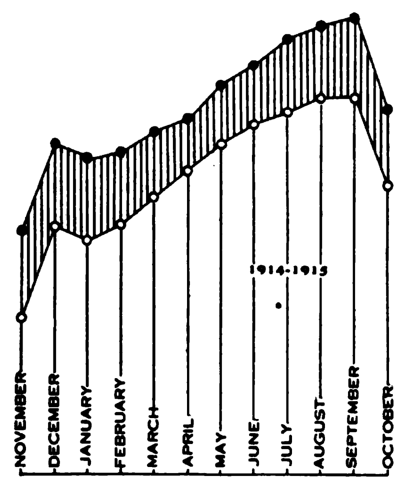

FIG. 1

The correlation between the production of single months and the production of the remaining eleven months of the year has, however, in every instance a substantial value. The coefficients range from 0.295 to 0.567 in the several months of 1913–1914 and from 0.240 to 0.567 in 1914–1915. The average reduction from the corresponding coefficients for 12 month periods is 20.45% in each year. Thus one must conclude that the birds are permanently, at least during the period of the first egg laying year, differentiated in respect of capacity for egg production.

The magnitude of the correlation coefficients measures on the universally applicable scale of -1 to $+1$ the closeness of interdependence of the egg production of the two periods. For purposes of prediction the correlation coefficients may be thrown into the form of linear

regression equations, which have been found to give reasonably good fits to the empirical means for the annual egg records of birds laying various numbers of eggs in the individual months. The slope of the lines when plotted shows there is an increase of from 2.6 to 5 eggs in mean annual production associated with a variation of one egg in monthly record. Since in practical selection groups of birds differing by far more than a single egg may be recognized, the difference in annual production secured by selecting in any month may be of very practical importance, amounting to from 30 to 60 eggs per year.

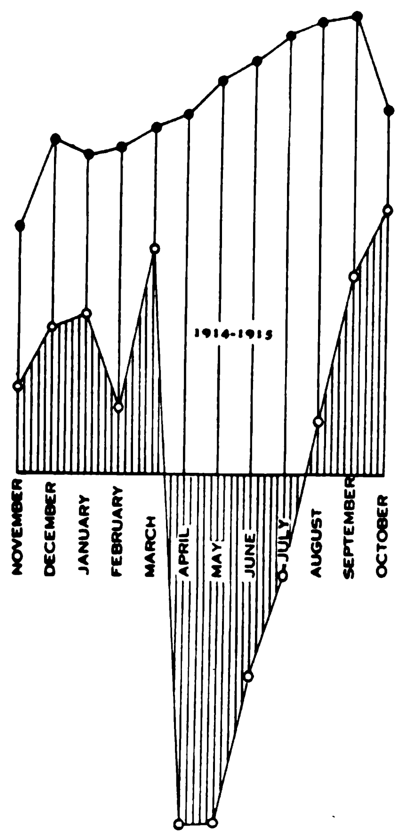

FIG. 2

While the correlation between the monthly egg record and the total annual production of the bird is the measure of the greatest practical value, forming as it does the basis for the prediction of average annual production from the observed performance of a limited period, neither it, nor the biologically more satisfactory correlation between monthly record and the production of the remaining eleven months, gives all

the information which the physiologist desires. A special coefficient due to Pearson and Harris[2] shows the correlation between the annual total and the deviation of the monthly record from the value which it should have if variation in monthly production were directly proportional to variation in the annual production. Figure 2 gives the results for 1914–1915—those for the preceding year being similar.

The coefficients show that in both years the winter months November, December, January and February and the following autumn months, September and October, show an increase over their theoretical quota of eggs when the annual egg production rises above the normal. The spring and summer months, March, April, May, June and July, show a lower relative contribution to the annual total than is theoretically to be expected when this total varies in the direction of an increase above the mean annual egg production of the flock as a whole. Thus in the diagram the coefficients in both years increase from November to December or January, then drop to the first negative value of the year in March and reach their numerically negative largest value in April, after which they rise and become positive in sign but numerically very low in August and attain their maximum positive value in October.

A knowledge of the correlation between the egg records of the individual months is essential to a full understanding of the physiology of egg production in the fowl. We have worked out sets of correlations, 110 coefficients in all, for the production of five of the individual months and the production of each of the other months of the contest year.

The months selected are November of the pullet year and the following October and three intervening months, January, April and August.

The fact that these 110 coefficients are without exception positive in sign seems to us a result of very material biological significance. It indicates that if abnormally high laying at one period tends, as the result of nutritional or other physiological factors, to result in abnormally low production during a subsequent period, the reduction is not sufficient to outweigh the influence of the initial differentiation of the birds in their capacity for egg production suggested above.

Two laws are evident in these inter-mensual correlations. These are to some degree mutually obscurant and must be considered in their mutual relations, but for the sake of simplicity may be stated categorically.

1. The correlation between the egg production of the individual months tends to become smaller as the records upon which the correlations are based become more widely separated in time.

2. There is a more intimate correlation between the egg production

of the autumn and winter months at the beginning and end of the contest year than between the egg production of these months and the productions of the intervening spring and summer months.

The relationship between the intensity of correlation and the degree of separation of the periods of egg production compared is best illustrated by the results for April and the five preceding and the six following months. The magnitude of the constants decreases with fair regularity from March to the preceding November and from May to the following October.

The second law is best exemplified by the coefficients measuring the relationship between November or October production and the record of the other months. Note that in the biological year of this investigation these months do not fall in the same but in different calendar years.

The results are to appear in detail in *Genetics*.[3]

[1] Harris, Blakeslee, Warner, and Kirkpatrick, *Genetics, Cambridge*, 2, 1917, (36–77). Also these PROCEEDINGS, 3, 1917, (237–241).

[2] Harris, *Biometrika, Cambridge*, 6, 1909, (438–443).

[3] A treatment of the relationships between part of the year's yield and the output for the entire year is in preparation by Mr. L. E. Card for a Bulletin of the Storrs Experiment Station.

TWO LAWS GOVERNING THE IONIZATION OF STRONG ELECTROLYTES IN DILUTE SOLUTIONS AND A NEW RULE FOR DETERMINING EQUIVALENT CONDUCTANCE AT INFINITE DILUTION DERIVED FROM CONDUCTIVITY MEASUREMENTS WITH EXTREMELY DILUTED SOLUTIONS OF POTASSIUM CHLORITE

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Communicated by A. A. Noyes, August 4, 1917

Any theoretical interpretation, in terms of the Ionic Theory, of the properties and behavior of any solution containing electrolytes involves as one of its essential factors a knowledge of the degrees of ionization of the electrolytes present in the solution. The most reliable method of determining the degree of ionization, α_C, of a uni-univalent electrolyte at the concentration C is by means of the relationship, $\alpha_C = \Lambda_0/\Lambda_C$, where Λ_C is its equivalent conductance (corrected if necessary for viscosity effects) at the concentration C, and Λ_0 is its equivalent conductance at zero concentration.

The value of Λ_0 is usually obtained by extrapolating to zero concentration, some empirical function which is found to represent more

or less satisfactorily the observed relation between Λ and C over at least the lower portions of the concentration range. The lowest concentration to which any observer has hitherto found it practicable to push accurate conductivity measurements is 0.0001 normal. (Measurements by Kohlrausch[1]). This limitation of the concentration range open to measurement is due to the magnitude of the 'water correction' in the very dilute solutions, and to the uncertainty involved in applying it. Conductivity water which is approximately stable in contact with the atmosphere has a specific conductance of the order of magnitude of 1.10^{-6} reciprocal ohms, due to the impurities (chiefly CO_2) which it dissolves from the air. The 'water correction' which must therefore be applied to the measured value of the conductivity of a dilute salt solution may amount at 0.0001 normal to as much as 10%, at 0.00001 normal to as much as 100%, and at 0.000001 normal to as much as 1000% of the conductivity of the salt itself. And, aside from the magnitude of the correction the proper method of applying it is rendered uncertain owing to our lack of knowledge of the exact nature and amounts of the impurities present in the conductivity water, as well as to lack of sufficient constancy in the value of the conductivity of the water during the time which is required to prepare and measure the conductivity of the salt solution.

The Λ_0 values in use at present have, consequently, been obtained by extrapolation from conductivity data which in the most favorable cases do not extend below 0.0001 normal and which in most cases stop at 0.001 normal. The values of Λ_0 selected by different observers using the same conductivity data are, therefore, almost as numerous as the different functions which have been proposed for representing the data over the range open to measurement, ranging, for example, in the case of KCl at 18° from $\Lambda_0 = 128.3$ (Kraus and Bray[2]), and $\Lambda_0 = 128.5$ (Wegscheider[3]) to $\Lambda_0 = 130.0$ (Kohlrausch; Noyes and Falk[4]). Now unless the Λ_0 value for a salt can be determined with a certainty of at least 0.01 to 0.02% we cannot hope to obtain any information concerning questions which involve a knowledge of the concentration of the un-ionized molecules of strong electrolytes in dilute solutions, such questions, for example, as the behavior of strong electrolytes with respect to the requirements of the Mass-Action Law.

About eight years ago the writer became convinced that the only possible way of securing an answer to these much discussed questions was to obtain accurate conductivity data in the range of concentrations below 0.0001 normal, and that, in spite of the great difficulties which such an investigation presented, the questions at stake were of

sufficient importance to make the attempt worth while. Accordingly, with the aid of a grant from the Bache Fund of the National Academy, the investigation was begun in 1911.

The first part of the problem consisted in developing and perfecting the Kohlrausch method for measuring the conductance of electrolytes so that an accuracy of better than 0.01% could be obtained in measuring the conductance of very pure water and of these very dilute solutions. With the valued assistance of Dr. J. E. Bell and Mr. Karr Parker this part of the problem was successfully solved, and the results have already been published.[5]

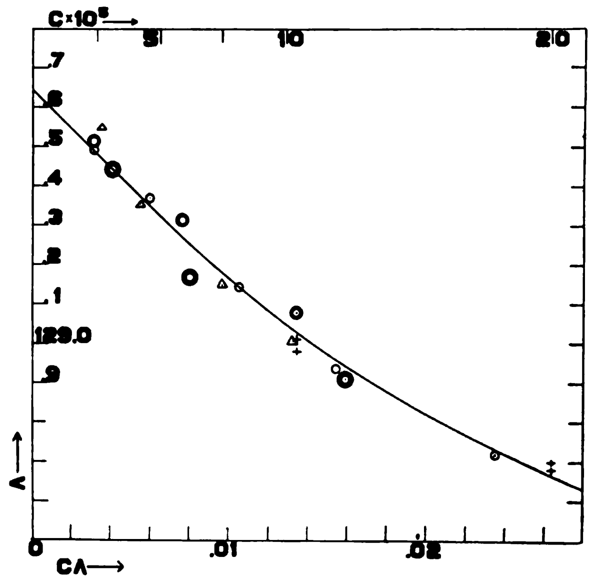

FIG. 1

The second part of the problem resolved itself into the preparation of large quantities of conductivity water which should be practically chemically pure, and the preservation of the purity of this water during the operations of preparing and measuring the conductivity of the solutions. This was successfully accomplished by means of a large quartz still connected to a three-liter quartz conductivity cell provided with platinum electrodes sealed into quartz tubes, the distillation being carried out in a current of carefully purified air. In this way three liters of conductivity water having a specific conductance at 18° of only 0.05×10^{-6} reciprocal ohms could be obtained and preserved in the conductivity cell for at least 12 hours without any appreciable increase in its specific conductance. The solutions were made up by

dropping into this cell small individual crystals of potassium chloride weighed out on an assay balance. In this way the conductivity data were carried down to 0.00002 normal.

The values obtained are shown in figure 1 (Λ, $C\Lambda$ curve), which is taken from the thesis of Dr. H. J. Weiland,[6] to whose careful work the final success of the investigation has been largely due. In this figure are represented four independent series of experiments, through which the best representative curve is drawn. The small crosses show the values found by Kohlrausch and Maltby.

In order to determine the value of Λ_0, and also the limiting value, K_0, of the mass-action expression as the concentration approached zero, it is necessary to extrapolate from 0.00002 normal. Of the various methods which have been employed for extrapolating conductivity data, those of Kohlrausch[1] and of Noyes,[4] as well as all others which are based upon the assumption that the electrolyte does not obey the Mass-Action Law within any concentration range must, it seems to the writer, be rejected because the known behavior of such electrolytes is entirely in harmony with obedience to the Mass-Action Law in sufficiently dilute solutions. Functions of the form

$$\frac{\alpha^2 C}{1-\alpha} = \frac{\Lambda^2 C}{\Lambda_0(\Lambda_0 - \Lambda)} = K_E = K_0 + k\left(C\frac{\Lambda}{\Lambda_0}\right)^h$$

which have been proposed and employed by Kraus and Bray[2] must also be rejected, because, although in form they reduce to the Mass-Action Law when $C = 0$, they cannot as a matter of fact be made to represent the conductivity data in dilute solutions, being nothing more than empirical interpolation equations which will express approximately (0.1%) the variation of conductivity with concentration between 0.001 and 2 normal. The recent function proposed by Bates,[7] $\log K_E = \log K_0' + k'\left(C\frac{\Lambda}{\Lambda_0}\right)^{h'}$ expresses very accurately the conductivity data for potassium chloride between 0.0001 normal (the lowest concentration reached by Kohlrausch) and 1 normal, but it also cannot be made to express the conductivity data below 0.0001 normal. Both the Kraus equation and the Bates equation fulfil the condition of obedience to the Mass-Action Law when $C = 0$, but they both impose upon the electrolyte an arbitrary method of approach to the condition of constancy required by the Mass-Action Law. The two methods of approach are both radically different and both wrong, since neither form of function can be fitted to the data in the most dilute solutions. In fact, any method of extrapolation which imposes an arbitrary a priori determined path by

which the values of K_E shall approach constancy must obviously be rejected if we expect to obtain reliable values for Λ_0 and K_0.

In order to avoid the errors involved in what may be called the 'arbitrary function' method of extrapolating, and also in order to avoid the uncertainties involved in attempting a direct graphical extrapolation, the following method was devised. It rests only upon the following two assumptions: (1) That as C decreases, the value of the mass-action expression K_E also decreases and gradually approaches a constant,

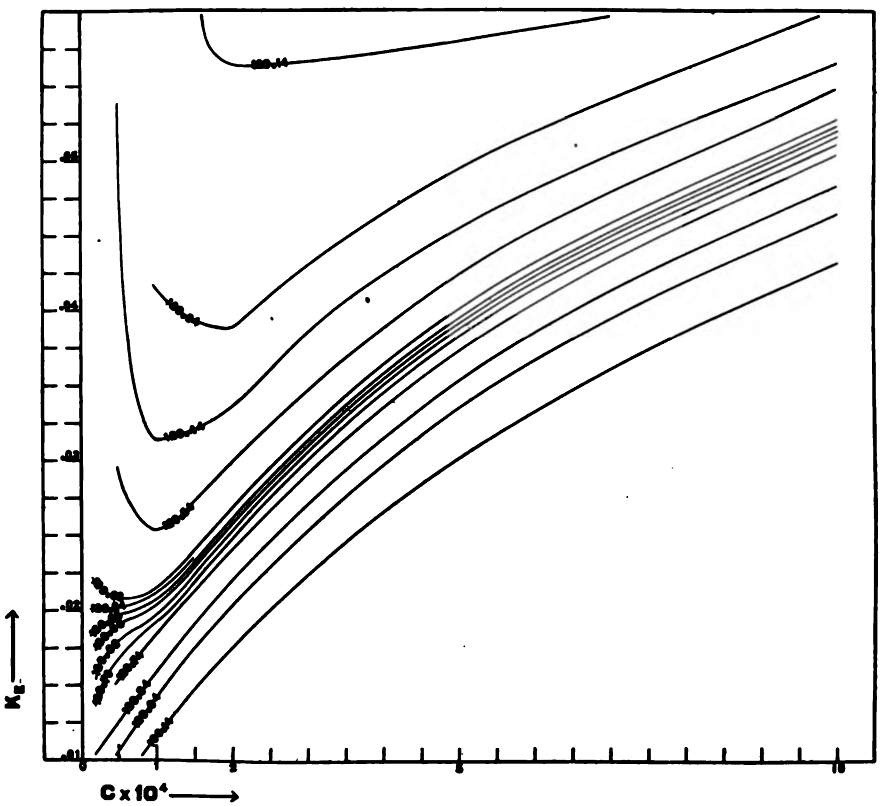

FIG. 2

K_0, at extreme dilutions; and (2) that when the value of K_E once begins to approach a constant value, it will not thereafter, that is in more dilute solutions, exhibit any erratic behavior such as a rapid rise as the concentration decreases, or a sudden fall under the same conditions. In other words, it is assumed that those influences which cause a strong electrolyte to deviate from the Mass-Action Law gradually and steadily become smaller and smaller and finally disappear at high dilutions.

The method consists simply in plotting values of K_{∞}, the mass-action expression, against values of the concentration, employing different assumed values of Λ_0 and rejecting such values of Λ_0 as cause the curve in dilute solutions to exhibit sudden changes in direction. In this way it is found that, if too small a value is employed for Λ_0, the curve will eventually shoot upward; while, if too large a value is employed, it will turn downward in the lower concentration ranges. By employing this method it is possible to determine the value of Λ_0 with a certainty of 0.01%, provided the conductance data themselves are as accurate as this and extend at least as low as 0.00002 normal.

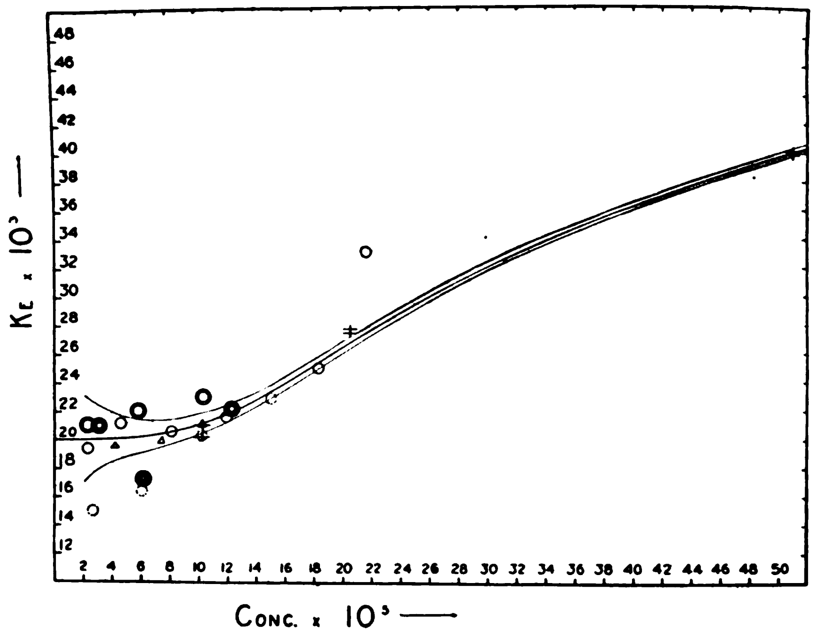

FIG. 3

This will become evident from an examination of figure 2, which has been constructed from conductance values which are substantially those represented by the curve in figure 1. It is also evident from this figure that the value of Λ_0 for potassium chloride at 18° is 129.64 ± 0.02, and that the value of K_0 is 0.020 ± 0.001.

Figure 3 illustrates the behavior of potassium chloride with respect to the requirements of the Mass-Action Law and shows that this law is obeyed within the experimental error in the concentration range 0 to 0.0006 normal. The different series of points represent independent sets of measurements. Those indicated by the dotted circles represent

a rejected series. The two bounding curves above and below the heavy curve represent the shift in the position of this 'best' curve which would be produced by a total error of 0.02%; for instance, an error of 0.01% in the concentration in combination with an error of 0.005° in the temperature of the solution. Since most of the observed points fall within the limits set by these two curves, it is evident that the agreement among them is even better than could be expected, considering all of the difficulties and chances of error involved in the work. A detailed account of Dr. Weiland's work will be published later.

With the aid of the conductivity data for potassium chloride presented above, it becomes possible to establish two general laws with respect to the behavior of strong electrolytes in dilute solutions. These laws may be stated as follows:

1. In sufficiently dilute solution (i.e., for all practical purposes below 0.0001 normal) all uni-univalent salts of strong acids and bases obey the Mass-Action Law, and all of them have the same ionization constant.

2. In sufficiently dilute solutions the values of the mass-action expression for all such salts are identical, the identity persisting up to higher concentrations the more nearly the salts under comparison resemble each other, and in any case persisting within the experimental error up to 0.0002 normal for all salts which have been accurately measured at these concentrations. In the case of two salts, such as potassium chloride and potassium bromide, for example, which resemble each other very closely, this identity persists up as high as 0.005 normal.

With the aid of the second of these laws it is now possible to derive a general rule by means of which the Λ_0 value for any uni-univalent salt can be accurately obtained from the value of the equivalent conductance of that salt at a single concentration, say 0.0001 normal. This rule is deduced as follows:

The second law stated above is expressed mathematically by the equation,

$$\frac{\Lambda^2 C}{\Lambda_0(\Lambda_0 - \Lambda)} = K_E$$

where the value for K_E is independent of the nature of the salt, and can therefore be read off from the curve in figure 3 (or preferably from a similar curve for a salt resembling as closely as possible the one under examination). Solving this expression we find (with sufficient accuracy), $\Lambda_0 = \Lambda_C\left(1 + \frac{C}{K_E}\right)$, an expression by means

of which the value of Λ_0 for any salt can be calculated from a single value of Λ_C for that salt. A convenient value of C to employ generally for this purpose is 0.0001, and for this concentration the above expression becomes $\Lambda_0 = 1.00476\ \Lambda_{0.0001}$ for all uni-univalent salts of strong acids and bases.

The reason why strong acids and bases themselves, as well as salts of weak acids and bases, are not included in the scope of the laws as formulated above, is not because there is any reason to suppose that these substances form exceptions to the laws, but merely because there are no reliable data available for testing the laws in any of these cases, nor can such data be secured except by employing extremely pure water, such as that described above.

The new rule for calculating Λ_0 has been tested at concentrations 0.0001 and 0.0002 in the case of all salts for which there are reliable data available, and in every instance the rule applied to both concentrations is found to give within the experimental error the same Λ_0 value. As an illustration of this, the data for a few of the salts whose conductivities are most accurately known are shown in the accompanying table. A general revision of Λ_0 values and ionic conductances on the basis of the above laws will be published later.

TABLE

Illustrating the Calculation of Λ_0 Values for Typical Uni-Univalent Salts by Means of the General Relation, $\Lambda_0 = \Lambda_0\left(1 + \frac{C}{K_E}\right)$, and Demonstrating the Identity of the K_E Values of all Such Salts up to $C = 0.0002$. [Measurements by Kohlrausch and Maltby,[1] Corrected (+ 0.02%) for Metathesis but Not for Atomic Weights.]

$C \times 10^4$ equivalents per liter	NaCL		KNO_3		LiCl		$LiNO_3$	
	Λ_C	Λ_0	Λ_C	Λ_0	Λ_C	Λ_0	Λ_C	Λ_0
1.037	108.20	108.72	125.48	126.09	98.17	98.63	94.48	94.92
	108.11	108.63	125.54	126.15	98.11	98.57	94.44	94.88
	108.04	108.57						
$K_E = 0.0212$	Mean	108.64± .05	Mean	126.12± .03	Mean	98.60± .03	Mean	94.90± .02
2.053	107.83	108.65	125.16	126.11	97.85	98.58	94.16	94.90
	107.82	108.64	125.22	126.17	97.86	98.59	94.14	94.88
	107.82	108.64						
$K_E = 0.0268$	Mean	108.64± .01	Mean	126.14± .03	Mean	98.59± .01	Mean	94.89± .01
0.5212								
	108.41	108.69						
$K_E = 0.02017$								

[1] Friedrich Kohlrausch, *Gesammelte Abhandlungen*, Leipzig, 1911.

[2] Kraus, C. A., and Bray, W. C., *J. Amer. Chem. Soc., Easton, Pa.*, **35**, 1913, (1412).

[3] Wegscheider, *Zs. physik. Chem., Leipzig*, **69**, 1909, (603).

[4] Noyes, A. A., and Falk, K. G., *J. Amer. Chem. Soc., Easton, Pa.*, **34**, 1912, (461).

[5] Washburn, E. W., and Bell, J. E., *Ibid.*, **35**, 1913, (177); Washburn, *Ibid.* **38**, 1916, (2431); Washburn and Parker, K., *Ibid* **39**, 1917, (235).

[6] Weiland, H. J., *Urbana, Univ. Ill. Theses*, 1917.

[7] Bates, S. J., personal communication to the writer. See Washburn's *Introduction to the Principles of Physical Chemistry*, New York, 1915, p. 216. Bates does not propose his function as a basis for selecting the value of Λ_0. For this purpose he employs a method which, while based upon substantially the same character of assumptions as the one adopted by the writer and giving nearly the same results, does not seem so direct and simple in its application. See Bates, *J. Amer. Chem. Soc., Easton, Pa.*, **35**, 1913, (526).

ON THE GROWTH AND FECUNDITY OF ALCOHOLIZED RATS

By E. C. MacDowell and E. M. Vicari

STATION FOR EXPERIMENTAL EVOLUTION, COLD SPRING HARBOR, N. Y.

Communicated by C. B. Davenport, August 16, 1917

In the course of an experiment conducted primarily for a different purpose, we have obtained data upon the effect of large doses of inhaled alcohol on the growth and fecundity of a series of albino rats. From the time of weaning (28 days) the alcohol was administered daily. Except for the first few weeks when the doses were lighter, the rats were left in the inhalation tanks till they could no longer stand upon their feet and could move but very feebly, if at all. The purpose was to give as heavy a dose as possible without killing the animal. This took different times for different rats, and the time required on different days varied according to the intensity of the alcohol vapor in the tanks. No data were recorded as to the time required to regain normal activity, but it may be said in general that this was roughly a matter of three to four hours.

For each rat alcoholized, a normal control from the same litter and of the same sex was raised. These controls were kept in companion cages adjacent to their corresponding alcoholic sibs. Except for the alcohol treatment, all the rats were given the same care and attention.

Weekly weighings were made of all the rats and individual growth curves were plotted. From these curves the weights at the ages given in the standard growth curves of Donaldson were estimated. In making these estimations, allowances were made for temporary losses in weight due to food conditions or minor illnesses; in doing this the whole curve of the individual was considered as well as the curves of its sibs and the standard Wistar curves. The weights so obtained were averaged for the two groups of rats, and these averages plotted in figure 1.

The weights given for the first point (30 days) are mainly approximations, as the first weighings of many of the rats were not taken till after this time. For this reason the weights at 40 days have been given. It will be noted that the averages of both sets of rats for 90 days are a little low. This lowering is unquestionably the result of a reduced diet that was given to all the rats in connection with a series of behavior experiments.

The divergence shown between the two curves is sufficiently clear to make it impossible to question the conclusion that the alcoholics

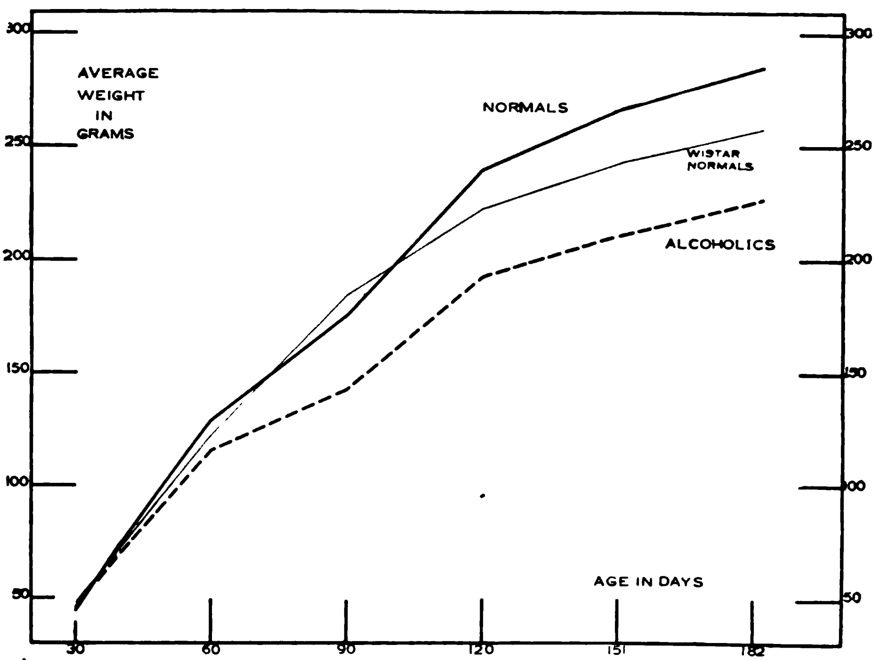

FIG. 1

have suffered a loss in weight as a result of the treatment with alcohol. The amount of this loss as shown in the curves is very close to 20% of the normal weight at 182 days. The light line in the figure was plotted from the averages given by H. H. Donaldson, *The Rat*, (Philadelphia, 1915), page 113, table 67. As all the rats here described were bred from the Wistar standard stock, the higher averages of these normals appear to indicate that the food and environmental conditions have been especially favorable. It follows from this that the lowering of the weights of the alcoholic rats is not partly due to unfavorable conditions.

In this report the males alone have been considered. As the corresponding females were used for breeding, the frequent pregnancies rendered it extremely difficult to estimate the normal weights at the specified ages. Moreover, the females are less satisfactory for this study since during the times they were nursing their young the alcohol treatment was suspended, although during pregnancy up to the time for the young to be born, they did receive the alcohol.

The reduction in the number of young born to alcoholic parents is even more striking than the loss in weight. The figures in table 2 show that, while 29 pairs of normal rats produced 300 young, 30 pairs of alcoholic rats produced in the same time 108 young. The summaries for the four lines all show a similar decrease in fecundity of the alcoholics as compared with the normals as is shown in the grand total.

The discussion and a more critical analysis of these results will be reserved for an extended account at a later date.

TABLE 1

AVERAGE WEIGHTS (IN GRAMS) OF ALCOHOLIZED MALE RATS COMPARED WITH THEIR NORMAL BROTHERS

AGE IN DAYS	ALCOHOLICS		NORMALS		DIFFERENCES IN BODY WEIGHT
	Number of individuals	Average body weight	Number of individuals	Average body weight	
		grams		grams	
30	27	46.2	22	45.4	0.8
40	27	70.5	22	74.5	4.0
60	27	116.1	22	128.7	12.6
90	26	142.3	22	174.9	32.6
120	22	192.5	21	239.8	47.3
151	16	211.7	18	267.0	55.3
182	10	227.1	13	285.0	57.9

TABLE 2

FECUNDITY RECORDS OF ALCOHOLIC RATS COMPARED WITH NORMAL RATS; MATINGS OF ALCOHOLICS AND THE CORRESPONDING NORMALS MADE ON THE SAME DAY, AND THE RECORD OF BIRTHS TAKEN DURING EQUAL PERIODS

GROUP	NUMBER OF PAIRS		NUMBER OF YOUNG BORN	
	Alcoholics	N rmals	Alcoholics	Normals
A	10	10	23	75
B	4	3	13	31
C	7	8	30	81
KC	9	8	42	113
Total	30	29	108	300

NATIONAL RESEARCH COUNCIL

MINUTES OF THE MEETING OF THE NATIONAL RESEARCH COUNCIL HELD ON THURSDAY, APRIL 19, 1917, IN ROOMS 42 AND 43 OF THE UNITED STATES NATIONAL MUSEUM, WASHINGTON, D. C.

The Chairman of the Council, Dr. George E. Hale, presided and called the meeting to order at 10.15 a.m. The following members were present: Dr. Alsberg, Dr. Baekeland, Dr. Bogert, Dr. Carty, Dr. Clarke, Dr. Conklin, Dr. Coulter, Dr. Davis, Dr. Day, Mr. Dunn, Admiral Earle, Mr. Freeman, General Gorgas, Dr. Hale, Dr. Holmes, Dr. Keen, Mr. Manning, Dr. Millikan, Dr. Moore, Dr. Noyes, Dr. Pupin, Mr. Skinner, General Squier, Dr. Stratton, Mr. Swasey, Chief Constructor Taylor, Dr. A. E. Taylor, Dr. Vaughan, Dr. Walcott, Prof. Webster, Dr. Welch, Dr. Whitney, Professor Wood, Dr. Yerkes; and the Secretary, Cary T. Hutchinson.

The minutes of the organization meeting of the Council held in September, 1916, and of meetings of the Executive Committee held since then, were presented by the Secretary, and upon motion were adopted as printed in the PROCEEDING OF THE NATIONAL ACADEMY OF SCIENCES.

The Chairman stated that the main purpose of the present meeting was to offer an opportunity for a discussion and determination of methods for advancing the work of the Council as affected by actual war conditions. He spoke particularly in this connection of the work of the Foreign Service Committee of the Council.

The Chairman referred to the material assistance furnished to the Council by the Engineering Foundation, and to the need for additional donations with which to continue its work.

Dr. Stratton as Secretary of the Military Committee, presented a report for that Committee laying emphasis upon the customary mode of procedure in selecting and detailing a scientist, or a group of scientists, to work upon definitely specified problems in direct co-operation with representatives of the War Department and the Navy Department of the Government.

Discussion took place with reference to the desirability of publication of lists of problems for distribution among men of science.

The Chairman mentioned the appointment of Dr. Millikan as Vice-Chairman and Executive Officer of the Council, and the presence in Washington of the chairmen of many important committees.

Modes of procedure to be adopted in organizing the Washington Office of the Council, and in bringing about the necessary co-operation between committees were discussed, and the need was emphasized for the publication of statements concerning the activities of the Council, particularly for the use of members of the Council and of Chairmen of Committees.

Discussion also took place concerning the availability of men for special work, and concerning methods to be used in designating them for such work.

The Chairman, in the absence of Professor Chittenden, submitted a report of progress for the Committee on Research in Educational Institutions.

Dr. Millikan spoke of the work of his office and of his efforts to bring about co-ordination in the solution of specific problems.

Dr. Coulter spoke of the activities of the Botany Committee, this leading to a discussion of co-operative measures necessary in dealing with the subject of botanical raw materials.

Dr. Carty outlined plans for the work of the Committee on Industrial Research.

Dr. Noyes submitted statements concerning the existing conditions of the nitrate problem with particular reference to its association with agricultural work. He reported that the Committee of the Council on this subject as recommended in its report to the Secretary of War has been superseded by an official Government committee.

In the absence of Dr. Stratton, the Chairman reported progress for the Committee on Census of Research.

Dr. Bogert, Chairman of the Chemistry Committee, discussed general questions of organization and spoke of the close co-operation obtained in the work of his Committee between laboratories of individuals, universities, manufacturers, and the Government.

The Chairman, in the absence of Professor Pickering, reported upon the work of the Astronomy Committee.

Dr. Vaughan submitted a detailed report for the Committee on Medicine and Hygiene, calling attention to suggestions regarding problems, and to the advisability of appointment of special committees to consider these problems.

In the absence of Dr. Cannon, the Chairman reported progress for the Physiology Committee.

Dr. Davis, Chairman of the Geography Committee, reported upon the immediate need for increasing the preparation of topographic maps, for the publication of aero maps, and for the preparation of military hand books for various parts of the United States and for Central Europe. He also expressed the hope that the next census of the United States would provide for a physiographic division of the country.

Dr. Clarke, Chairman of the Geology Committee, outlined problems relating to military affairs and suggested means of co-operation on this subject.

Dr. Holmes, Chairman of the Anthropology Committee, read a report and stated that the activities of his Committee at present relate to questions concerning recruits and methods of examination, and also to statistics arising therefrom.

Mr. Dunn, Chairman of the Enginering Committee, spoke of the formation of this Committee, and of its co-operation with the national engineering societies.

Dr. Moore, Chairman of the Mathematics Committee, reported that the Committee is in a position to consider and aid in the solution of any problems of a mathematical nature which may be submitted by the Government.

Dr. Yerkes, Chairman of the Psychology Committee, emphasized the need of psychological studies of recruits, and mentioned the far-reaching significance of such studies in the selection of men for special purposes.

The meeting adjourned at 1.50 p.m.

CARY T. HUTCHINSON,
Secretary.

NATIONAL RESEARCH COUNCIL

MEETINGS OF THE EXECUTIVE COMMITTEE

The twenty-fourth meeting of the Executive Committee convened in the offices of the Council in the Munsey Building, Washington, D. C., on July 12, 1917, and was called to order at 9.30 a.m. by the Chairman of the Counci .

Messrs. Bogert, Hale, Millikan, Pupin, and Stratton were present and, by invitation, Messrs. Durand and Mendenhall.

The Chairman reported:

1. That the Director of the Council of National Defense has requested the National Research Council to submit to him regular monthly reports of its activities, and that in order to comply therewith the Chairmen of the Committees of the Research Council have been asked to forward reports periodically to the Washington office with regard to the nature, organization and progress of investigations, and the results of researches, inquiries, and conferences bearing upon the work of the Council.
2. That Admiral William S. Benson and Dr. W. F. Durand have been appointed members of the Council by the President of the National Academy of Sciences.

The Chairman read a letter from General Squier, dated July 2, requesting the Research Council to act as the advisory agent of the U. S. Signal Corps in the organization of its various scientific activities and in the solution of research problems and suggesting that Dr. Millikan apply for a Major's Commission in the Officers' Reserve Corps, for detail in charge of this work. Upon motion it was voted that this request be complied with.

Before this formal arrangement was entered into with General Squier, upon his request that the Council recommend some one to take charge of the sound ranging work of the Army, Dr. Augustus Trowbridge of Princeton was designated. Upon motion the Executive Committee ratified this action.

Dr. Millikan reported that General Squier had referred to the Council a cablegram from Colonel Russell of the American Expeditionary Force in France, asking that a research and inspection group be sent over to France at once. He also stated that Major Carty has designated Mr. E. H. Colpitts and Mr. H. E. Shreve for signal work in this connection.

Dr. Durand reported on the question of the development and production of aeronautic instruments, and upon the need for a scientific advisor in this field. It was voted, upon motion, that the National Research Council recom-

mend to General Squier the appointment of Dr. C. E. Mendenhall in charge of the design and development of all instruments used on airplanes.

Dr. Millikan reported that on recommendation of the National Research Council the General Munitions Board held a special meeting devoted to the problem of gas warfare, to which representatives of the Departments of the Government having any connection with the ubject were invited, as well as members of the Noxious Gases Committee of he Council. Dr. Ames and Dr. Dakin of the Foreign Service Committee were also present; and the subject under discussion was presented at leng h by M. Grignard of the French Scientific Mission. As a result of this meeting the Chairman of the General Munitions Board was authorized to appoint a special committee to make recommendations concerning a pro er or anization of gas warfare. Dr. Millikan has been appointed on this committee as a representative of the National Research Council.

Upon recommendation of Dr. Bogert, as Chairman of the Chemistry Committee:

The appointment of a Subcommittee on Synthetic Drugs with Professor Julius Stieglitz of the University of Chicago as Chairman, was approved.

The appointment of Dr. Prevost Hubbard as Chairman of the Subcommittee on the Chemistry of Cements and Related Building Materials, upon the resignation of Dr. A. S. Cushman, who has entered the service of the War Department, was approved.

The appointment of Dr. George H. A. Clowes and Professor J. U. Lloyd as members of the Subcommittee on the Chemistry of Glue and other Colloids was approved; also the appointment of Mr. David Wesson as Vice-Chairman of the Subcommittee on Fats, Fatty Oils, and Soaps.

The Submarine Committee of the National Research Council with the following membership was approved: R. A. Millikan, Chairman; H. D. Arnold, W. F. Durand, Irving Langmuir, Frank B. Jewett, C. E. Mendenhall, Ernest Merritt.

It was decided that the necessary travelling expenses of the group working at New London be borne by the Research Council, this being A. A. Michelson, Chairman; Ernest Merritt, Vice-Chairman; H. A. Bumstead, Max Mason, E. F. Nichols, G. W. Pierce, H. A. Wilson, and John Zeleny.

The following appropriations were made: $300 to be expended under the authorization of the Vice-Chairman of the New London Committee for expenses in connection with submarine investigation; $670.62 for purchase of a portable house for use at the station; and $30 to Dr. H. A. Wilson for the purpose of making a condensing lens for sound rays.

The appointment of a Botanical Raw Products Committee with the following membership was approved: Edward M. East, Chairman; Oakes Ames, L. H. Dewey, Henry Kramer, George T. Moore, John E. Teeple, and additional members to be appointed later.

The organization of a Subcommittee on Forest Products of the Botany Committee was approved. The Chairman of the Council was authorized

in consultation with the Chairman of the Botany Committee to select the Chairman and members of this subcommittee.

The appointment cf the Committee on Anthropolgy with the following membership was approved: W. H. Holmes, Chairman; Ales Hrdlicka, Secretary; Charles B. Davenport, Madison Grant, Frederick L. Hoffman, E. A. Hooton, George M. Kober, Tom A. Williams. A report of this committee was presented, a copy of which had been sent to the Secretary of War, its recommendations having also been presented to the General Medical Board of the Council of National Defense.

The meeting adjourned at 12.10.

The twenty-fifth meeting of the Executive Committee convened in Room 518 Munsey Building, Washington, D. C., on July 23, 1917, and was called to order at 9.35 a.m. by the Chairman of the Council.

Messrs. Bogert, Conklin, Dunn, Hale, Millikan, Pearl, Pupin, and Stratton were present and, by invitation, Messrs. Durand and Mendenhall.

The Chairman read a letter from Brigadier-General Crozier reporting that Col. E. B. Babbitt, a member of the Committee on Noxious Gases of the Council, had been appointed to other duty in the War Department, and that Capt. Earl J. W. Ragsdale had taken Colonel Babbitt's place in the Bureau of Ordnance. Upon his recommendation therefore, Captain Ragsdale was substituted as a member of the Committe on Noxious Gases in place of Colonel Babbitt.

Upon recommendation of Dr. Hale, the sum of $100 was appropriated for current and miscellaneous expenses of the Astronomy Committee.

The Chairman stated that, after conference with the Chairman of the Botany Committee, he wished to recommend the appointment of Dr. Irving W. Bailey of the Bussey Institution as Chairman of a Subcommittee on Forestry of the Botany Committee. Upon motion, this appointment was accordingly made.

The Chairman stated that need had arisen for funds to carry on work of experimentation in connection with the investigation of devices for the magnetic detection of submarines now in progress by Dr. L. A. Bauer, Director of the Department of Terrestrial Magnetism of the Carnegie Institution of Washington. He also stated that this work had the approval of the President of the Carnegie Institution of Washington. Upon motion, an appropriation of $500, or as much thereof as may be needed, was made from the funds of the Council for the assistance desired in this connection.

Dr. Hale reported that he had recently visited the work of the special committees of the council at Nahant, Massachusetts, and New London, Connecticut, outlining the nature of investigations carried on in each place. Upon motion, it was voted that an appropriation of $2,000, or as much thereof as may be needed, be made to defray the miscellaneous expenses incident to the work of the Committee at New London.

At 9.50 the Chairman announced that the meeting would resolve itself into a joint meeting of the Executive Committee and of the Military Committee of the Council, to hear reports and recommendations of the members of the Foreign Service Committee who had returned from Europe. (A report of this conference will appear later.)

The twenty-sixth meeting of the Executive Committee convened in the offices of the Council in the Munsey Building, Washington, D. C., on July 31, 1917, and was called to order at 10.10 a.m. by the Chairman of the Council.

Messrs. Hale, Noyes, Pearl, Stratton, Vaughan, and, by invitation, Dr. Durand, were present.

The Chairman reported the recent death of Dr. William Bullock Clark, Chairman of the Subcommittee on Roads and Road Metals of the Geology Committee of the Council.

The question of defraying the cost of publication of articles relative to the work of the National Research Council in the Proceedings of the National Academy of Sciences was discussed and, upon motion of Dr. Noyes, it was voted to appropriate from time to time such sums as may be necessary to pay the cost on a pro rata basis of space used, of publishing all such reports and articles.

Upon recommendation of the Chairman of the Chemistry Committee, the following appointments were made to membership in Subcommittees of this Committee.

Sub-Committee on Biochemistry: Prof. Frank P. Underhill, Yale University, New Haven Conn.; Chairman, to replace Dr. A. E. Taylor, resigned.

Sub-Committee on the Chemistry of Ceramics: Mr. A. V. Bleininger, U. S. Bureau of Standards, Pittsburgh, Pa.; Mr. L. E. Barringer, General Electric Co., Schenectady, N. Y.

Sub-Committee on the Chemistry of Leather: Mr. Fritz H. Small, 38 Berwick Street, Worcester, Mass.; Mr. John H. Yocum, 325 Academy Street, Newark, N. J.; Mr. H. C. Reed, 227 Fulton Street, New York City.

Sub-Committee on the Chemistry of Forest Products: Dr. C. P. Winslow, Forest Products Laboratory, Madison, Wis.

Sub-Committee on the Chemistry of Dyestuffs: Mr. Wm. M. Grosvenor, 50 East 41st Street, New York City; Mr. L. DaCosta Ward, 320 So. Broad Street, Philadelphia, Pa.; Mr. R. C. Huston, Michigan Agricultural College, East Lansing, Mich.

Sub-Committee on the Chemistry of Fermentation: Dr. H. S. Paine, U. S. Dept. of Agriculture, Washington, D. C.; Dr. Max Henius, 1135 Fullerton Avenue, Chicago, Ill.

Sub-Committee on Synthetic Drugs: Dr. W. A. Puckner, 535 North Dearborn Street, Chicago, Ill.; Prof. Moses Gomberg, University of Michigan, Ann Arbor, Mich.; Prof. Roger Adams, University of Illinois, Urbana, Ill.

Dr. Hale reported that he soon expects to return to California, and discussion took place thereupon relative to the conduct of affairs of the Council during his absence. It was voted that meetings of the Executive Committee of the Council he held hereafter about once a month, or at such times as business demands, and that notices of such meetings be sent in advance accompanied by tentative orders of business.

The Chairman presented a letter from the U. S. Commissioner of Patents requesting the co-operation, advice and assistance of the National Research Council in questions of organization and conduct of affairs at the Patent Office. Preliminary correspondence with the Commissioner of Patents on this subject was also read. After discussion, a special committee consisting of Prof. W. F. Durand, Chairman, Dr. R. A. Millikan and Dr. S. W. Stratton, was appointed to confer with representatives of the Patent Office and report at a subsequent meeting of the Executive Committee relative to appropriate action which the Council may take in this matter.

The meeting adjourned at noon.

Subject to ratification at the next meeting of the Executive Committee, the Chairman subsequently appointed a special committee consisting of Dr, Millikan, Chairman, and Messrs. Bogert, Durand, Mendenhall, Stratton. and Vaughan, to meet once a week to consider questions relating to the conduct of affairs of the Council in intervals between the meetings of the Executive Committee.

CARY T. HUTCHINSON, *Secretary.*

VOLUME 3 OCTOBER, 1917 NUMBER 10

PROCEEDINGS

OF THE

NATIONAL ACADEMY OF SCIENCES

OF THE

UNITED STATES OF AMERICA

Publication Office: Williams & Wilkins Company, Baltimore
Editorial Office: Massachusetts Institute of Technology, Cambridge
Home Office of the Academy: Washington, D. C.

Entered as second-class matter at the Post Office at Baltimore, Md., under the Act of August 24, 1912

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PROCEEDINGS
OF THE
NATIONAL ACADEMY OF SCIENCES

Volume 3 OCTOBER 15, 1917 Number 10

ON THE GENERAL THEORY OF CURVED SURFACES AND RECTILINEAR CONGRUENCES

By Gabriel M. Green

DEPARTMENT OF MATHEMATICS, HARVARD UNIVERSITY

Communicated by W. F. Osgood, August 15, 1917

During the past two or three years, I have presented several communications to the American Mathematical Society concerning the general theory of curved surfaces and rectilinear congruences. I have been unable to find the leisure to write out in full my results, and take the present opportunity to gather together a few of the more important, in the hope that I may soon be in position to publish an extended treatment elsewhere.

Let the non-developable surface S be referred to a non-conjugate parametric net (u, v). Its equations in homogeneous coordinates may be taken in the form

$$y^{(k)} = y^{(k)}(u, v) \qquad (k = 1, 2, 3, 4), \tag{1}$$

where the determinant $|y_{uv}, y_u, y_v, y|$ is nowhere zero, and the four functions $y^{(k)}$ will then be a fundamental system of solutions of a completely integrable system of partial differential equations[1]

$$\begin{aligned} y_{uu} &= a\, y_{uv} + b\, y_u + c\, y_v + d\, y, \\ y_{vv} &= a' y_{uv} + b' y_u + c' y_v + d' y. \end{aligned} \tag{2}$$

The coefficients in these equations are functions of u, v which satisfy certain conditions of complete integrability, which we shall not write out here. Suffice it to say that if the integrability conditions are identically satisfied, any derivative of y is expressible, and in only one way, as a linear combination of y_{uv}, y_u, y_v, y,

$$\frac{\partial^{p+q} y}{\partial u^p \partial v^q} = a^{(pq)} y_{uv} + b^{(pq)} y_u + c^{(pq)} y_v + d^{(pq)} y. \tag{3}$$

The two points defined by the equations

$$\rho = y_u - \nu y, \qquad \sigma = y_v - \mu y, \tag{4}$$

where μ and ν are functions of u, v, lie on the tangents to the parametric curves. Let us denote by l the line joining ρ and σ; we have thus associated with each point y of the surface a definite line l lying in the corresponding tangent plane. The congruence of lines l we shall denote by Γ.

Again, if μ and ν are any functions of u, v, the point

$$z = y_{uv} - \mu y_u - \nu y_v \tag{5}$$

does not lie in the tangent plane to S at y, and the line l' joining y and z therefore protrudes from the surface. The lines l' constitute a congruence Γ'.

If, now, the functions μ and ν are the same in equations (4) and (5), the lines l and l' are in a certain characteristic geometric relation, which may be described as follows: The ruled surface $R^{(u)}$ formed by the tangents to the curves v = constant along a fixed curve u = constant is skew, since the parametric net is non-conjugate. This ruled surface is touched, in the point ρ defined by (4), by the plane determined by the line l' and the tangent $y\, y_u$. Similarly the point σ is the point in which the plane determined by l' and the other parametric tangent $y\, y_v$ is tangent to the ruled surface $R^{(v)}$ formed by the tangents to the curves u = constant along a fixed curve v = constant. The line l is therefore determined uniquely by the line l', and, conversely, if the line l is given, or in other words the points ρ and σ, the line l' is defined as the line of intersection of the tangent planes to the ruled surfaces $R^{(u)}$ and $R^{(v)}$ constructed at the points ρ and σ. The relation between the lines l and l', or between the congruences Γ and Γ', is a reciprocal one, which for want of a better name I shall call the *relation R*. It is determined of course by the particular parametric net to which the surface is referred.

The developables of the congruence Γ' cut the surface S in a net of curves whose differential equation is

$$\begin{aligned}&\left[c^{(21)} - c\mu - \nu_u + \nu\left(a^{(21)} - a\mu - \nu\right)\right] du^2 \\ &+ \left\{c^{(12)} - c'\nu - \nu_v + \nu\left(a^{(12)} - a'\nu - \mu\right) - \left[b^{(21)} - b\mu - \mu_u + \mu\left(a^{(21)} - a\mu - \nu\right)\right]\right\} du\,dv \\ &- \left[b^{(12)} - b'\nu - \mu_v + \mu\left(a^{(12)} - a'\nu - \mu\right)\right] dv^2 = 0.\end{aligned} \tag{6}$$

The quantities with double upper indices are coefficients of equations of the form (3). Likewise, the developables of the congruence Γ correspond to a net of curves on S defined by the differential equation

$$\begin{aligned}&[d-\nu_u+(b-\nu)\nu+c\mu+a(\mu_u+\mu\nu)]\,du^2\\&+\{a'[d-\nu_u+(b-\nu)\nu+c\mu]-a[d'-\mu_v+b'\nu+\mu(c'-\mu)]+\mu_u-\nu_v]\,dudv\\&-[d'-\mu_v+b'\nu+\mu(c'-\mu)+a'(\nu_v+\mu\nu)]\,dv^2=0.\end{aligned} \tag{7}$$

The consideration of these developables, together with the focal points of the lines l and l', leads to many theorems which are generalizations of known theorems concerning conjugate nets. The point-conjugate of the surface S with respect to Γ', i.e., the surface generated by the harmonic conjugate of y with respect to the focal points of l', plays an important part in the discussion.

Of more immediate interest is the case in which the parametric net consists of the asymptotic curves of S. The differential equations of the surface may then be written[2]

$$y_{uu}+2by_v+fy=0,\quad y_{vv}+2a'y_u+gy=0, \tag{8}$$

and the differential equations defining the developables of the congruences Γ' and Γ respectively

$$\begin{aligned}&[f+\nu^2+\nu_u-2b\mu+2b_v]\,du^2+(\nu_v-\mu_u)\,dudv\\&\quad-[g+\mu^2+\mu_v-2a'\nu+2a'_u]\,dv^2=0,\end{aligned} \tag{9}$$

$$\begin{aligned}&[f+\nu^2+\nu_u+2b\mu]\,du^2+(\nu_v-\mu_u)\,dudv\\&\quad-[g+\mu^2+\mu_v+2a'\nu]\,dv^2=0.\end{aligned} \tag{10}$$

Especially interesting in this connection are the directrix congruences defined by Wilczynski.[3] These are in the relation R, since the directrix of the first kind is the line joining the points

$$r=y_u-\frac{a'_u}{2a'}y,\qquad s=y_v-\frac{b_v}{2b}y, \tag{11}$$

and the directrix of the second kind is the line joining the point y with the point

$$t=y_{uv}-\frac{b_v}{2b}y_v-\frac{a'_u}{2a'}y_v. \tag{12}$$

The following new geometric characterization of the directrix congruences may be given: *two congruences Γ and Γ' which are in the relation R to the asymptotics of a surface S, are the directrix congruences of S if and only if their developables correspond to the same net on S.*

A number of propositions, which Wilczynski has proved for the directrix congruences, subsist also for any congruences Γ and Γ' in the relation R with respect to the asymptotic net of the surface.

In the general case, i.e., when the parametric net is not necessarily asymptotic, the developables of the congruence Γ correspond to a conjugate net on S if and only if $\mu_u - \nu_v = 0$. Borrowing a locution of Guichard's, used by him in a quite different connection, I shall say that a congruence Γ is *harmonic to a surface* S if its developables correspond to a conjugate net on S. *If a congruence* Γ *is harmonic to a surface* S, *or if its developables correspond to the asymptotic curves of* S, *and then a line* l *of* Γ *is met in its focal points by the tangents to the curves corresponding to the developables of* Γ, *and conversely.* This theorem is a generalization of a corresponding theorem for the case in which the parametric net is conjugate and the congruence Γ is the *ray congruence*, i.e., the congruence of lines joining the first and minus first Laplace transforms of the parametric conjugate net. The theorem affords a new geometric characterization of conjugate nets with equal Laplace-Darboux invariants.[4]

A geometric characterization of isothermal nets may be given in terms of the relation R. Let the surface be referred to an orthogonal net, and let the congruence Γ' consist of the normals to the surface. Then the parametric net is isothermal if and only if the developables of the related congruence Γ correspond to a conjugate net on S. This characterization, together with another, is soon to appear in the Transactions of the American Mathematical Society.

In what follows, I shall always suppose that the asymptotic net is parametric. Again borrowing a terminology used in a different sense by Guichard, I shall say that a congruence Γ' is *conjugate to a surface* S if its developables cut the surface in a conjugate net. Then *if the congruence* Γ' *is conjugate to the surface* S, *the related congruence* Γ *is harmonic to* S, *and conversely.* The two conjugate nets can coincide only when Γ and Γ' are the directrix congruences.

An important question naturally arises concerning the existence of congruences conjugate to a given surface and uniquely determined thereby. The surface normals form such a congruence, so that a projective generalization of metric theorems would demand the existence of a congruence projectively determined by the surface and conjugate to it. I have found that such a congruence is generated by the lines l' joining the point y with the point

$$\zeta = y_{uv} + \tfrac{1}{2}\left(\frac{a'_v}{a'} + \frac{b_v}{b}\right) y_u + \tfrac{1}{2}\left(\frac{a'_u}{a'} + \frac{b_u}{b}\right) y_v. \qquad (13)$$

The corresponding line l joins the points

$$\rho = y_u + \tfrac{1}{2}\left(\frac{a'_u}{a'} + \frac{b_u}{b}\right)y, \quad \sigma = y_v + \tfrac{1}{2}\left(\frac{a'_v}{a'} + \frac{b_v}{b}\right)y. \tag{14}$$

The following geometric characterization of these points ρ and σ will of course afford a characterization of the line $y\zeta$.

Let l be any line in the tangent ~~line~~, and l' the corresponding line through the point y. Project the asymptotic curves on the tangent plane, from any point on l', and let C_1 and C_2 be the conics which osculate these projections at y. There exists but one pair of lines l, l' such that the interesections of l with the asymptotic tangents are the double points of the involution determined by the two pairs of points in which l cuts the conics C_1 and C_2. Let R and S be these double points; they are given by the expressions

$$R = y_u + \tfrac{1}{4}\frac{b_u}{b}y, \quad S = y_v + \tfrac{1}{4}\frac{a'_v}{a'}y. \tag{15}$$

Recalling that the directrix of the first kind intersects the asymptotic tangents in the points r and s defined by equations (11), one finds that the point ρ is the harmonic conjugate of r with respect to y and R, and σ is the harmonic conjugate of s with respect to y and S. This completes the required characterization.

The points R and S, whose coordinates are given by equations (15), are of importance for still another reason. Darboux has shown[5] that in terms of the non-homogeneous coordinates of a regular point of a surface the equation of the surface may be written in essentially either of the following two forms, provided a local tetrahedron of reference be properly chosen:

$$z = xy + \tfrac{1}{6}(x^3 + y^3) + \tfrac{1}{24}(Ix^4 + Jy^4) + \ldots,$$
$$z = xy + \tfrac{1}{6}(x^3 + y^3) + \tfrac{1}{24}xy(I^1x^2 + J^1y^2) + \ldots.$$

Darboux did not characterize either tetrahedron. The tetrahedron which gives rise to the first expansion was completely characterized by Wilczynski.[8] To obtain the second expansion, three of the vertices of the tetrahedron of reference must be taken at the points y, R, S, and the fourth at the intersection of the canonical quadric with the line corresponding to RS in the relation R.

The congruence of lines $y\zeta$, since its developables cut the surface in a conjugate net, would very naturally take the place of the congruence of normals to a surface in projective generalizations of metric theorems. The said conjugate net would then play the part of the lines of curva-

ture. If this conjugate net has equal Laplace-Darboux invariants, a particular class of surfaces analogous to isothermic surfaces is defined. A projective generalization of geodesics may be made in terms of the congruence $y\zeta$, since[6] there exists a two-parameter family of curves on the surface whose osculating planes contain the lines $y\zeta$. It must be possible also to generalize a good part of the theory of triply orthogonal systems and families of Lamé, although the generalization can never be complete on account of the essential differences between metric and projective space. The field seems, on the whole, to be very promising.

[1] Green, G. M., *Trans. Amer. Math. Soc., New York*, **17**, 1916, (483–516).

[2] Wilczynski, E. J., *Ibid.*, **8**, 1907, (233–260).

[3] Idem, *Ibid.*, **9**, 1908, (79–120).

[4] Green, G. M., *Amer. J. Math., Baltimore*, **38**, 1916, (313).

[5] Darboux, *Bull. Sci. Math., Paris*, (Ser. 2), **4**, 1880, (348–384).

[6] Cf. the abstract of Miss P. Sperry, *Bull. Amer. Math. Soc., New York*, **22**, 1915–1916, (441–442). The normal congruence is there replaced by the directrix congruence of the second kind, whose developables, however, do not cut the surface in a conjugate net.

A CONTRIBUTION TO THE PETROGRAPHY OF SOUTHERN CELEBES

By J. P. Iddings and E. W. Morley

BRINKLOW, MARYLAND AND WEST HARTFORD, CONN.

Communicated August 20, 1917

In a paper in the *Journal of Geology*, Chicago, **23**, 1915, (231–245), the authors described some rocks collected in Java and Celebes in 1910. The chemical analyses of seven of these were from lavas and coarsely crystalline igneous rocks occurring in the neighborhood of Bulu Saraung (Pic de Maros). The rocks analyzed are trachytes, absarokite, nephelite-syenite and fergusite, besides kentallenite and marosite, rocks related to shonkinite.

In November, 1914, a more extended visit was made to Southern Celebes under the auspices of the Bureau of Mines of the Netherlands Government. The mountainous region from Maros to Malawa and Batuku was studied in company with Mr. 'T Hoen and Mr. Ziegler, geologists of the Bureau. The region visited consists of several nearly parallel ranges of volcanic mountains, whose lavas are underlaid by faulted and dislocated strata which are exposed in the valleys and along the base of the volcanic ridges.

The faulting and dislocation of the limestones and coal-bearing shales antedated the eruption of the igneous rocks, for the distorted strata are overlaid by volcanic breccias which form much less disturbed beds

in nearly horizontal positions in some parts of the region, but have dome-like positions in other places where great bodies of intruded lavas have lifted the sedimentary strata and overlying breccias, and have formed large laccolithic masses. The sedimentary strata are also traversed by intrusive sheets or sills of igneous rocks which occupy slightly different horizons in adjacent blocks of faulted sediments, showing that their intrusion was subsequent to the faulting and dislocation of the sedimentary strata. Dikes of igneous rocks occur more abundantly in some localities than in others, but are not very numerous.

The volcanic breccias, which are probably the oldest eruptions of the series in this region, vary somewhat in different localities and in different parts of one mountain ridge. For the most part they are basaltic in appearance with small phenocrysts of augite and olivine, and very few of feldspar. The more feldspathic varieties occur chiefly in the breccia mountains heading the valley north of Bulu Saraung, that is, the southwestern end of the region visited. These rocks are mostly trachy-andesites, with small phenocrysts of augite and calcic plagioclase, in a groundmass of more alkalic feldspar which is in part orthoclase. With these andisitic breccias are associated more basaltic varieties and smaller amounts of trachytic rocks, some of which contain leucite. While most of this breccia is without noticeable bedding, is chaotic, parts of it are distinctly bedded and contain well-worn pebbles of the same kinds of rock as those forming the chaotic breccia.

Farther north-east, in the vicinity of Malawa and Batuku, the breccia is almost wholly basaltic, with phenocrysts of augite and olivine; some varieties containing abundant small leucites, some being rich in large leucites. Leucitophyres constitute a great part of the volcanic breccias of the ridges visited, and are said to occur throughout a range of mountains at least 60 miles in length. Leucite-bearing rocks have been found in scattered localities from the Saleier Islands at the southern extremity of Celebes to the northern end, a distance of about 500 miles.

Large bodies of lava have broken through the volcanic breccias in places, and have formed masses of trachyte and phonolite. This is the case especially in the southwest. The summit of Bulu Saraung (Pic de Maros) is phonolite which is younger than the basaltic breccias and tuffs that form the ridge to the east. Other large bodies of massive lava form peaks north of the road nead Bua. Dikes of porphyritic trachyte, and of other kinds of rocks, cut the breccias and sedimentary strata beneath them. A large dike of trachyte cuts limestone in the valley of the Sangara (Gentungen) above Balotji. It is exposed in a wall 6 feet thick and 30 feet high. Intrusive sills occur in the stratified

rocks and at the base of the breccias, and have a wide range of composition. Great sheets of basalt form cliffs with limestone west of Maros near Patinuan. West of Birau, which is north of B. Saraung, there are sills of fine-grained syenite and leucitophyre. Leucitophyres form sills in the coal-bearing strata near Batuku. Laccolithic bodies of great size occur at the west base of B. Saraung, also in the valley east of Tjamba, and in the valley of Malawa. The rocks forming the laccoliths vary somewhat in composition and in grain. The largest are shonkinites, fergusite and essexite, which merge at their margins into fine-grained and aphanitic porphyries, with small porphyritic leucites. Other laccolithic bodies are medium to fine-grained rocks, some of which are more feldspathic than the shonkinites, and approach monzonites and syenites. There are phases of the laccolithic rocks very rich in biotite; augite and olivine, and others, occurring as veins in the principal rocks, that are syenites and nephelite-syenites. In contrast to these feldspathic rocks are highly mafic lavas, found as boulders in streams, which consist almost wholly of augite and olivine.

While some parts of the igneous rocks in this region are much decomposed, the great majority of the boulders in the streams, and of massive exposures in place, are extremely fresh, even the crystals of leucite and olivine, although the lavas were probably erupted in late Tertiary times. This may be due to the absence of frost and the vigorous surface action of abundant rains and strongly flooded streams.

A large collection of rocks from the localities visited shows the great variety of leucitic lavas and the freshness of the rocks in most instances. The accompanying chemical analyses of twelve specimens illustrate the most interesting varieties so far studied. In addition to the seven analyses previously published in the *Journal of Geology*, and those published and described by A. Schmidt, they furnish a fair idea of the chemical composition of the igneous rocks of this part of Celebes.

Analysis 1 is of a non-porphyritic trachytic phonolite, which forms the summit of Bulu Saraung (Pic de Maros). It is holocrystalline with a trachytoid texture, and consists of prismoid alkalic feldspar with abundant minute crystals of what is probably sodalite. There is a small amount of brownish green pyroxene, which is slightly pleochroic, colorless wallastonite, and euhedral magnetite. Analysis 2 is of a porphyritic pseudoleucite-trachyte, with large phenocrysts of altered leucite, now analcite, and fewer of orthoclase and plagioclase. Analysis 3 is of a porphypitic leucite-trachyte with phenocrysts of augite, biotite and altered leucite, in a groundmass of alkalic feldspar and biotite.

Analysis 4 is of a leucitophyre from Batuku. The phenocrysts of

leucite are large and fresh,—also of various sizes, to microscopic dimensions. The groundmass consists of small augites, prismoids of alkalic feldspar, magnetite and blades of ilmenite. Analysis 5 is of a minette, an aphanitic rock, with microscopic phenocrysts of biotite, magnetite, feldspar and augite, The groundmass consists of alkalic feldspar, biotite, augite and anhedral calcite, with some sodalite or altered leucite. The calcite is secondary.

The following six analyses are of shonkinites and leucitophyres. The shonkinites differ from one another, somewhat in composition, and might possibly be given different names. That from which analysis 6 was made is from the laccolith east of Tjamba. It resembles a medium-grained gabbro, and consists largely of augite, with less olivine and magnetite, a small amount of biotite, and considerable orthoclase, each crystal having a clouded core of altered plagioclase. There is also a zeolite, which probably replaces nephelite. The shonkinite, from which analysis, 7, was made, is much richer in biotite than the rock from East of Tjamba, is free from olivine, has abundant augite and orthoclase, besides clouded portions with a somewhat radiate structure, probably an intergrowth of feldspar and nephelite, now altered. There is also some zeolite as an alteration product. The shonkinite of analysis 8 is rich in augite and magnetite, with less feldspar, in part orthoclase, in part alkalic plagioclase, both with marginal intergrowths of a mineral with still lower refraction, which is probably altered nephelite. Analysis 10 is of another shonkinite with much augite. and orthoclase, and a zeolite which replaces pseudoleucite or nephelite.

The leucitophyre from Batuku, whose analysis is 9 has abundant phenocrysts of augite and fresh leucite, in a groundmass of small leucites and augites, with some plagioclase, magnetite, and secondary chlorite and zeolite. Analysis 11 is of a leucitophyre exceptionally rich in calcium oxide, and low in alumina. The rock has abundant phenocrysts of augite and fresh leucite, in a groundmass of small leucites, augite, anhedral wollastonite, and magnetite. The norm contains an unusually high percentage of wollastonite.

Analysis 12 is exceptional because of the low alumina and relatively high amount of potash. The rock from which it was made is an aphanitic porphyry, consisting mainly of augite with less olivine, as phenocrysts in a groundmass of augite, magnetite, and leucite. There is over 80% of mafic minerals, so that the rock is a leucitic limburgite, or a highly mafic leucitite. It is clearly an extremely mafic phase of leucitophyre, and since it does not correspond to any lava, so far described and analyzed, it seems advisable to name the rock batukite after the locality in which it occurs.

TABLE OF CHEMICAL ANALYSIS AND NORMS OF LAVAS FROM CELEBES

	1	2	3	4	5	6	7	8	9	10	11	12
SiO_2.....	58.33	56.59	50.72	49.42	47.00	48.06	46.27	46.32	46.45	46.45	45.70	47.72
Al_2O_3....	20.37	20.15	17.44	18.04	16.65	12.86	15.10	12.27	13.01	14.81	7.58	4.65
Fe_2O_3....	1.48	1.97	2.96	2.85	3.37	6.05	4.30	6.62	5.67	5.73	5.72	4.84
FeO.....	0.75	1.31	3.68	4.24	4.25	4.50	4.57	5.07	5.21	4.47	4.23	3.97
MgO....	0.30	1.09	2.98	3.16	2.58	3.26	6.70	6.34	6.44	4.97	4.76	16.98
CaO.....	2.81	3.81	5.18	6.66	6.28	12.72	8.27	12.45	11.74	9.55	21.66	15.43
Na_2O....	7.14	5.78	3.22	1.52	3.70	2.22	2.60	2.51	1.75	4.31	1.01	0.88
K_2O.....	6.17	4.63	7.74	9.24	6.90	4.71	5.04	3.49	4.82	4.25	3.79	2.27
H_2O+...	1.37	2.32	2.13	1.08	2.47	2.82	2.26	1.34	1.68	1.59	2.41	1.54
H_2O-...	0.18	0.85	0.39	1.07	0.44	0.48	0.53	0.28	0.43	0.54	0.99	0.43
TiO_2....	0.27	0.52	1.14	0.58	1.21	0.91	1.64	1.23	1.22	1.01	0.83	0.46
ZrO_2....	0.00	0.00	0.00	0.00	0.00	0.01	tr	0.02	0.01	0.00	tr	0.00
CO_2......	0.10	0.00	0.24	0.03	3.59	0.01	0.53	0.01	0.22	0.00	0.18	0.00
P_2O_5....	0.11	0.11	1.14	0.92	0.82	0.69	1.03	1.18	0.67	0.92	0.65	0.41
Cl......	0.13	0.14	0.13	0.09	0.05	0.02	0.09	0.08	0.06	0.06	0.05	0.08
F.......	0.05	0.05	0.08	0.07	0.06	0.05	0.06	0.09	0.05	0.06	0.03	0.06
S.......	0.07	0.05	0.08	0.09	0.08	0.03	0.12	0.04	0.06	0.13	0.03	0.07
Cr_2O_3...	0.00	0.02	0.02	0.00	0.01	0.01	0.00	0.03	0.02	0.01	0.05	0.11
MnO....	0.38	0.37	0.37	0.27	0.60	0.29	0.55	0.55	0.33	0.54	0.33	0.15
BaO.....	0.05	0.02	0.20	0.38	0.14	0.24	0.19	0.21	0.17+	0.71 $(\frac{2}{2})$	0.04	0.16
SrO.....	0.00	0.02	0.07	0.07	0.07	0.02	0.09	0.19	0.12	0.12	0.15	0.04
	100.07	99.80	99.93	99.58	100.29	99.96	99.94	100.32	100.13	100.23	100.19	100.25

NORMS

	1	2	3	4	5	6	7	8	9	10	11	12
or.......	36.70	27.24	45.59	40.59	40.59	27.80	29.47	20.57	28.36	26.13	11.68	11.68
ab......	35.11	42.44	10.48		3.14	10.48	7.34	7.86				
an......	5.28	15.29	10.29	15.01	8.34	11.40	14.73	12.23	13.34	8.34	5.28	1.95
ne......	13.63	3.41	9.09	6.82	15.34	4.26	7.95	7.10	7.95	13.06	4.54	4.26
lc.......				10.90						9.59	8.28	1.31
di.......	2.72	2.19	6.48	10.88	14.28	22.18	16.06	33.87	32.42	26.68	30.17	56.54
wo......	1.86					8.12					24.94	
ol.......		1.56	5.58	5.44	2.56		8.91	1.99	2.96	1.70		13.31
mt......	2.09	3.02	4.18	4.18	4.87	8.82	6.26	9.51	8.35	8.35	8.35	7.19
il.......	0.64	0.91	2.13	1.22	2.28	1.67	3.04	2.28	2.28	1.98	1.52	0.91
ap......	0.34	0.34	2.69	2.02	2.02	1.68	2.35	2.69	1.68	2.02	1.68	1.01
etc......	1.95	3.47	3.14	2.50	6.77	3.45	3.59	1.89	2.53	2.39	3.78	2.22
	100.32	99.87	99.65	99.56	100.19	99.86	99.70	99.99	99.87	100.24	100.22	100.38

1 Trachytic phonolite: beemerose-miaskose. I′. (5) 6. 1(2). (3) 4. Summit, Bulu Saraung. E. W. Morley.

2 Pseudo-leucite-trachyte: pulascose-laurvikose. I′. 5. 2. (3)4. East of Malawa. E. W. Morley

3 Pseudo-leucite-trachyte: ciminose-monzonose. II. 5(6). 2. (2)3. Stream S. W. of Bulu Saraung. E. W. Morley.

4 Leucitophyre: vicose. II. 6. 2. 2. Batuku. E. W. Morley.

5 Minette: borolanose. II. 6. ′2. 3. Dike, stream S. W., of Bulu Saraung. E. W. Morley.

6 Skonkinite: lamarose. III. 5′. 2′. 3′. Laccolith east of Tjamba. E. W. Morley.

7 Shonkinite: ourose-shonkinose. (II)III. (5)6. 2(3). 3. Laccolith, road S. of Bulu Saraung. E. W. Morley.

8 Shonkinite: ourose-shonkinose. III. (5)6. 2(3). 3. Stream, Malawa. E. W. Morley.

9 Leucitophyre: ourose-ottajanose. III. '6. (2)3. 2(3). Stream, Batuku. E. W. Morley.

10 Shonkinite: kamerunose-cascadose. 'III. (6)7. 2. 3(4). Laccolith E. of Malawa. E. W. Morley.

11 Leucitophyre: ———. IV. '2. $_1$. 1. '$_4$. 2(3). Stream, Malawa. E. W. Morley.

12 Batukite, leucite-limburgite, brunose-belcherose. IV. 1(2). $_2$. 1. $_{2\cdot(3)}$. 1(2). Batuku. E. W. Morley.

ON THE NON-EXISTENCE OF NERVOUS SHELL-SHOCK IN FISHES AND MARINE INVERTEBRATES

By Alfred Goldsborough Mayer

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Communicated August 30, 1917

Experiments made at Tortugas, Florida, during the summer of 1917 indicate that the nervous systems of fishes and invertebrates are remarkably resistant to the injurious effects of sudden explosive shocks transmitted through the water.

Many experiments were made upon the Scyphomedusa *Cassiopea xamachana*. The medusae were paralyzed by removing their marginal sense organs, and then a ring-shaped strip of subumbrella tissue was set into pulsation by an induction shock; thus producing a single neurogenic contraction which travels through the circuit-shaped strip of tissue at a uniform rate of speed, provided temperature, salinity and other factors remain unchanged. It is thus possible accurately to ascertain not only the rate of nerve conduction but also the peculiar individual characteristics of the wave in each pulsating ring.

These rings were placed in a light silken bag immersed about 10 feet below the surface of the sea; and then a half stick of dynamite was exploded within 3 feet of them. This, however, produced no effect either upon their rates or the character of their pulsation waves, although fishes possessing swim-bladders were killed within 10 feet, and injured so that they turned ventral side uppermost within 20 feet of the exploding dynamite.

When the pulsating rings were placed in glass jars or tin cans, partially filled with air, the containers were crushed or shattered by the explosion and much mechanical injury sustained by the medusa rings, which however, could at once be restored to normal pulsation by an induction shock, if their pulsations had ceased. It was also observed that the lacerated area regenerated at a normal rate.

Prof. J. F. McClendon suggested that fishes with swim-bladders might

prove to be more sensitive to explosive shocks than those without swim-bladders, and experiments showed that a half stick of dynamite may be exploded within 3 feet of a small shark, which has no swim-bladder, without producing any apparent injury. This also applies to a lesser degree to such teleosts as lack swim-bladders. Dr. S. C. Ball kindly dissected some of the fishes with swim-bladders which had been killed by the explosions, and found that the swim-bladder had burst, and the tissues were crushed in around it, often breaking the vertebral column of the fish. Moreover, Prof. W. H. Longley, who has had much experience in the use of dynamite, tells me that echinoderms, and crustacea, if not mechanically torn apart, show no apparent ill effects but however move away from the site of the explosion.

It appears, then, that the nervous system of these lower forms is extraordinarily insensitive to shock due to explosion of dynamite, and that the injurious effects of the explosion if present are due to mechanical lacerations of tissues and especially the crushing inward of air-filled cavities. It seems possible, therefore, that the cavities of the middle ear and eustachean tubes may be a source of danger to men standing near exploding shells.

It has been suggested that the sudden reduction in atmospheric pressure in the close proximity of an exploding shell might set free dissolved gases in the blood and elsewhere, thus vacuolating the tissues and producing pressure and other effects upon the nerves; but our experiments with pulsating rings of *Cassiopea* seem to negative this hypothesis for no injurious effects other than those of simple asphyxiation were produced by sudden exhaustion of the air surrounding the animals; and recovery, when replaced in normal sea water, was almost immediate.

These results are in accord with the conclusion of Grasset, Eder, Babinski et Froment, and others that war shock is predominantly a psychic phenomenon, and being hysteria it can be cured by hypnotic suggestion.

CHEMICAL DIFFERENTIATION OF THE CENTRAL NERVOUS SYSTEM IN INVERTEBRATES

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Communicated by J. Loeb, September 1, 1917

The selective action of drugs for certain tissues forms the basis of the science of pharmacology. The action of such substances as strych-

nine in increasing the sensitivity of the sensory-motor junctions of the spinal cord is definitely known. In a more general way the selective affinity of caffein, camphor and atropin for specific parts of the vertebrate central nervous system has been established.

As an instance of the application of this method to the study of the nervous systems of other forms, Baglioni[1] has attempted an analysis of the cephalopod ganglia by means of strychnine and phenol. Strychnine was found to be specific for the cerebral ganglia and phenol for the mantle ganglia.

The writer recently has found that freshly hatched squid (Loligo pealii) furnish excellent material for demonstrating reactions of the type under discussion. These little animals show a delicate sensitivity to the action of the substance used. Their response to stimulation is easily demonstrable in the muscular convulsions of tentacles, neck and mantle, and the spasms are in every case, except where specifically inhibited, accompanied by a striking play of the chromatophores. The cephalopod chromatophore is a globular sac containing either yellow, red or brown pigment. At rest these bodies appear as small dark spots, scattered inconspicuously over the surface of the mantle, head and tentacles. Muscle fibers attached radially to the chromatophore control its form. Contraction of these fibers stretches the sac into a sheet, so that the surface of a given chromatophore may be increased a hundred-fold by this mechanism.

The result of strong stimulation, such as mechanical injury or the action of potassium chloride, is a series of clonic spasms of the entire musculature, invariably involving extreme extension of the chromatophores. The play of brilliant color may therefore be used as an indicator of stimulation. If the precaution is taken to view the animals against a white background, the reaction can be followed with the naked eye. Details can be studied with the aid of the low power objective of the microscope.

In carrying out an experiment, a half dozen freshly hatched squid were put into a Syracuse watch glass containing a solution of the substance to be tested. In sufficiently dilute solutions the characteristic effects could be followed for an hour or longer. A strychnine sulfate solution (1: 100,000) maintained the squid in a highly sensitive state for over an hour, so that a slight jar was sufficient to throw the animal into spasms and bring on the play of chromatophores over the entire body.

When put into caffein solution (1: 10,000) juvenile squid show rapid circus movements. The circular character of the course is due to the

cramped bending of the neck. The tentacles also show convulsions. At the same time a lively expansion of the chromatophores takes place in the entire head region and solely in this region. Only rarely may the display extend to the mantle.

In sea water one-sixth saturated with camphor gum, the reaction of young squid is exactly the reverse of that in caffein. While the head and tentacles remain passive and their chromatophores closed, the muscular spasms and the play of color take place in the mantle. The fact that the camphor acts directly on the mantle (stellar) ganglia may be demonstrated in the following way. With the stroke of a sharp scalpel sever the head from the body of the squid. In a few seconds the results of mechanical stimulation have abated and both parts are at rest with the chromatophores closed. Now put the two parts of the animal, head and body, into the camphorated sea water. The mantellar region shows muscular convulsions and the brilliant play of chromatophores, while the head and tentacles remain quiescent unless stimulated directly.

Juvenile squid immersed in atropin sulfate solution (1:2500) show abnormally rapid swimming movements, soon followed by failure of locomotion. The animals lie on the bottom of the dish and with the microscope one may see the spasmodic contractions of the tentacles and mantle. There is no play of the chromatophores. This fact distinguishes atropin spasms from those due to strychnine, caffein and camphor. It may be that the failure of the radial muscles of the chromatophores to contract is due to the blocking of the impulses to these muscles. In such a case we would have an analogy in the action of atropin in paralyzing the terminations of the sympathetic in vertebrates.

Crustaceans are far less favorable material for the study of the selective action of drugs. This is undoubtedly in part due to a failure of the substance used to penetrate the tissues. The writer has shown that certain fresh water crustaceans are rendered more sensitive to light by treatment with solutions of strychnine, atropin and caffein.[2] Striking results may also be obtained in marine forms with saturated solutions of camphor gum. Specimens of the hermit crab (Pagurus longicarpus) after a few minutes' immersion in the solution of camphor gum in sea water are seized with tremors at each attempt at locomotion. This is due to the fact that all the appendages are thrown into convulsive trembling movements resembling the symptoms of palsy in human beings. Similar effects may be observed in the marine shrimp (Crangon vulgaris). In this form the thoracic appendages first show tremors, then complete paralysis. The swimmerets are next extended at right angles

to the median line of the body and show continuous tremors. At this stage, the animal, when stimulated, invariably swims forward, and is entirely unable to move backward. This is due to the fact that the swimmerets are able to make only the stroke which carries the animal forward. This fact may indicate that camphor acts selectively on the ganglia of the abdominal nerve cord, paralyzing the motor elements which control backward swimming, and rendering more sensitive the nervous elements controlling forward movement. Eventually complete paralysis supervenes.

Further tests were made to determine the character of the action of strychnine, atropin, caffein and camphor on starfish (Asterias), medusa (Gonionemus) and the sea anemone (Metridium). In the case of medusa and sea anemone, atropin alone was an effective reagent in increasing the animal's sensitivity. A sea anemone put into atropin sulfate solution (1:2000) upon stimulation withdraws the tentacles, contracts the sphincter, and spasmodically contracts the longitudinal muscles. The animal does not fully recover the relaxed condition in the atropin solution but revives in a few minutes in a stream of fresh sea water. Specimens of medusa put into atropin sulfate solution (1:2500) contract their tentacles and show continuous activity of the bell with an abnormally high rate of pulsation. Starfish were affected by atropin in the same way as by strychnine, viz: the animals show a strong tendency to bend the rays dorsalward, which is increased by stimulation. Caffein and camphor are not effective in increasing the irritability of starfish.

These results indicate an increased chemical complexity of the central nervous system developing *pari passu* with an increase in morphological complexity. Thus, in the actinian and coelenterate where the diffuse nerve net system obtains, strychnine, caffein and camphor exert no excitatory action. With the development of localized nerve tracts and the beginning of a central nervous system in the asteroids, strychnine, in comparatively high concentrations, produces its characteristic effects. In crustacea and the cephalopod mollusca with their highly developed central nervous systems, the strychnine is, as we should expect, effective in low concentrations. Caffein and camphor act as excitants only where the histological elements of the nervous system are completely elaborated, viz.: in crustacea and the cephalopod mullusca.

Atropine stands in a separate category, since it is effective as a nerve excitant in members of all the classes tested. It would seem, then, that atropine forms a compound with a fundamental constituent of all the nerve cells, while strychnine, caffein and camphor combine with neuronic molecules which are the products of a higher organization.

Summary.—As evidence for the chemical differentiation of the central nervous system in invertebrates, it has been shown that, in the cephalopod (Loligo pealii), caffein brings about hyperirritability of the cerebral ganglia, while camphor affects the stellar ganglia alone in the same sense. Atropin causes spasms in the squid, but inhibits the activity of the chromatophores.

Camphor shows a selective action on the central nervous system of the shrimp (Crangon vulgaris) paralyzing the elements which function in backward swimming movements and exciting those controlling forward movement.

Atropin increased the sensitivity of all the forms tested.

[1] Baglioni, S., *Zs. allg. Physiol.*, **5**, 1905, (43–65).
[2] Moore, A. R., *Science, New York*, N. S., **38**, 1913, (131–133).

PROOF OF THE MUSCLE TENSION THEORY OF HELIOTROPISM

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Communicated by J. Loeb, September 13, 1917

That the mechanism by which heliotropic organisms are oriented to light should be identical with that for the orientation to the galvanic current was demanded by the tropism theory developed by Loeb in 1888.[1] The identity of the mechanism in sessile forms was early established by Loeb, while more recently Bancroft[2] has clearly shown that the tenets of the theory hold also in the movements of *Euglena*, which are accomplished in an identical way whether under the influence of light or of the constant current.

It is the aim of this paper to show that the movements of many heliotropic insects under the influence of light are to be explained as forced motions due to the effects of light upon the tension or tonus of the muscles and that in this regard, there exists an exact analogy to the effects of the constant current in the animals investigated.

Loeb and Maxwell[3] showed that when a constant current passed from side to side of *Palæmonetes* the legs on the anodal side were flexed, while those on the cathodal side were extended. With the anode at the anterior end the anterior legs were flexed and the posterior extended. Reversal of the current reversed the condition of tension in these legs and body changes were also induced. The whole response persisted while the current flowed and put the animals in such a condition that any movement forced them toward the anode. Similarly it was shown

by Loeb and Garrey[4] that *Amblystoma* larvae reacted to the constant current in such a way that the muscle tonus was different depending upon the direction of the current. With the anode at the anterior end, the body curved ventrally and the legs were directed backward, while with the cathode at the head end the body was in the opisthotonic condition, concave dorsally, and the legs were directed forward. These differences in muscle tension made motion difficult or impossible in any direction except to the anode, toward which the animals were forced to move.

In the heliotropic insects the characteristic reactions are mediated through the eyes, the central nervous system, and thus reflexly, the musculature. Loeb proposed the following theory of the direction of the motions of such heliotropic animals.[5] The "photosensitive elements are arranged symmetrically in the body and through nerves are connected with symmetrical groups of muscles. Light causes chemical changes in the eyes. . . . . If the rate of photochemical reaction is equal in both eyes the effect upon symmetrical muscles is equal, and the muscles of both sides of the body work with equal energy. If a positively heliotropic animal is struck by light from one side, the effect on the tension or energy production of muscles connected with this eye will be such that an automatic turning of the head and the whole animal toward the source of light takes place," until the eyes are equally illuminated, when the symmetrical muscles work equally so that the animal will continue to move in this direction, or any deviation from this line of orientation will result in a repetition of the manouver.

It is our purpose to show that inequality in the illumination of the two eyes of many insects does in reality produce the differences in muscular tension demanded by this theory, that their movements are determined by this condition, that the phenomena are quite general in this group of animals and identical with those described for galvanotropism where the tension theory is proven.

Holmes[6] showed that the tonus of the muscles of the water scorpion, *Ranatra*, was markedly affected by illuminating the animal from different directions and by blackening different parts of the eyes, thus changing the equality and symmetry of illumination of the two sides. His results were in absolute harmony with Loeb's theory, the animals reacted according to its demands with 'machine-like' precision. The striking similarity to the behavior of *Palæmonetes* or *Amblystoma* when under the influence of the galvanic current would have sufficed to establish the identity of heliotropism with galvanotropism, had it not been for the diverting effect of Holmes's psychological speculation,

centering as it did upon other features in the behavior of *Ranatra*. This case stands as an isolated example whereas, the features it presents are in reality generally applicable to heliotropic animals, since our experiments have demonstrated their existence in practically every group of the insecta. They are well shown by many genera of butterflies and moths, by the bees, by many of the commoner flies including *Musca*, *Caliphora*, *Tabanus*, and *Eristalis*. While future reference will be made to these forms, the present communication will deal mainly with the reactions of the robber flies, in some forms of which the effects of light on the muscle tonus are most striking. The best of these which has come to our notice is one of the large brown forms, *Proctacanthus*, although *Promachus* and *Deromyia* show the reactions well.

Methods were used to produce unequal illumination, usually by blackening some part of the eyes with asphalt black, which is practically opaque and hardens into a brittle shell-like film upon drying.

1. Blackening all of one eye. When one eye of positively heliotropic insects is blackened, circus motions are made toward the opposite (normal) side and in all the forms studied they occur both when flying and creeping, as noted by Parker for *Vanessa antiopa*.[7] They may be noted in practically all of the commoner butterflies. Differences in the tonus of the muscles of the two sides are in evidence when the animals are at rest under constant illumination. The bodies are tilted well toward the side of the good eye and the legs of that side are flexed with the body resting against their upper segments, while the terminal segments of that side are well under the body, and those of the opposite side are extended away from the body. The head is usually rotated on the body so that both antennae may actually be below the line of the wings.

The robber fly, *Proctacanthus* (Sp. ?) shows the tonus changes of the legs much more strikingly than any other insect examined. On the side of the good eye they show a continued state of flexion involving all three legs. The anterior leg may be so far adducted as to cross under the body farther to the side of the blackened eye than the corresponding leg of that side. The legs on the side of the blackened eye are more extended than normally and spread farther apart. The body may tilt so far toward the side of the normal eye as to press the legs to the table. The head not only rotates on the long axis of the body toward the good eye but is also flexed toward that side, a considerable angle appearing between the head and thorax. In some cases the abdomen also shows flexion concavely toward the side of the good eye even when the animal is at rest. The posture assumed is the characteristic one assumed by

Palæmonetes when the constant current is passed through the body from side to side, and by *Ranatra* when illuminated from one side. The figures of the authors referred to above in this connection will serve for illustration of the conditions resulting from our experiments.

All of the muscular conditions upon which the resting postures are dependent are accentuated by activity. The robber flies move in circles by farther flexing the legs on the side of the functioning eye and extending those on the side of the blackened eye. The head bends farther toward the center of the circle and the abdomen curves so that the body is concave, forming an arc of the circle in which movement is executed.

2. *Illuminating one eye.* Bringing one eye of *Proctacanthus* into the bright beam of light directed through the objective of the optical system of the string galvanometer, while the other eye is illuminated only by the subdued light of the optical room, promptly produced the same postural relations described in the previous section. In this case the diffusely illuminated eye corresponds to the blackened eye. The result is due simply to a difference in the intensity of illumination of the two eyes. Mast[8] has produced similar tonus differences by reflecting light from a small mirror on one of the eye spots of *Arenicola* larvae; the body musculature contracted on the brightly illuminated side. This observation we have repeated and noted further that the contracted state persists as long as the light is held on the eye spot. The same curvatures have been noted by us when the constant current is passed transversely through the body of the marine worm *Podarke* and have also been described by Moore and Kellogg for the earthworm.[9] The general mechanism is the same for the action of light and the galvanic current.

3. *Blackening the lower half of both eyes equally* results in a symmetrical position of the legs of the two sides but the anterior and middle pair are extended forward to the maximal extent, producing a striking posture in which the anterior end of the robber fly is pushed up and back from the surface of the table. The front pair of legs may even be poised up in the air. The body is in opisthotonus with the abdomen concave on the dorsal side, while the head is tilted far up and back on the long axis of the body. The whole posture is that assumed by *Amblystoma* or by *Palæmonetes* as described above, when subjected to the influence of the constant current with the cathode at the anterior end. Holmes showed similar postures for *Ranatra* with a light placed above and posterior to the animal.

When walking these robber flies gave the impression of trying to climb up into the air. The wings are frequently somewhat spread and the

animal may push itself up and back until poised vertically on the tips of the wings and abdomen. The tendency to fly is very pronounced in this condition and upon the slightest disturbance the fly soars upward and backward, striking the top of a confining glass dish or completing a circle by "looping the loop" backward. If it falls upon its back it rights itself by turning a backward somersault. Unequal blackening of the lower parts of the two eyes results in a combination of the effects just described, with those described for blackening one eye, for the animal also performs circus motions.

4. *With the upper halves of the eyes blackened* the attitude is the reverse of that described in the preceding section. The anterior and middle pairs of legs are flexed. The anterior and posterior ends of the body bend ventrally with the body in emprosthotonus. The head is bent far down until its anterior aspect is parallel to the surface of the table. The animal may actually stand on its head, but the abdomen retains its ventral curvature, leaving a considerable angle open between its dorsum and the wings which normally rest on it.

In both walking and flying it continually keeps close to the table, and upon encountering an obstacle it frequently does a forward somersault. If it gets on its back it rights itself with greatest difficulty as its efforts simply result in bending the tail and head ventrally until they may form a complete ring. In galvanotropism the same general picture is presented by *Palæmonetes* and *Amblystoma* when the anode is at the head end, the tonus changes involved being identical in the two conditions.

5. *By blackening the upper half of one eye and the lower half of the other*, circus motions are performed toward the side with the lower half blackened. If for example the lower half of the right eye and the upper half of the left eye of the robber fly are blackened flexion of the anterior leg on the right side results. There is extension of the anterior leg on the left side. The body is thus somewhat twisted on its long axis for there is also flexion of the posterior leg on the left side and extension of its mate on the right side. Such a bizarre effect is hardly explicable by any recourse to the assumption that the animal is "avoiding the darkened field." In reality the tilting of the body and the twist of the head are toward the blackened part of the eye viz., *down* on the side on which the lower half of the eye is blackened, and *up* on the side on which the upper half has been blackened. The result is readily explained on the basis of a crossed innervation from the eyes to the extensor muscles of the opposite side, a view for which much evidence is at hand.

6. *Blackening corresponding halves of both eyes of Proctacanthus*, for example, the outer half of the right eye and the inner half of the left eye, causes postural changes similar to those produced by blackening all of one eye (the right) and circus motions away from the side to which the black has been applied, i.e., to the left in our example. This statement of fact is, however, capable of overemphasis for the intensity of the reaction depends upon the relative amount of black applied to each eye, thus if a greater area of the inner side of the left eye be blackened or a lesser area on the outer (right) side of the right eye the tendency to move in a circle to the left is lessened, nullified, or even reversed by combining the two procedures. This fact is an interesting and fatal contradiction to the view that the postures are assumed, and the circus motions made, in an attempt "to avoid the dark field which appears as an obstruction to the path." The reaction is a quantitative one and depends on the relative areas blackened as much as upon the part of the eye covered. The space relation of light fields to dark fields, does not change in the above experiment but the tonus of the muscles does change with the change in the areas blackened, whereby the behavior of the robber fly is reversed and it circles toward the side on which both eyes have been blackened.

When either the inner halves or the outer halves of both eyes are blackened, the muscles of the two sides remain absolutely symmetrical provided the eyes are equally and symmetrically painted. If the painted area is considerably less on one eye than on the other, the tonus changes again approach those found by blackening all of one eye and the animal, in walking, circles to the side with the greater area exposed to the action of light. Complete blackening of both eyes results in marked relaxation of all the musculature, although the two sides are symmetrical.

7. All the experiments show that the muscle tone is dependent upon the intensity of the light and that the postures assumed depend upon the relative differences in the illumination of the eyes. In animals with one eye completely covered the radii of the circles in which they moved were shorter the more intense the illumination of the normal eye. With one eye partially covered the circles were larger than when completely covered and in the same way, the circles were larger when one eye was covered by a film of collodion or of brown shellac, which admit some light, than when subsequently covered by opaque asphalt black. When one eye was partially covered by central application of the black paint the tilting and circling to the opposite side were abolished or reversed by brilliant illumination of the partially blackened eye. These re-

sults explain why a positively heliotropic animal with one eye blackened approaches a light by a series of alternating small and large circles, the former being executed when the good eye is illuminated from the source of light, the larger when it is in the shadow.

8. *Differential sensibility.* Robber flies with one eye blackened show the postural conditions in the most pronounced way in the early morning or after being kept for some hours in the dark. Constant exposure to the light produces considerable fatigue of the eye with recovery in the dark. These facts among others suggested the possibility of producing a different sensitiveness of the two eyes and corresponding differences in the muscle tonus with asymmetry of position, and in physiological action of the muscles of the two sides of the body when the two eyes were equally illuminated. Such an experiment constitutes a crucial test of the tonus theory of heliotropism. It succeeded beyond our greatest expectations. Asphalt black was applied to the right eye of several specimens of *Proctacanthus*. In two or three days the paint had formed a brittle shell. During this time the blackened eye had become 'dark adapted.' When such a fly is exposed to light, it tilts and circles to the left. If now the brittle shell is cracked off the right eye by carefully pinching with fine forceps, the exposure of this very sensitive eye to light results in a reversal of the whole picture; the fly circles toward the side from which the black was removed. Although the illumination of the two eyes is of equal intensity, what was the normal eye now becomes relatively a darkened eye owing to its lesser sensitiveness. A differential effect results, probably due to a difference in the rate of photochemical change in the two eyes. This reversal of the muscle tonus, and of forced motions, may persist for an hour or two or even longer, until the two eyes become, as they ultimately do, of equal sensitiveness and the fly behaves like a normal animal.

These experiments are not only incompatible with any avoidance idea, for after removal of the black there is nothing to avoid, but they are also incompatible with the conception of 'habit formation,' for "habit" in the performance of the circling movements is of no avail when light is admitted to the darkened eye. The animals circle to that side because the tonus of the muscles is such that they are forced to do so.

9. *Post mortem rigor.* *Proctacanthi* kept in a moist atmosphere but without animal food, have lived two or three weeks. During this time if one eye was blackened the tonus change in the muscles became almost a fixed condition, probably the result of an atrophy of the muscles or a lack of tonus, similar to the effects of disuse. Death supervening, the

onset of rigor exaggerated the condition existing prior to death and the animal stiffened in the characteristic postures. This follows the rule for rigor mortis, that the more strongly acting muscles contract more strongly after death. This fact lends additional proof for the tonus theory of the tropisms.

Conclusion.—These experiments remove, in our opinion, the last doubt that the motions of animals to or from a source of light are due to an influence of the light on the tension of muscles of different sides of the body whereby the animal is automatically carried to or from a source of light.

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[2] Bancroft, F. W., *J. Exp. Zool., Wistar Inst., Philadelphia*, **15**, ,1913, (383).

[3] Loeb, J., and Maxwell, S. S., *Arch. ges. Physiol. Bonn*, **63**, 1896, (121).

[4] Loeb, J., and Garrey, W. E., *Ibid.*, **65**, 1896, (41).

[5] Loeb, J., *The Organism as a Whole*, New York, 1916, pp. 257–259.

[6] Holmes, S. J., *J. Comp. Neur. and Psychol., Wistar Inst., Philadelphia*, **15**, 1905, (305).

[7] Parker, G. H., *Mark Anniversary Volume*, 1903, (453).

[8] Mast, S. O., *Light and the Behavior of Organisms*, 1911.

[9] Moore, A. R., and Kellogg, F. M., *Biol. Bull., Wood's Hole, Mass.*, **30**, 1916, (131).

CHANGEABLE COLORATION IN BRACHYURA

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Communicated by A. G. Mayer, September 10, 1917

Although changes in coloration have been observed commonly in crustacea, only two recorded cases appear to refer to Brachyura. Fritz Müller reported to Darwin[1] that the male of a Brazilian species of *Gelasimus* is subject to rapid and remarkable changes in appearance. R. P. Cowles[2] has also described changes in color in *Ocypoda arenaria* Say, which he believes are in the main, if not entirely, dependent upon changes in the intensity of the light and variations in the temperature to which the creatures are exposed. It would seem, however, that crabs are able at least to change their shade much more commonly than recorded observations would indicate and that other factors than those suggested may determine their coloration at a given moment.

Portunus depressifrons Stimp., *Callinectes ornatus* Ord. and *C. marginatus* A. M.-Edw. display striking changes in coloration under certain conditions. Others less obvious or less closely studied have been noted too in *Portunus spinicarpus* Stimp. and *P. sayi* Gibbes and in

Euryplax nitida Stimp., all of which as well as those mentioned above have perhaps been overlooked because they are much less apparent in mature than in immature specimens. It is also clear that in *O. arenaria* as well as in the six species last enumerated, the shade of the substratum plays a very important part in determining that of individuals resting upon it, since in each within a few minutes, usually less than ten, at temperatures commonly prevailing in the laboratory it is possible to reverse the shade of the two members of an adapted pair simply by transferring each to the dish occupied by the other.

Light colored crabs, essentially white ones, may be cooled in white dishes from temperatures between 30° and 35°C. to 12° and be completely paralyzed by the cold without changing in the least in shade. Crabs whose shade is adjusted to that of light and dark colored dishes respectively may be transposed at a temperature at least as low as 16°C. and have their coloration reversed as definitely as under warmer conditions. Cool crabs in dark dishes may be heated to approximately 35°C. before they commence to show any effect of the treatment. At higher temperatures, which vary in individual cases, they are blanched. At such temperatures it is also impossible to induce darkening, or at least the development of a distinct pattern, by transposing a light crab to a dark dish. Hence it is apparent that temperature is not the basic factor concerned in the matter, but merely one that limits adaptive color changes in *Ocypoda* near the upper level at which it is able to survive. It is possible, indeed, that the very appearance of heat-blanching indicates the abnormality of the situation, for, regardless of the shade of the substratum, a similar effect follows when the water in which the crabs are is charged artificially with CO_2.

The degree to which *Ocypoda's* coloration is independent of temperature may be demonstrated most strikingly in a single experiment by transposing specimens which have been standing in cold white dishes and warm black ones respectively. In a specific instance the coloration of two crabs was reversed without that one which was turning from dark to light ever becoming warmer than 17°C., or the other cooler than 35°. Equality of shade was attained in twenty-nine and complete reversal in about fifty minutes. Thus it is plain that temperature is so much less effective than the shade of the underlying bottom in determining coloration that the latter is able to induce adaptive changes in opposition to Cowles's laws in the face of a temperature difference of not less than 18°C. The time relation established, however, does not represent the minimum required to effect the change, since the water in the dark dish was for a time above the temperature at which heat-

blanching appears, and the darkening of the crab in it was therefore delayed.

It does not seem to be true, as has been suggested by Dr. Cowles, that the "blanching of individuals on the sands of Loggerhead Key is probably due to high temperature alone," or that during a large part of the time Ocypoda would be heat-blanched rather than adaptively colored. He observed that water standing outside the laboratory was heated to 45°C., at which temperature experiment shows that the substratum exercises a negligible effect in determining the crabs' coloration. Even higher temperatures than that recorded may occur, but this fact has only a slight bearing upon the matter at issue, for this excessive heat does not characterize the creatures' environment when they are exposed.

The animals live and feed largely in the zone below high water mark, where actual observation twice showed a temperature of 39°C. when the same thermometer lying flat in the same way on the sand 25 feet above it registered 49° and 45° respectively on the two occasions. It may also be demonstrated readily by raising the instrument half an inch on crotched sticks that the temperature prevailing at the level at which an active crab's body would actually be is considerably lower than on the sand, being 37° and 36.5° in the two cases cited. That is to say, at the hottest time in the day the temperature throughout the normal range of active crabs is very little above the minimum at which heat-blanching occurs. But at these times very few crabs are out of their burrows: five were digging them or standing idly in their mouths in the first case and only one in the second, while none at all were moving about freely over the sand where dozens might be seen both earlier and later in the cooler parts of the day. It is clear, therefore, that, unless their reactions to air and water temperatures are not the same, when the animals are actually exposed to the attacks of enemies, temperature is not sufficiently high to inhibit the effect of the substratum in inducing adaptive color changes which might reduce their conspicuousness.

So far it is apparent that the inference that adaptive color changes occur in the species mentioned rests wholly upon the demonstration of their ability to effect such changes in the laboratory. Similar adjustments are made, however, under natural conditions. It was, indeed, observation of the fact that, as one walks along a beach on which there is striking contrast between the dark and light patches covered by shallow water, the individual young *Callinectes marginatus* and *ornatus* that dart away from under foot differ in shade according as they start up from one or the other, that attracted attention to the possibility that such relations exist as have been reported in this article.

There is then every indication that further study will show that the colors of crabs and their capacity to change them vary from species to species according to the same general rules that appear to prevail among fishes.[3] But if in two groups of animals so widely separated the same laws of coloration prevail, and if the observed facts point so unanimously to the concealing function of coloration in each, it becomes increasingly improbable that other laws prevail as extensively as has been supposed among other animals, and increasingly evident that adaptation of the organism to its environment is one of the most striking of natural phenomena.

[1] Darwin, C., *Descent of Man*, Chap. 9.
[2] Cowles, R. P., *Washington, Carnegie Inst., Pub.*, No. 103, 1908, (1-41).
[3] Longley, W. H., *J. Exp. Zoöl., Wistar Inst. Philadelphia*, **23**, 1917, (536-601).

THE EQUILIBRIUM OF TORTUGAS SEA WATER WITH CALCITE AND ARAGONITE

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Communicated by A. G. Mayer, August 30, 1917

The question of the solubility of calcite and aragonite in sea water is a matter of interest in relation to the geology of limestone and dolomite. Murray and Hjort[1] maintain that sea water is so complicated a mixture that the solubility of $CaCO_3$ cannot be calculated with certainty (from the law of mass action) but that the experiments of Anderson and of Cohen and Rahen show that sea water is saturated with calcite. They add, (p. 181) that dolomite is less soluble than calcite in carbonated waters. Their book summarizes observations showing that calcium carbonate is precipitated in shallow tropical waters, but that even shells are dissolved in the red clay bottoms of the depths.

Mayer[2] placed pieces of *Cassis* shell in sea water for more than a year and found them to maintain their weight within about $\frac{1}{10}$ of 1%. The precipitation of $CaCO_3$ at Tortugas was studied by T. Wayland Vaughan, R. B. Dole, and G. H. Drew.[3] Drew observed that a denitrifying bacillus, *Pseudomonas calcis*, obtained from the sea water, was capable of changing calcium nitrate to calcium carbonate in culture media and supposes a similar process to occur in sea water. Since Vaughan has observed that calcium carbonate is constantly precipitating at Tortugas, Drew's hypothesis necessitates the presence of an appreciable amount of nitrates or nitrites, and I have attempted to determine them.

A half liter of sea water was boiled in an all-glass still and the distillate collected in a series of 25 cc. Nessler's tubes. Another series of Nessler's tubes were filled with a graded series of concentrations of ammonium chloride. One cubic centimeter of Nessler's reagent was added to each tube and agitated. After fifteen minutes, the tubes were compared colorimetrically and the ammonia recovered from the sea water was estimated. After no more ammonia could be distilled from the sea water, amalgamated aluminium shavings were introduced into the still and the distillation process repeated. The ammonia recovered was formed by reduction of nitrates and nitrites. Duplicate analyses gave less than 0.01 mgm. of nitrogen per liter as ammonia and less than 0.01 mgm. nitrogen per liter as nitrates and nitrites. Raben found more than ten times these quantities in North Sea water.[4] Evidently, *Pseudomonas calcis* and other organisms have almost completely removed the fixed nitrogen from Tortugas sea water. The effect of this probably explains the scarcity of life in the vicinity of Tortugas as compared with colder seas (law of minimum). There is, however, a constant renewal of fixed nitrogen from the atmosphere, from the decay of organisms and probably from water rising from the depths of the ocean. If *Pseudomonas calcis* is an important agent in the precipitation of $CaCO_3$, its action is evidently more intense in places where calcium salts, nitrates and nitrites are carried from the land into the sea.

That calcium carbonate is withdrawn from surface waters of the sea, is shown by chemical analyses. Dittmar[5] found an average of 0.44% less calcium in surface waters than in deeper waters. This is true notwithstanding the fact that calcium carbonate is constantly being added to the surface waters. The drainage of the land contains an excess of calcium carbonate and flows out on the surface of the sea, where the water evaporates leaving the excess of $CaCO_3$ in the sea water. The action of organisms in building calcareous structures may account for a large part of the depletion of surface waters, but the precipitation of calcareous mud at Tortugas has been observed by Vaughan.

The analysis of the calcium content of sea water requires double precipitation, and filtration for separation from magnesium and hence large samples and great care are required for accuracy. Theoretically, however, we may detect differences in calcium content by titration. Dittmar showed that except for H_2O, calcium and gases, sea water is remarkably constant in composition. The water content may be determined by titration of the chlorides and the gases may be eliminated by boiling after the addition of enough acid to decompose the carbonates. If we disregard carbonic acid, there is an excess of bases

in sea water, i.e., the sum of the base equivalents is greater than the sum of acid equivalents. Since calcium is added to or taken from sea water in the form of $CaCO_3$, and change in the calcium content causes an equivalent change in the excess base or alkaline reserve, as it is called by chemists. The alkaline reserve may be titrated while boiling the sea water to eliminate CO_2. The exact value of the titration depends on the indicator used and the exact color of the indicator that is taken as the end point, hence only those titrations done in the same manner can be strictly compared. The titrations, used for the present paper were made by adding di-brom-o-cresol-sulphone-phthalein to 100 cc. of sea water in a flask of resistance glass and titrating with 0.01 *N* HCl, while boiling, until the purple color changed to yellow and did not become purple again after boiling for 5 minutes longer. The sides of the flask must not be allowed to dry as this would cause HCl to escape from the chlorides, due to the action of Magnesium. The results per liter were recorded and some titrations were made at 20° and others at 30°, but the errors due to change in volume of the sea water is within the limits of accuracy of the method. If the sea water is diluted with rain water, the alkaline reserve will be lowered, but this error may be compensated by dividing by the chlorine content (grams chlorine per kilogram sea water). In other words: a change in the quotient of the alkaline reserve by the chlorine per cent indicates a gain or loss of $CaCO_3$.

	ALKALINE RESERVE	Cl °/°°	$\frac{\text{Alk. res.}}{\text{Cl °/°°}} \times 10{,}000$
Sea water from San Diego, Cal.	0.00235	18.7	1.257
Sea water from Woods Hole, Mass.	0.00240	17.7	1.356
Gulf Stream, off Miami, Fla.	0.00250	19.9	1.257
Gulf Stream, off Tortugas, Fla.	0.00250	19.9	1.257
Average, Tortugas, June and July	0.00247	20.0	1.235
Average, Key West, June and July	0.00237	20.0	1.185

The above table indicates that some $CaCO_3$ has been removed from Tortugas sea water, as compared with other sea water, and to a greater extent from Key West sea water. In other words, the precipitation observed by Vaughan is not due to a greater amount of calcium in Tortugas or Key West sea water but to local conditions which cause the precipitate to form.

According to the law of mass action, in a saturated solution of $CaCO_3$ in sea water at constant temperature, salinity, etc.,

$$[Ca^{\cdot\cdot}] \times [CO_3''] = \text{a constant.}$$

Not all of the calcium is, however, in the form of $CaCO_3$ and $Ca^{\cdot\cdot}$, for some is undissociated $CaCl_2$, $CaSO_4$, $Ca(OH)_2$, and $CaHCO_3$. The chlorides and sulphates are constant but $[CaHCO_3]$ and $[Ca(OH)_2]$ change with the total CO_2 content of the sea water. But I have shown[6] that if the alkaline reserve remains constant, the total CO_2 of sea water (within limits found in nature) varies inversely with the pH (= $-$ log.$H^{\cdot}$ concentration). Hence the determination of the pH may be substituted for that of the total CO_2. The determinations I have made of the water of the Pacific and North Atlantic showed the pH to vary from about 8.1 to 8.25 and those of Dr. A. G. Mayer in the Pacific showed only a little wider range (below 8). Earlier observations at Tortugas gave the same range, but my more extended observations made this summer demonstrate the inadequacy of a few determinations. The pH is influenced by plant and animal life and rises at Tortugas to 8.35 during the day over well-lighted bottoms rich in vegetation, and falls to 8.18 during the night. It may be said, therefore, that conditions in shallow water over eel-grass or other sea-weed or corals (with symbiotic algae) favor the precipitation of $CaCO_3$.[7]

The question arises whether the occasional high pH of Tortugas sea water is sufficient to explain the precipitation of $CaCO_3$, or whether nuclei for the separation of the solid phase are necessary. A large amount of $CaCl_2$ may be added to sea water without causing a precipitation. If the pH is increased by the addition of NaOH, the result depends on the speed at which the alkali is added. If the NaOH is added suddenly in the form of a strong solution, colloidal precipitation membranes form about the drops and if the membranes are broken by shaking or stirring, a great mass of $Mg(OH)_2$ is included in the precipitate. If a very dilute solution of NaOH is added very slowly, $CaCO_3$ possibly contaminated with $Mg(OH)_2$, is precipitated. The exact pH at which precipitation first occurs cannot be determined by this method as the first precipitation occurs before the solutions are mixed and the crystals thus formed serve as nuclei for further precipitation. If Tortugas sea water is kept in glass bottles, precipitation occurs on the glass while the pH of the water is within the natural limits, but the pH at the glass surface is higher, due to solution of glass.

Although the pH at which precipitation would occur without nuclei for the separation of the solid phase, may be practically impossible to determine, the final equilibrium with an abundance of nuclei is not a difficult problem. Calcite and aragonite crystals to serve as nuclei were produced by the methods of Johnston, Merwin and Williamson.[8] The crystals were examined under the microscope and tested with cobalt

nitrate solution. These observations, together with the mode of preparation, leave little doubt that the crystals actually were calcite and aragonite. Under the microscope an occasional calcite crystal could be found among the aragonite crystals but the number was not sufficient to affect the cobalt nitrate test. These calcite crystals apparently grew slightly during the experiments but apparently no new ones were formed. To determine the equilibria, crystals were mechanically stirred or shaken with sea water in 'nonsol' flasks, six to fourteen hours at 30°, then the pH and alkaline reserve determined.

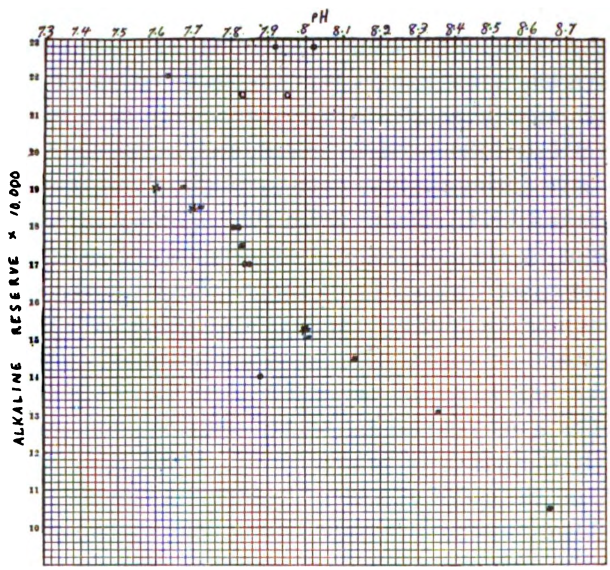

FIG. 1

The results are shown in figure 1. The alkaline reserve is measured on the ordinate and the pH on the abscissa. The results of agitating 100 grams of calcite crystals with a liter of sea water until equilibrium was approximately reached are shown by black rhombs in figure 1. If we draw a straight line from the intersection of the ordinates of pH 7.3 and alkaline reserve 0.0023 to pH 8.8 and alkaline reserve 0.0009, the determinations with calcite will fall very close to it. This shows that sea water of the surface of the oceans of the whole world is supersaturated in respect to calcite. We may therefore conclude that suitable nuclei for the precipitation of calcite are absent or deficient in number. The solubility of crystals varies inversely with their size, but after they have attained sufficient size to be readily examined with low powers of the microscope, further increase has an unappreciable effect on solubility. But such crystals if present, would be rapidly precipitated to the bot-

tom of the sea, hence the absence of nuclei for precipitation of calcite is what one might expect.

Aragonite is said to be about 10% more soluble than calcite, but no difference in the point of equilibrium of the two substances with sea water was detected in these experiments. This may be explained by the facts that a few calcite crystals were mixed with the aragonite, equilibrium was only approximated and there were slight errors in the determinations. The results are shown by black rosettes in figure 1.

During the rough weather, white calcareous mud is stirred with the sea water at Key West and to a lesser extent at Tortugas, and it was thought possible that the mud granules might form nuclei for precipitation and explain the low alkaline reserve at Tortugas and lower alkaline reserve at Key West. On agitating white calcareous mud, dredged from the bottom, with sea water, no definite equilibrium was reached, even at the end of four days. If the alkaline reserve was first lowered by removal of some $CaCO_3$, it remained lower than if shaken with calcite and if normal sea water was used the alkaline reserve remained higher than with calcite. It was thought possible that the grains were covered with an impenetrable film of organic matter; so some mud was dried and powdered in a mortar in order to break the pieces and form fresh surfaces, but similar results were obtained with this. The results are shown by black circles in figure 1.

If mud was mixed with an equal weight of calcite, the results were the same as with pure calcite, as shown by the black dumb-bells in figure 1. This mud and calcite on standing in sea water for thirty days had not changed to calcite. All these facts tend to show that the particles in the mud are in some way retarded or prevented from getting into equilibrium with the water.

In order to be sure of clean surfaces of natural calcareous substances, a specimen of coral, *Maeandra clivosa*, was ground and powdered in a mortar and agitated with sea water in the same manner as in previous experiments. The results were similar to those with mud, as shown by the white circles in figure 1.

There seems to be a more soluble form of calcium carbonate (the μ $CaCO_3$ of Johnston), but since it cannot be obtained in a pure state, no attempt was made to prepare it. One experiment, however, was made with a precipitate of $CaCO_3$ that appeared as spherical grains under the microscope. It was agitated for twelve hours with sea water and the alkaline reserve was 0.0022 at pH 7.95. No further experiments were made to determine whether equilibrium had been approximated.

These experiments clearly show that the surface water of the sea is a supersaturated solution of $CaCO_3$ and it is only necessary to introduce calcite crystals in order to cause considerable precipitation of this substance. Precipitation goes on in the bodies of organisms in the surface waters of all seas. The precipitation observed by Vaughan at Tortugas is very finely divided, but whether it was formed in the bodies of minute organisms, which subsequently died, has not been determined. Such particles might slowly grow, since the agitation of them with sea water was found to take a trace of $CaCO_3$ out of the water. Small crystals have been seen in the bodies of Protista, and whether they are $CaCO_3$ or not, they might form nuclei for the precipitation of $CaCO_3$ if released into the sea water.

In some experiments in liter flasks of resistance glass, filled into the neck (and hence admitting of but slight loss of CO_2) the pH and alkaline reserve was determined immediately before and after agitation with calcite, and the loss of CO_2 from the sea water calculated from the pH and from the loss of $CaCO_3$ (alkaline reserve).

SEA WATER	pH	TOTAL CO_2	ALKALINE RESERVE	CALCULATED LOSS OF CO_2	
				From pH	From alk. res.
Before agitation with calcite	8.2	44.5	0.0025	6	6.72
After agitation with calcite	7.67	38.5	0.0019		
Before agitation with calcite	8.25	43.8	0.00250	6.8	7.27
After agitation with calcite	7.72	37.0	0.00185		

In the above table, the agreement is very striking in view of the probable error in determination of pH and the liability to loss of CO_2 from the water surface in the neck of the flask, agitated by the rotary stirrer.

If the pH of sea water should be maintained (by the action of plants) at 8.2 while it was agitated with calcite crystals, the loss of $CaCO_3$ would be about 0.001 N, or 0.0005 M, or 0.1 gram per liter. This would cause a deposit of 10 kgm. per square meter of bottom in water 100 meters deep. This would cause a lowering of the calcium content of Tortugas sea water by about 4.5%.

[1] *The Depths of the Ocean*, London, 1912, p. 178.

[2] These PROCEEDINGS, **2**, 1916, (28).

[3] *Washington, Carnegie Inst.*, Pub. No. 182, 1914, (Tortugas Lab., vol. 5).

[4] *Depths of the Ocean*, p. 368.

[5] *Voyage of H. M. S. Challenger, Physics and Chemistry*, vol. 1.

[6] McClendon, *J. Biol. Chem.*, Aug., 1917.

[7] It would be interesting to know whether corals and calcareous algae deposit as much $CaCO_3$ in the dark as in the light. Corals from deep water are smaller, more fragile, and deposit less $CaCO_3$ than those of shallow water, but the same is true of animals without symbiotic algae. The deposition is, however, related to the pH, since Palitzsch has shown that the pH decreases with depth.

[8] *Amer. J. Sci., New Haven,* **41,** 1916, (473).

AN OENOTHERA-LIKE CASE IN DROSOPHILA

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Communicated by T. H. Morgan, September 14, 1917

Although the large bulk of the Drosophila work has been remarkably self-consistent, and amenable to orderly and definite rules of factor transmission, yet from the outset the ideal scheme has been confronted with two unconformable cases. These are the cases of beaded wings and of truncate wings, both of which seemed to belie the idea of clear cut segregating gens. In the case of beaded wings, which will be briefly reported here,[1] many generations of selection were carried out by Morgan with the purpose of obtaining a pure breeding stock, yet for several years it was impossible to attain this object. The character showed all the peculiarities which would be expected as a result of factor fluctuation and miscibility. It was increased in intensity as a result of selection yet its essential variability remained, and the latter was proved by crosses to be genetic, for Morgan found that reversed selection produced a marked and immediate retrogression in the proportion of beadeds thrown. Finally, however, he did obtain a race which threw no normals, but the reason for this change in behavior seemed now just as difficult to discover as the cause of the previous variability. On crossing, various apparently irregular results and aberrant ratios followed.

The work of Dexter, which showed that environmental conditions and factors in both the second and third chromosomes are all concerned in the development of this character, provided valuable information for the present investigation. Starting from Dexter's finding that there was a chief factor for beaded in chromosome III[2] an attempt was made by the writer to find the precise location of this gen. It proved to lie at the extreme right hand end of the known factors in the third chromosome, being two and a half units beyond the factor for rough eyes, which is otherwise the furthest factor to the right. It was found also that the apparently pure beaded stock is not homozygous for beaded, but in reality contains two very different kinds of third chromosomes. It was by investigating this phenomenon, with the aid of the data secured in

the linkage study, and by dissecting, by means of crossing over, each of these two kinds of chromosomes separately, that a complete explanation of the beaded case was finally obtained. It would be impossible here to give even an outline of the detailed steps of analysis, but the following are the concrete results and theoretical conclusions which have been arrived at.

1. The difficulty which was experienced in getting pure stock was due to the fact that the chief factor for beaded—B_d'—is lethal, killing all flies homozygous for it. Heterozygous beadeds are not killed but usually show the beaded character. Thus B_d', like yellow in mice, is dominant for its visible effect and recessive for a lethal effect. The dominance of B_d' is variable, however.

2. The reason that a race which bred true to beaded was finally secured is not because a condition of homozygosis was at last established, but, on the contrary, because of the establishment of a state of enforced heterozygosis, wherein not only the homozygous beadeds but also the homozygous normal winged flies were prevented from hatching. This was due to the appearance, by mutation, of another lethal factor, $l_{III\,1}$, in that third chromosome of the impure stock which contained the normal allelomorph of B_d'. This lethal, like B_d', kills all flies which are homozygous for it, although, unlike B_d', it has no visible effect when it is heterozygous. Since, in the flies of this heterozygous race, it arose in the chromosome containing the factor for normal wings, no homozygous normals will be able to appear except those in which $l_{III\,1}$ has been separated by crossing over from the normal allelomorph of B_d'. The usual amount of crossing over between these loci of $l_{III\,1}$ and of B_d' is 10%, as $l_{III\,1}$ was found to be 10 units to the left of B_d'. On this basis it is to be expected that the selected beaded stock would throw 90% beaded flies and 10% normal winged crossovers.

3. In this selected stock, however, not even 10% of the flies have normal wings, because of the existence of another mutant factor, C′, which almost entirely prevents crossing over in the region of the chromosome in which $l_{III\,1}$ and B_d' lie. $l_{III\,1}$. on this account, always remains with the normal allelomorph of B_d' and all the homozygous normal winged, as well as the homozygous beaded flies are thus prevented from appearing. It should be noted that the factor C′, too, always remains heterozygous, for it is contained only in the chromosome having $l_{III\,1}$ and the normal allelomorph of B_d', not in the chromosome with B_d' itself. This state of heterozygosis for C′ is also a necessary condition for the mechanism whereby only beaded flies are produced, since C′ produces its inhibition of crossing over only when it itself is heterozygous. Aside

from its influence on crossing over, no other effect of this factor has been discoverable. The locus of C′ is within 10 units of that of sooty body color, being to the left of the locus of $l_{III\,1}$, and to the right of the locus of kidney eyes. C′ probably existed in the chromosome of beaded stock now containing it before $l_{III\,1}$ arose in that chromosome by mutation.

4. This remarkable genetic situation, wherein both types of homozygotes are prevented from appearing by the action of lethal factors lying in opposite chromosomes, may be termed a condition of 'balanced lethal factors.' Surprising as it may seem, it appears that such a condition is no mere 'freak of nature,' and that it is apt to arise wherever dominant mutant factors exist which either have some natural survival value, or have, like beaded, been artificially selected for. This conclusion is based upon the following experimental results and theoretical considerations.

(a) In an attack upon this question, an investigation was undertaken by the writer to determine how generally dominant mutant factors of Drosophila are lethal when homozygous. There were nine dominant mutants known (excluding the intensifiers of beaded and truncate), and they were distributed equally among the three large chromosomes. The viability of the three sex-linked dominants was, of course, already known; the remaining six dominants were tested. In all, it was found that three of the nine are not lethal, one (in chromosome III) is semi-lethal, and five (one in chromosome I, two in II and two in III) are completely lethal when homozygous. Thus it is a phenomenon of common occurrence in Drosophila for dominant mutant factors to be lethal when homozygous.

(b) It would be very far fetched to assume that the natures of the characters produced by dominant factors differ as a class from those produced by recessives. We must therefore believe that lethals are very frequent among recessive factors also. It should be noted, however, that in the case of recessive factors a lethal action prevents or greatly hinders their discovery, whereas with factors dominant in respect to some visible character, a lethal effect, when homozygous, does not interfere with their being detected; for this reason it is quite in accord with expectation that a much smaller proportion of lethals has actually been found among the recessives than among the dominant mutants. The evidence for the frequency of origin of recessive lethals is not entirely by analogy, however, for in the case of sex-linked factors the discovery of lethals is easier, owing to their effect upon the sex ratio, and here a considerable number of lethals has in fact been found, by

various observers. Following up, now, our original inference regarding the high frequency of lethals among recessive mutants, it should further be pointed out that since recessive mutants as a class are much more numerous than dominant mutants, recessive lethals also should arise much oftener than dominant ones.

(c) It now remains to join these two results together in one conclusion. Suppose that a race already contains a dominant mutant factor which is favored by selection. Firstly, as shown in (a), this is likely to be lethal when homozygous. Then, as shown in (b), it is likely that a recessive lethal will some time, in some line of individuals, arise by mutation in the opposite chromosome. Since the first factor is being selected for, and the presence of this second lethal will cause the production of a smaller proportion of individuals not showing the desired dominant character, the line containing the second lethal in addition will tend to be selected. Thus a condition of balanced lethal factors will automatically become established, just as it did in the case of beaded wings. Factors which prevent crossing over, or balancing lethals which cross over less frequently with the desired dominant, will of course also be favored by selection, provided they occur, for the less crossing over there exists between the two balanced factors, the more perfect is the balance and the greater is the proportion of individuals showing the advantageous character. The frequency of factors like C′ is however unknown, although they have been discovered in nearly a dozen stocks of Drosophila. In the case of the beaded stock, the presence of C′ seems to be just a happy coincidence, as it probably existed there before $l_{III\,1}$ arose. But, however that may be, it is evident that the present case is but a special instance of a general class of cases of balanced lethal factors that will probably confront the geneticist in increasing numbers.[8]

5. What will be the distinguishing characteristics of races in this condition? (a) In the first place, crosses of these varieties to other races will result in the production of hybrids of two types, according to which of the chromosomes of the balanced pair they receive. Thus, flies of beaded stock crossed to normal give 50% beaded and 50% normal in F_1. In cases where one of the lethal containing chromosomes is not dominant to the chromosomes of the foreign race in any 'visible' factor, one of these hybrids (in our case the normal) will appear to breed true, while the other will show segregation in subsequent generations. By introducing other mutant factors besides into the balanced chromosomes of beaded stock, these results were made more striking and made to apply to a number of different characters at once.

(b) Still more unusual results can be and were obtained by crossing a

balanced race with another which also contained lethal factors (either the same or different in nature and grouping). In such cases not only twin but also multiple hybrids may be produced, that may or may not be constant. The results, however, always came out according to the prediction based on knowledge of the factorial composition of the flies. One such result which was especially noteworthy was a prearranged cross in which a dominant character present in one of the parent flies was caused to disappear completely, being absent from all the progeny of the cross and all subsequent generations.

(c) A lowered productivity is of course noticeable in balanced races, owing to the action of the lethal factors.

(d) In stocks in which other recessive mutant factors had been introduced into one or other of the chromosomes containing the lethals, these factors, of course, usually failed to manifest themselves, owing to the enforced heterozygosis. Occasionally, however, one or more crossed over from the lethal factor with which it was bound, and so was enabled to become homozygous. As crossing over occurs with predictable frequencies, these individuals showing characters abnormal to the stock were thrown continually in a definite, very small per cent of cases. This caused the stock to appear 'eversporting.' Crossing over between the lethal factors themselves also occurred very rarely, giving rise to individuals no longer exhibiting the unusual genetic behavior due to balanced lethals.

6. The striking parallel between the above behavior and that exhibited in Oenothera make it practically certain that this, too, is a complicated case of balanced lethal factors, and that some (if not most) of the so-called mutations in O. lamarckiana are but the emergence into a state of homozygosis, through crossing over, of recessive factors constantly present in the heterozygous stock. Proof of the spuriousness of some of the mutations in Oenothera is, however, not an argument against the validity of the modern mutation theory; the fact of real mutation has been amply demonstrated in Drosophila as well as elsewhere, and it should be emphasized that these mutations can here be distinguished with certainty from the superficially similar phenomena that are observable in the beaded stock, because the genetic constitution of the flies can be analyzed in detail.

In Oenothera a form of the balanced lethal explanation was suggested by de Vries only to account for his double reciprocal crosses, but it is evident from the analogy of the beaded case that it probably lies at the root of nearly all the unusual genetic phenomena of the genus. The two cases differ in detail, however, in that one or more of the lethals in

Oenothera produce their effect upon the gametes, rather than upon the zygotes.

Double throwing stocks (Matthiola) present another case of balanced factors. This too differs in detail from the beaded case, for one of the factors acts very early, producing its lethal effect directly upon the gametes (pollen), as in Oenothera, whereas the other, although it affects the zygotes, does not act as a lethal to their soma, but merely causes their sterility. It is this factor which causes the double flower.

7. The condition of balanced lethal factors must slowly lead to a partial degeneration of the chromosomes containing these lethals. For any new lethal recessive factors that arise in these chromosomes will never have the opportunity of becoming homozygous and producing their harmful effect, and so there will be no cause for natural selection to eliminate them. Lethal recessive mutant factors of all sorts (including 'deficiencies') will therefore gradually accumulate in the chromosomes of the affected pair. Moreover, although $l_{III\ 1}$ is the first lethal recessive which has been found in an autosome of Drosophila, theoretical considerations and the experiments above reported lead to the conclusion that in the course of time the number of such mutations will have been not inconsiderable, as they probably form a large proportion of all the mutations that occur. For similar reasons, chromosomes of stocks which are continually outcrossed, and the Y-chromosome in all species containing it, should undergo degeneration, because these chromosomes, too, are always protected, by heterozygosis, from the action of natural selection.

8. Not only are mutants of an undesirable nature not eliminated from balanced chromosomes by natural selection, but recessive mutant factors of a desirable type also are prevented from becoming homozygous and producing their effects, and so they cannot be selected for. On account of this latter circumstance, evolution is hindered in these varieties. As each chromosome of the balanced pair degenerates, however, it must gradually lose the dominant normal factors that prevented recessive mutant allelomorphs in the opposite chromosome from manifesting themselves. The balanced races might, moreover, eventually return completely to a condition of normal genetic behavior, owing to the occurrence of doubling or non-disjunction, which might make two normal pairs of chromosomes out of one balanced pair.

9. The inheritance of beaded is complicated not only by balanced lethal factors, but also by a modifiability of the character under the influence of environmental conditions, and by multiple factors. A number of the well known mutant factors for totally different characters

than beaded have been found to intensify or inhibit the development of the latter, and there is besides, as Dexter has shown, a factor in the second chromosome of the selected beaded stock itself which has no visible effect other than to increase the degree of beading. This must have arisen by mutation and have been perpetuated in the process of selection. Crosses made in the course of the present work have shown that this intensifier, I_{Bd}', is partially dominant, but is not a lethal, and, in contrast to the other factors involved in beaded stock, that it exists here in homozygous condition. At least one mutant factor has also been found, by the writer, which can produce a character similar to beaded even when the factor B_d' itself is not present; this mutant is not ordinarily present in beaded stock, however. All the facts of the present section may be summed up in the single generalization that beaded is a character depending upon a developmental reaction that is readily modifiable.

The complete formula of the selected beaded race, representing all the pairs of factors wherein it differs from the wild type, may now be given:

$$\frac{I_{B_d}'}{I_{B_d}'} \; \frac{c'l_{III\,1}\,B_d'}{C'l_{III\,1}\,b_d'}$$

10. The beaded case illustrates to great advantage the danger of confusing characters with gens and of drawing radical conclusions concerning the behavior of gens on the basis of uncritical experiments. The work of the first four years upon the inheritance of beaded wings gave evidence which would to many have appeared most elaborate and convincing, that the hereditary material in this case was fluctuating and miscible, consisting of vague and plastic "tendencies," rather than definite physical particles. Precise analysis, of a sort comparable to that of chemistry, has, however, been possible here, and it has demonstrated that a very different set of processes from those that might have been imagined is responsible for the peculiar results—processes which in their essence conform strictly to the genotype conception.

It will accordingly be necessary in other cases also not to accept evidence apparently in favor of factor inconstancy until factorial analyses of a similarly rigorous character have proved such an interpretation to be correct. A similar criticism applies to the acceptance of results that seem to be non-Mendelian; and also to the incautious estimation of apparent mutations at their face value. 'Non-Mendelian' results of all kinds and also 'mutations' may be prearranged and brought about at will with the beaded flies, but here analysis has made the

Mendelian machinery at work evident. Unwelcome as these conclusions may sound to obscurantists and to those in general who have an antipathy for exact modes of procedure, the necessity for such refined methods here should be obvious. In particular, it will be desirable to examine more intensively those cases which show the characteristics described in Section 5 for balanced lethal factors.

[1] The full account of this case will appear subsequently, and also an account, by Altenburg and Muller, of the truncate case.

[2] Sturtevant also had performed certain linkage experiments in which beaded was present; these showed that it was well on the other side of sooty from pink.

[3] Since the above has gone to press, a case in *Campanula* which may be due to balanced lethals has been reported by Miss Pellew in the *Journal of Genetics*.

IS DEATH FROM HIGH TEMPERATURE DUE TO THE ACCUMULATION OF ACID IN THE TISSUES?

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Communicated September 12, 1917

I find that there is a converse relation between the rate of oxygen consumption in reef corals and their ability to resist high temperature, those corals which are most readily killed by heat having the highest metabolism (rate of oxygen consumption).

NAME OF CORAL	CONSTANT TEMPERATURE EXPOSURE TO WHICH CAUSES DEATH IN ONE HOUR	RELATIVE RATE OF OXYGEN CONSUMPTION PER GRAM OF LIVING SUBSTANCE IN EACH CORAL
	°C.	
Acropora muricata	34.7	18.7
Orbicella annularis	35.60	6.1
Maeandra areolata	36.80	5.5
Favia fragum	37.05	3.8
Siderastrea radians	38.20	1.0*

* At 28.5°C. 1 gram of living substance of *Siderastrea radians* consumes 0.0256 cc. of oxygen per hour. The oxygen being measured at 0°C. and 760 mm. pressure.

Also, if sea water be super-saturated with carbon dioxide gas, the toxic effect is in the same sequence as that of high temperature. That is to say, those corals which are readily killed by heat are also correspondingly easily killed by H_2CO_3.

This toxic effect of carbon dioxide is not due to its replacing some of the oxygen of the sea water, for I find that corals are remarkably insensitive to a reduction in the oxygen supply, all species except *Acropora* living more than eleven hours in sea water under an air pump which reduced the oxygen to less than 5% of that of normal sea water; and even *Acropora* can withstand six hours of this treatment.

We know, indeed, from the studies of M. Henze that sea anemones use less oxygen the less its concentration in the sea water, and in 1917, J. F. McClendon found that the medusa *Cassiopea* can survive without apparent injury for more than seven hours in the absence of oxygen, and during this time does not give out CO_2. Thus these cœlenterates can temporarily suspend their metabolism for a protracted period if oxygen be absent.

Winterstein's theory that death from heat is due to asphyxiation appears to be refuted by these experiments.

E. N. Harvey, 1911, found that if the sea water be heated the rate of nerve conduction in *Cassiopea* augments in a right line ratio up to a certain point and then rapidly declines just before death ensues.

In 1917, I found that this temperature curve, up to its maximum point, has no time factor. That is to say, the rate at 35°C. is the same whether the medusa be placed at once in 35°C. or warmed slowly for several hours until it arrives at this temperature. Moreover, the normal rate for 29° is regained almost immediately when the medusa is replaced in this normal temperature.

When the rate is declining, due to injuriously high temperature, however, I find that a time factor is involved, the decline becoming more pronounced as the heat is continued. Also, if after this the medusa is replaced in sea water of 29°C., its former rate is much reduced and may never be recovered, although if exposure to the heat was not too long or the heat not too excessive, a slow recovery is usually observed so that after a few hours the rate may again become normal.

It will be recalled that Harvey, 1911, advanced the theory that some enzyme might be destroyed by the excessive heat, and being essential to nerve conduction, its loss caused the rate to decline.

Our experiments however make it seem more probable that some toxic acid substance, possibly lactic acid or H_2CO_3, is formed under the influence of excessive heat. It is easy to see how an acid of this sort might be eliminated and the rate gradually restored when the animal is replaced in normal sea water, whereas if an enzyme were destroyed it might not so readily be replaced.

In any event one or the other of the above mentioned hypotheses seems more in accord with the facts than does Winterstein's asphyxiation theory or the theory that death from heat is due to coagulation of proteid substances.

Death occurs at too low a temperature for coagulation in most if not all proteids; and when killed, the animals are fully relaxed as shown by Harvey. Moreover, coagulated proteins could not readily be eliminated when the animal was restored to water at normal temperature.

NATIONAL RESEARCH COUNCIL

MEETINGS OF THE EXECUTIVE COMMITTEE

The twenty-seventh meeting convened in the offices of the Council in the Munsey Building, Washington, D. C., on Wednesday, August 15, 1917, and was called to order at 6.00 p.m. by the Vice-Chairman, Mr. Millikan.

Messrs. Bogert, Millikan, Stratton, Vaughan, and, by invitation, Durand and Mendenhall were present.

Mr. Millikan presented a report from the Chairman of the Committee on Noxious Gases, recommending an additional appropriation of $662,900, which it is estimated will be required for gas warfare investigations for the fiscal year ending June 30, 1918. After discussion, these estimates for expenditures, which are additional to the original allotment of $175,000 recommended for such investigations at the meeting of the Executive Committee of the Council on May 28, were approved and the Vice-Chairman and Executive Officer of the Council were requested to transmit them with such approval to the Secretary of War and to the Secretary of the Navy.

The twenty-eighth meeting convened in the offices of the Council in the Munsey Building, Washington, D. C., on Wednesday, August 22, 1917, and was called to order at 9.10 a.m. by the Vice-Chairman, Mr. Millikan.

Messrs. Bogert, Carty, Chittenden, Millikan, Noyes, Pupin, Stratton, Vaughan, and, by invitation, Durand were present.

The minutes of the meetings of July 31 and August 15 were read and approved..

Upon motion the Committee approved the action of the Chairman of the Council in appointing a special committee to meet once a week to consider questions relating to the conduct of affairs of the Council in intervals between meetings of the Executive Committee; the membership of this committee to consist of Mr. Millikan, chairman, and Messrs. Bogert, Durand, Mendenhall, Stratton and Vaughan.

It was also voted that the minutes of the meetings of this special committee be duplicated and distributed regularly to members of the Executive Committee.

Explanation was made of the financial business of the Council and statements were submitted of the balances available in its three bank accounts.

Mr. Pupin invited discussion relative to relations which may exist between the National Research Council and the Engineering Foundation after the termination of the present agreement on September 20, 1917, by which the income of the endowment of the Engineering Foundation is made available for purposes of the Council. After lengthy discussion the following statement expressive of the appreciation and opinion of the Executive Committee in this matter was approved and ordered for transmission to the Engineering Foundation:

As the year for which the cooperative arrangement was made between the Engineering Foundation and the National Research Council will soon expire, the Research Council wishes again to record its high appreciation of the liberal financial and personal assistance which the Foundation and its members have afforded in the organization and support of the work of the Council in this first year of its existence. It seems exceedingly desirable to maintain the close relation between engineers and research workers which the Research Council has for the first time succeeded in establishing. Accordingly the Council wishes to express the hope that it may be practicable to continue some effective form of cooperation between the two bodies and it would be glad to appoint representatives to consider this matter with men designated by the Foundation.

Mr. Durand submitted a report from the special committee appointed at the meeting of the Executive Committee of July 31st to confer with representatives of the U. S. Patent Office with regard to the possible cooperation of the Council in questions of organization and conduct of affairs of this Office.

After extended discussion relative to the advisability of undertaking such work in the present national emergency it was voted, upon motion of Mr. Noyes, that the special committee in question be requested to confer with the new Commissioner of Patents, when appointed, to emphasize the importance of the proposed work and to state the willingness of the National Research Council to cooperate with the Patent Office in such effective ways as may be available; but at the same time to call attention to the large scope of the project and the inability of the National Research Council to cope with the financial considerations involved.

Upon motion it was decided to issue a new descriptive pamphlet relative to the work and organization of the Council, the Executive Officer of the Council being requested to appoint a special committee to consider and expedite this work.

Mr. Vaughan, as Chairman of the Committee on Medicine and Hygiene, presented the following resolution adopted by the Sub-Committee on Psychiatry of this Committee:

The nervous diseases grouped under the term 'shell shock' constitute a new and very important medical problem of war. They are responsible for fifteen per cent of discharges for disability from the British Army. Soldiers with these disorders present difficult problems in diagnosis and treatment and constitute a large proportion of those who after long continued hospital treatment become pensioners. These disorders in troops at the front are seriously prejudicial to discipline, morale and military efficiency. In spite of the extraordinary prevalence, very little is known about their physiological or psychological basis. The research which has been carried on since the war began has been seriously retarded by the stress of war conditions and little has been accomplished in any of the European armies.

As the American Army is certain to suffer no less severely from these disorders than the armies of our Allies, it is essential to secure without delay more information than we now possess as to their causes, nature and best methods of treatment.

With this object in view, it is resolved that the National Research Council be requested to permit this Sub-Committee to organize a Commission and to establish an experiment station in France for the study of 'shell shock' and to secure funds to meet the expenses of the work of this Commission.

Upon motion this request of the Sub-Committee on Psychiatry was approved, with the understanding that funds to meet expenses of the proposed work be secured from other sources.

Mr. Durand presented a request for an allotment of $250 for incidental expenses in connection with experiments being undertaken by Messrs A. M. Hunt and Cary T. Hutchinson on the production of artificial spray in which to envelop ships for purposes of protection. He pointed out that this investigation is a very promising one and has the approval and cooperation of the Ship Protection Committee of the Emergency Fleet Corporation. Upon motion, an allotment of $250 was made for this purpose, to be expended from the funds of the Council which have been deposited with the Engineering Foundation.

Mr. Bogert, Chairman of the Chemistry Committee, submitted a general statement relative to the work of this Committee, and upon his recommendation the following additional appointments were made to membership in Sub-Committees:

Sub-Committee on Chemistry of Fuels: W. A. Hamor, W. E. Vawter, and E. R. Weidlein, all of the Mellon Institute of Industrial Research, Pittsburgh, Pa.

Sub-Committee on Chemistry of Leather and Tanning: F. P. Veitch, U. S. Department of Agriculture.

Sub-Committee on Chemistry of Dye-stuffs: Marx Hirsch, 50 East 41st Street, New York, N. Y.

Sub-Committee on Organic Chemistry: Roger Adams, University of Illinois, Urbana, Illinois.

After discussion, upon motion of Mr. Noyes, it was decided to discontinue the Committee on Nitrate Supply of the Council and to organize a new Committee on Nitrogen Fixation as a Sub-Committee of the Chemistry Committee.

A report was read from the Chairman of the Psychology Committee descriptive of the organization and progress of psychological work which has been obtained in relation to military activities and, upon motion of the Chairman of the Committee, the following Sub-Committees were appointed:

Sub-Committee on Methods for the Psychological Examining of Recruits: Chairman, R. M. Yerkes; W. V. Bingham, H. H. Godard, T. H. Haines, L. M. Terman, T. L. Wells, G. M. Whipple.

Sub-Committee on Tasks of Special Skill: Chairman, E. L. Thorndike; J. C. Chapman, T. L. Kelley, W. D. Scott.

Sub-Committee on Problems of Aviation, including the examination of aviation recruits: Chairman, H. E. Burtt; W. R. Miles, L. T. Troland.

Sub-Committee on Incapacity, Reeducation and Vocational Training: Chairman, S. I. Franz; K. S. Lashley, J. B. Watson.

Sub-Committee on Visual Problems: Chairman, Raymond Dodge; R. P. Angier, H. A. Carr, L. R. Geissler, S. P. Hayes, G. M. Stratton, L. T. Troland.

Communications received from the Chairman of the Geology and Paleontology Committee were presented and, after consideration, the following actions were taken:

EDWARD B. MATHEWS, of Johns Hopkins University, was appointed as Chairman of the Sub-Committee on Roads and Road Metals, to replace Dr. William B. Clark, deceased.

BASHFORD DEAN, Curator at the Metropolitan Museum of Art, was appointed a member of the Geology and Paleontology Committee.

A Sub-Committee was appointed, to be known as the *Pacific Coast Sub-Committee on Geology*, with the following membership: JOHN C. MERRIAM, Chairman; D. M. FOLSOM, J. C. JONES, HENRY LANDES, A. C. LAWSON, GEORGE D. LOUDERBACK, WARREN D. SMITH, BAILEY WILLIS.

The Vice-Chairman reported that W. P. Wilson, Director of the Commercial Museum, Philadelphia, Pa., has accepted appointment as a member of the Botanical Raw Products Committee of the Council, but that John C. Teeple has found it impossible to become a member of this Committee.

A statement was also read of the proposed work of this Committee, as well as a proposed budget of the annual expenses for the organization of such work.

Mr. Durand submitted a statement relative to the work of the Engineering Committee of the Council and referred particularly to consideration which has been given to the question of steel body armor and the probability that this work may be extended and organized.

Mr. Millikan submitted a report concerning the question of the detail of drafted men for special technical services in the army. He mentioned requests which have been received from France for groups of scientists to work in foreign laboratories and to undertake certain researches under the auspices of the U. S. Signal Corps. He reported that the following additional recommendations had been submitted to the Chief Signal Officer relative to the organization of work in meteorology and sound-ranging:

1. That Major E. H. Bowie be appointed American Forecaster in general meteorology in France to work in close cooperation with the French Military Meteorological Service, with headquarters which will probably be at Paris.

2. That Dr. William H. Blair be placed in full charge of the Aerological Service of the Army both in the United States and at the Front. It is understood that this work involves surface observations as well as upper air observations.

3. That after the preliminary organization of the aerological work in this country, as outlined in the subjoined recommendations, Dr. Blair proceed at once to France to study the conditions and organize the American Aerological Service there.

4. That Major Bowie proceed at once to France to begin work in connection with the French Forecasting Service.

5. That Mr. R. Hanson Weightman be commissioned as a First Lieutenant and assigned as an assistant to Major Bowie in forecasting work in France.

6. That Prof. Theodore Lyman be commissioned as a Captain and Mr. Norman R. French be commissioned as a Lieutenant in the Signal Corps, Sound Ranging Service, and that Major Trowbridge accompanied by the above officers proceed to France at the earliest possible moment and report to Colonel Russell.

7. That Dr. Herbert B. Williams be commissioned as a Captain in the Signal Corps, Sound Ranging Service, and that he be detailed to Princeton in charge of the construction work on sound ranging instruments during Major Trowbridge's absence.

8. That Mechanician William Duryea be taken into the employ of the Signal Corps at a salary of $1500 per annum and that Mr. Sanford P. Wicks now enlisted in the 3rd Ambulance Company, National Guards, State of New York, be transferred to the Signal Corps and assigned to mechanical work under Mr. Duryea and that Major Trowbridge be authorized to hire one other man as a mechanic at current wages.

The twenty-ninth meeting convened in the offices of the Council in the Munsey Building, Washington, D. C., on Wednesday, September 5, 1917, and was called to order at 4.30 p.m. by the Vice-Chairman, Mr. Millikan.

Messrs. Bogert, Carty, Dunn, Millikan, Stratton, Vaughan, and, by invitation, Colpitts, Durand, Manning, Mendenhall and Yerkes were present.

Mr. Millikan emphasized the importance of maintaining the Council as a centralizing agency for the activities of its committees and for the scientific interests which may be entrusted to it by various departments of the government. In this connection discussion took place relative to the detail of drafted men for scientific services in the army. Further discussion led to a consensus of opinion, which was recorded in the form of a motion, that, as a matter of policy in submitting requests for the detail of such men, it would be advisable to secure their services as privates with the understanding that additional request would subsequently be made for the appointment of a due proportion of them, as occasion demands, as commissioned officers.

Discussion took place concerning a proposed organization of American engineers urgently requested by French officials for services at the Front. The Executive Officer of the Council was requested to confer with the Chief Engineer, U. S. A., and offer the services of the Council in this connection.

Mr. Yerkes, Chairman of the psychology Committee submitted a report relative to the current development of the work of this Committee in connection with military needs.

After explanation by the Executive Officer the name of the Committee on Noxious Gases was changed to read "Committee on Gases used in Warfare." He also reported that upon order of the Surgeon General, U. S. A., Major Williamson, a member of this Committee, has been detailed for other duty and that Major Bradley Dewey of the U. S. Sanitary Corps has been appointed in his place as a member of the Committee on Gases used in Warfare. The Chairman of this Committee was furthermore authorized to appoint a representative of the Engineer Corps of the Army and to select a successor to Dr. Gatewood as additional members of the Committee.

Upon motion of the Chairman of the Chemistry Committee the following were appointed as members of the

Sub-Committee on the Chemistry of Fertilizers: F. G. Cottrell, Bureau of Mines, Washington, D. C.; Hoyt Gale, U. S. Geological Survey, Washington, D. C.; Bailey E. Brown, Bureau of Plant Industry, U. S. Department of Agriculture, Washington, D. C.; Jacob G. Lipman, New Jersey Experiment Station, New Brunswick, New Jersey.

Cary T. Hutchinson, *Secretary.*

VOLUME 3 NOVEMBER, 1917 NUMBER 11

PROCEEDINGS

OF THE

NATIONAL ACADEMY OF SCIENCES

OF THE

UNITED STATES OF AMERICA

Publication Office: Williams & Wilkins Company, Baltimore
Editorial Office: Massachusetts Institute of Technology, Cambridge
Home Office of the Academy: Washington, D. C.

Entered as second-class matter at the Post Office at Baltimore, Md., under the Act of August 24, 1912

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PROCEEDINGS
OF THE
NATIONAL ACADEMY OF SCIENCES

Volume 3 NOVEMBER 15, 1917 Number 11

A NECESSARY AND SUFFICIENT CONDITION FOR THE EXISTENCE OF A STIELTJES INTEGRAL

By Gilbert Ames Bliss

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Communicated September 12, 1917

The purpose of this note is to suggest a proof of the following theorem: *If $u(x)$ is of limited variation and $f(x)$ bounded on the interval $a \leqq x \leqq b$, then a necessary and sufficient condition for the existence of the Stieltjes integral*

$$\int_a^b f du \tag{1}$$

is that the total variation of $u(x)$ on the set D of discontinuities of $f(x)$ be zero. If $U(x)$ is the total variation of $u(x)$ on the interval ax, and α an interval with end points x_1, x_2, then by definition

$$U(\alpha) = U(x_2) - U(x_1)$$

and the total variation of $u(x)$ on D is defined to be the greatest lower bound of the sums

$$\sum_{k=1}^{\infty} U(\alpha_k)$$

for denumerable sequences of intervals $\{\alpha_k\}$ containing the points of D as interior points of the set which the intervals define.

Let the interval ab be subdivided by points x_i $(i = 0, 1, \ldots, n)$ with $x_0 = a$, $x_n = b$, $0 < x_i - x_{i-1} < \delta$ $(i = 1, 2, \ldots, n)$, and let S denote the sum

$$S = \sum_{i=1}^{n} f(X_i)\,[u(x_i) - u(x_{i-1})] = \sum_{i=1}^{n} f(X_i)\,\Delta_i u,$$

where the values X_i are arbitrarily selected in the intervals $x_{i-1}x_i$.

Then a necessary condition that the sums S have a limit as δ approaches zero is that

$$\lim_{\delta=0} \sum_{i=1}^{n} O_i \,|\, \Delta_i u \,| = 0, \tag{2}$$

where O_i is the oscillation of $f(x)$ on the interval $x_{i-1}x_i$. For it is clear that the values X_i can always be selected for two sums S, S' on the same partition so that the difference $S - S'$ is as near as is desired to the sum in (2). Hence if for every norm δ a partition can be found with

$$\sum_{i=1}^{n} O_i |\, \Delta_i u| > \eta > 0,$$

then for every δ two sums S, S' can be found with $|\, S - S' \,| > \eta$, and this would contradict the existence of the limit (1).

Let D_ϵ denote the closed set of points x where the oscillation $O(x)$ of f satisfies the inequality $O(x) \geqq \epsilon$, and let the intervals of S which contain points of D_ϵ as interior points be designated by the subscript j. Then it is also necessary that

$$\lim_{\delta=0} \sum_{j} |\, \Delta_j u \,| = 0. \tag{3}$$

For in every such interval $O_j \geqq \epsilon$, and if for every δ a sum as in (3) exists exceeding η, a sum as in (2) can be found exceeding $\epsilon\eta$.

Consider now a function $u(x)$ which is continuous as well as of limited variation, and suppose that the integral (1) exists. Then if δ is sufficiently small the j-intervals of S will be such that

$$\sum_{j} |\, \Delta_j u \,| < \eta.$$

Since $u(x)$ is now continuous the sum S can be altered by the introduction of new division points so that all the points of D are interior to the set of points defined by the sum of the j-intervals and the last inequality still true. Furthermore for the altered S it will follow that

$$\sum_{j} \Delta_j U < 2\eta$$

provided that δ is chosen so small that the total variation $U(b) - U(a)$, and $\sum_{i=1}^{n} |\, \Delta_i u \,|$, and hence also the corresponding quantities for every sum of partial intervals of ab, differ by less than η. Such a choice is always possible when $u(x)$ is continuous (Vallée Poussin, *Cours d'Analyse Infinitésimale*, vol. 1, 3d ed., p. 73). It follows then

that the total variation of u on the set D_ϵ must be zero, and hence also the total variation of u on the totality D of discontinuities of $f(x)$, since D is the sum of the sets $D_{1/n}$ ($n = 1, 2, \ldots$).

To prove the sufficiency of the condition of the theorem for a function $u(x)$ which is continuous, consider the two monotonically increasing continuous functions $P(x)$, $N(x)$ satisfying the relations (Op. cit., p. 72)

$$u(x) - u(a) = P(x) - N(x), \quad U(x) = P(x) + N(x). \qquad (4)$$

If the variation of $U(x)$ is zero on the set D then the same is clearly true of $P(x)$ and $N(x)$. Hence it is only necessary to prove the sufficiency for a monotonically increasing function $P(x)$. If D can be enclosed in the interior of a denumerably infinite system of intervals on which the sum of the variations of $P(x)$ is less than η the same is clearly true of its sub-set D_ϵ, and the latter is furthermore entirely interior to a finite number of the intervals since it is closed. (This follows from the Heine-Borel theorem. See, for example, Op. cit., p. 60, Lemme I. The proof is quite similar for a closed set or an interval.) The portion of ab remaining can be subdivided into intervals so small after D_ϵ is extracted that on each of them the oscillation of $f(x)$ is less than 2ϵ since on each such portion $O(x) < \epsilon$ (Op. cit., p. 254, §242). For every η and ϵ a partition of ab is defined in this way for which the inequality

$$\sum_{i=1}^{n} O_i \Delta_i P \leqq 2M\eta + 2\epsilon[P(b) - P(a)]$$

is true, where M is the upper bound of $|f|$ on ab. The first term on the right dominates the part of the sum for which the intervals contain points of D_ϵ, while the second does the same for the remaining terms. Hence the lower bound of the sum on the left is zero. But this is a sufficient condition that the integral

$$\int_a^b f\,dP$$

exists, as may be proved by precisely the method usually applied to the integral of Riemann (Op. cit., pp. 250, 255). It follows with the help of (4) that the integral (1) exists since the same is true when u is replaced by either P or N, and $u(x) - u(a) = P - N$.

The only case remaining to be considered is that of a function $u(x)$ of limited variation which has discontinuities. It is known that the totality of discontinuities of such a function is denumerable, and $u(x)$

is expressible as a sum $u(x) = v(x) + j(x)$, where $v(x)$ is of limited variation and continuous, and $j(x)$ is the 'function of jumps'

$$j(x) = \sum_{a < \xi \leqq x} [u(\xi) - u(\xi - 0)] + \sum_{a \leqq \xi < x} [u(\xi + 0) - u(\xi)]. \qquad (5)$$

The sums on the right are taken in each case for all the discontinuities ξ satisfying the inequality below the summation sign, and are absolutely convergent.

It can be shown first of all that if the integral (1) exists the discontinuities of f and u are necessarily distinct. For suppose there were a discontinuity ξ common to both with $|u(\xi + 0) - u(\xi - 0)| > \eta$, $O(\xi) > \eta$. On a sufficiently small interval $\xi - h \leqq x \leqq \xi + h$ the absolute value of $u(\xi + h) - u(\xi - h)$ and the oscillation of f would both exceed η. Hence for every norm δ there would exist two sums S and S' coinciding except on this interval and such that $|S - S'| > \eta^2$, which contradicts the existence of the limit (1). If $u(\xi - 0) = u(\xi + 0)$ at a discontinuity ξ, sums contradicting the existence of the limit can be similarly constructed by using intervals having ξ as end point.

In the second place the integral (1) with $j(x)$ in place of $u(x)$ always exists if the discontinuities of f and u are distinct, and it has the value

$$\int_a^b f\,dj = \sum_{\xi} f(\xi)[u(\xi + 0) - u(\xi - 0)], \qquad (6)$$

the sum being taken for all discontinuities ξ of $u(x)$ with the understanding that $u(a - 0) = u(a)$, $u(b + 0) = u(b)$. The series (5) for $x = b$ converge absolutely, and it is possible to select a set of discontinuities ξ_k $(k = 1, 2, \ldots, m)$ so that the sum of the absolute values of the terms of (5) not involving the ξ_k's is less than ϵ, and the corresponding sum from (6) less than ϵM. Consider now a sum S for the integral (6) with norm δ so small that no two values ξ_k can lie in the same interval. The value of S is the sum of the terms of $j(b)$ from (5) each multiplied by a factor of the form $f(X)$ with $|X - \xi| < \delta$. The terms not involving the ξ_k's have a sum less than ϵM in absolute value, while those involving the ξ_k's will differ from the corresponding terms of (6) by less than ϵM, say, if δ is sufficiently small since f is continuous at every ξ. Hence for a sufficiently small δ, the sum S differs from the sum on the right in (6) by less than $3\,\epsilon M$, which proves the statement at the beginning of this paragraph.

It follows then that a necessary and sufficient condition for the integral (1) to exist is that f and u have no discontinuity in common, and

that the integral shall exist with v in place of u. For since $u = v + j$ every sum S for u is the sum of two similar ones for v and j, and when f and u have no common discontinuity the last has a definite limit as δ approaches zero, as has been shown. This condition is, however, equivalent to that of the theorem. For the total variation of u on an interval is the sum of the total variations of v and j, and the fact that the set of points common to two denumerable sets of intervals is itself representable as such a denumerable set, makes it possible to show that the total variation of u on a set of points D is also the sum of the similar variations for v and j. Furthermore the total variation $J(x)$ of j is the value obtained from the series (5) by replacing each term by its absolute value, and on a set of points D containing no discontinuity of u it is zero. For if the ξ_k's are a set of points as described above, a denumerable set A of non-overlapping intervals can be selected approaching each ξ_k as a limit on both sides and containing all the points of ab except the ξ_k's. This set of intervals will also enclose D in its interior since D contains no ξ_k, and the sum of the total variations of j on A will surely be less than ϵ. It is easy to see, conversely, that when the variation of j is zero on D then the latter contains no discontinuity of u. Hence the existence of the integral with v in place of u and the conditions that f and u have no discontinuity in common imply that the total variation of u on D is zero, and conversely, which was to be proved.

TRANSFORMATIONS OF APPLICABLE CONJUGATE NETS OF CURVES ON SURFACES

By Luther Pfahler Eisenhart

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Communicated by E. H. Moore, September 24, 1917

When the rectangular point coördinates x, y, z of a surface satisfy an equation of the form

$$\frac{\partial^2\theta}{\partial u\partial v} = a\,\frac{\partial\theta}{\partial u} + b\,\frac{\partial\theta}{\partial v}, \tag{1}$$

the curves $u=$const., $v=$const. form a conjugate system. We assume that the parametric system of curves is of this sort throughout this note, and we shall speak of it as a *net*. Equation (1) is the *point equation* of the net.

If N is such a net, the coördinates x', y', z' of a second net N' are given by quadratures of the form

$$\frac{\partial x'}{\partial u} = h\frac{\partial x}{\partial u}, \quad \frac{\partial x'}{\partial v} = l\frac{\partial x}{\partial v}, \tag{2}$$

provided that h and l are functions of u and v subject to the conditions

$$\frac{\partial h}{\partial v} = a(l - h), \quad \frac{\partial l}{\partial u} = b(h - l). \tag{3}$$

Moreover, each pair of solutions h, l of these equations leads by quadratures to a net N', such that the tangents at corresponding points M and M' to the curves of the net are parallel. All nets *parallel* to N are obtained in this way.

If θ is a solution of equation (1), and θ' is the corresponding function given by

$$\frac{\partial \theta'}{\partial u} = h\frac{\partial \theta}{\partial u}, \quad \frac{\partial \theta'}{\partial v} = l\frac{\partial \theta}{\partial v}, \tag{4}$$

then the functions x_1, y_1, z_1 defined by equations of the form

$$x_1 = x - \frac{\theta}{\theta'}x' \tag{5}$$

are the coördinates of a net N_1, so related to N that the lines joining corresponding points M and M_1 of these nets form a congruence whose developables meet the surfaces on which these nets lie in the curves of the nets. We say that the nets so related geometrically are in the relation of a *transformation* T. Parallel nets are in such relation. We have shown[1] that any transformation T of N into a non-parallel net N_1 is given by equations of the form (5). Hence any transformation T of N is determined by a parallel net and by a solution of the point equation of the net.

When two surfaces are applicable to one another, in the correspondence thus established there is a unique conjugate system of curves on one surface corresponding to a conjugate system on the other. We say that these nets are *applicable*. This paper is concerned with the transformations of applicable nets into applicable nets.

If we have two applicable nets N and $\bar{N}$ with the respective point coördinates x, y, z and $\bar{x}$, $\bar{y}$, $\bar{z}$, the analytical condition of their applicability is

$$\Sigma\left(\frac{\partial x}{\partial u}\right)^2 = \Sigma\left(\frac{\partial \bar{x}}{\partial u}\right)^2, \quad \Sigma\frac{\partial x}{\partial u}\frac{\partial x}{\partial v} = \Sigma\frac{\partial \bar{x}}{\partial u}\frac{\partial \bar{x}}{\partial v}, \quad \Sigma\left(\frac{\partial x}{\partial v}\right)^2 = \Sigma\left(\frac{\partial \bar{x}}{\partial v}\right)^2 \tag{6}$$

the sign Σ indicating the summation of terms in x, y and z. Since the coefficients a and b of equation (1) are functions of the left-hand

members of (6) and their derivatives, equation (1) is likewise the point equation of the net $\bar{N}$. In view of this fact a pair of functions h and l satisfying (3) leads to a net $\bar{N}'$ parallel to $\bar{N}$ as well as to N' parallel to $\bar{N}$. Moreover, the nets N' and $\bar{N}'$ are applicable. This result is due to Peterson.[2] The common point equation of the nets N' and $\bar{N}'$ admits the solution

$$\theta' = x'^2 + y'^2 + z'^2 - \bar{x}'^2 - \bar{y}'^2 - \bar{z}'^2. \tag{7}$$

This function and the corresponding function θ given by the quadrature (4) determine transforms N_1 and $\bar{N}_1$ of N and $\bar{N}$ respectively, and these new nets are applicable. Hence each net parallel to one of two applicable nets determines a parallel to the other, and by a further quadrature two applicable nets which are T transforms of the original nets. Moreover, the function θ' given by (7) is the only one leading to such a result. This result can be generalized at once to applicable nets in space of any order.

Nets are of three types with regard to applicability. Nets of the first type do not admit any applicable nets. Those of the second type admit one applicable net, whereas each net of the third type admits an infinity of applicable nets. We say that the latter are permanent in deformation, and for the sake of brevity call them *permanent nets*. Every net parallel to a permanent net is a permanent net, and each of the infinity of nets applicable to the one is parallel to one of the infinity applicable to the other by the method of Peterson. Suppose now that we have a permanent net N, two applicable nets $\bar{N}$ and $\bar{\bar{N}}$, and the respective parallel applicable nets N', $\bar{N}'$, $\bar{\bar{N}}'$. By the process of the preceding paragraph we obtain two transforms N_1 and N_2 of N, in general distinct, such that corresponding points of N, N_1 and N_2 lie on the same line, whose direction-parameters are the coördinates of N'. At the same time we obtain two transforms of $\bar{N}$ and two of $\bar{\bar{N}}$. As N admits an infinity of applicable nets, this process can be extended with the result that, in general, N and each of its deforms admit an infinity of transforms. We have raised the question whether in any case this infinity of transforms were coincident for each of the nets so that we obtain a permanent net N_1, whose infinity of applicable nets are the T transforms of the nets applicable to N. We refer to this question as *Problem A*.

Permanent nets belong to the general class of nets whose tangential coördinates satisfy an equation of Laplace with equal invariants. We have established[3] the existence of transformations T of nets of this kind into similar nets. When in particular the given N is a permanent net,

by the solution of a completely integrable system of partial differential equations of the first order a family of parallel nets of a particular type are obtained, each of which determines a T transform N_1, which also is a permanent net. All of these transformations are now shown to give a solution of Problem A.

In the transformations just referred to we did not consider permanent nets for which the curves in one family are represented on the Gauss sphere by one system of the imaginary generators. Drach[4] solved the problem of the deformation of nets of this kind. We show how in two ways these nets can be transformed into nets of the same kind as a solution of Problem A.

The third type of permanent nets are those whose two families of curves are represented on the sphere by its isotropic generators. These curves are the minimal lines on a minimal surface. There are no transformations of nets of this kind into similar nets furnishing a solution of Problem A.

[1] Eisenhart, *Trans. Amer. Math. Soc., New York*, **18**, 1917, (97-124).

[2] Peterson, *Ueber Curven und Flachen*, Moskau and Leipzig, 1868, (106).

[3] Eisenhart, *Rend. Circ. Mat., Palermo*, 39, 1915, (153-176).

[4] Drach, *Ann. Fac. Sci. Toulouse*, (Ser. 2), **10**, 1908, (125-164).

ON BILINEAR AND N-LINEAR FUNCTIONALS

By Charles Albert Fischer

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Communicated by E. H. Moore, October 1, 1917

It has been proved by Riesz[1,2] that if a linear functional $A(f(s))$ is continuous with zeroth order, there is a unique regular function $\alpha\,(s)$ which satisfies the equation

$$A\,(f)\ = \int f\,(s)\,d\alpha\,(s),$$

and the variation of α is the least upper bound of the expression $|\,A(f)\,|\,/mf$, where mf is the maximum of $|\,f\,(s)\,|$. From this theorem Fréchet][3] has proved that if $U\,(f\,(s), g\,(t))$ is bilinear, that is linear in each argument, there is a function $u\,(s,t)$ which is regular in t and satisfies the equation

$$U\,(f, g) = \int\int f\,(s)\,g\,(t)\,d_2u\,(s, t), \tag{1}$$

and by modifying the definition of the variation of a function of two variables, he has proved that the variation of $u\,(s,t)$ is the least upper bound of $|\,U\,(f, g)\,|\,/mfmg$.

In the present note equation (1) is derived by a different method, and the function $u(s, t)$ is proved to be regular in both arguments when determined by this method, and unique if regular. The theorem can then be extended by mathematical induction to n-linear functionals.

The variation V_2u of $u(s, t)$ is defined by Fréchet as the least upper bound of the expression

$$\sum_{i,j} e_i e_j \, \Delta_{ij} u(s, t),$$

where e_i and e_j are taken equal to plus or minus 1 in such a way as to make the summation as large as possible, and

$$\Delta_{ij} u(s, t) = u(s_{i+1}, t_{j+1}) - u(s_i, t_{j+1}) - u(s_{j+1}, t_j) + u(s_i, t_j).$$

The double Stieltjes integrals considered here will all be of the form

$$\int_T f(s)\, g(t)\, d_2 u(s, t) = \text{limit} \sum_{i,j} f(s_i')\, g(t_j')\, \Delta_{ij} u(s, t),$$

where the region T is defined by the inequalities

$$T:\ a \leqq s \leqq a';\ b \leqq t \leqq b',$$

and s_i' and t_j' are in the intervals (s_i, s_{i+1}) and (t_j, t_{j+1}) respectively. Such an integral always exists if f and g are continuous and V_2u is finite, and it must satisfy the inequality

$$\left| \int_T f(s)\, g(t)\, d_2 u(s, t) \right| \leqq mf\, mg\, V_2 u.^3 \qquad (2)$$

A function $u(s,t)$ will be called regular here if V_2u is finite, $u(a,t) = u(s, b) = 0$, and $u(s, t) = u(s+0, t) = u(s, t+0)$ excepting on the boundary of T. This makes some of the work simpler than to assume with Fréchet that $2u(s, t) = u(s-0, t) + u(s+0, t)$. If $u(s, t)$ is regular, its variation in one variable, when the other is constant, cannot be greater than V_2u. The double integral can then be expressed as an iterated integral by the equation[3]

$$\int_T f(s)\, g(t)\, d_2 u(s, t) = \int_a^{a'} f(s)\, d_s \int_b^{b'} g(t)\, d_t u(s, t).$$

The functional

$$v(g(t); s) = \int_b^{b'} g(t)\, d_t u(s, t)$$

can be proved regular in s by proving that its variation cannot be greater than $mg V_2u$, and then proving that $v(g; s + \epsilon)$ approaches $v(g; s)$ when ϵ

approaches zero. The details of the proof of this will not be given here.

If two regular functions $u(s, t)$ and u' (s, t) satisfy the equation

$$\int_T f(s)\, g(t)\, d_2 u(s, t) = \int_T f(s)\, g(t)\, d_2 u'(s, t) \tag{3}$$

identically in f and g, they will be proved to be identical. Since the similar theorem has been proved for single Stieltjes integrals, if u and u' are unequal at a point (s', t') there must be a continuous function $g(t)$ such that $v(g; s') \neq v'(g; s')$, where v' is the functional analogous to $v(g; s)$. Then since v and v' are regular in s there must be a continuous function $f(s)$ for which the two members of equation (3) are unequal. This proves that if a functional $U(f, g)$ satisfies the equation (1) where $u(s, t)$ is regular and independent of f and g, the function $u(s, t)$ must be unique.

As Riesz has proved,[2] the field of functions for which a linear functional is defined can be extended to any function which is the limit of a sequence of continuous functions which satisfy the inequalities

$$f_1 \geqq f_2 \geqq f_3 \geqq \ \ldots, \tag{4}$$

and any linear combination of such functions. Thus if $U(f, g)$ is bilinear, one or both of the functions f and g may be discontinuous if it is the limit of such a sequence. A function $f(s, s')$ will be defined by the equations

$$\begin{aligned} &f(s, a) = 0, && \\ &f(s, s') = 1, && (a \leqq s \leqq s';\ s' > a), \\ &f(s, s') = 0, && (s' < s \leqq a'), \end{aligned}$$

and a function $g(t, t')$ by the analogous equations. If f is considered a function of s it is approached by a sequence such as (4), as Riesz has shown,[2] and $g(t, t')$ has the same property. The function $u(s, t)$ will then be defined by the equation

$$u(s', t') = U(f(s, s'), g(t, t')).$$

This function vanishes for $s' = a$, or $t' = b'$, by definition. Since U is bilinear the expression $U(f, g)/mfmg$ is bounded[3] and its least upper bound will be called M, a constant independent of f and g. The variation of u is defined as the upper bound of the expression

$$\sum_{i,j} e_i e_j\, \Delta_{ij} u(s', t') = U\left(\sum_i e_i\, (f(s, s'_{i+1}) - f(s, s'_i)) \sum_j e_j\, (g(t, t'_{j+1}) - g(t, t'_j))\right),$$

and since from definition

$$\sum e_i \left(f(s, s'_{i-1}) - f(s, s'_i) \right) \leqq 1$$

the right member of the last equation cannot be greater than M. That is

$$V_2 u(s, t) \leqq M. \tag{5}$$

It can be proved by the method Riesz uses in showing that a linear functional is defined uniquely for the limit of a sequence such as (4), that the function $u(s, t)$ just defined is the limit of $u(s+\epsilon, t)$ when $\epsilon = 0$. Thus $u(s, t)$ is regular.

It will now be proved that

$$U(f, g) = \int_T f(s)\, g(t)\, d_2 u(s, t), \tag{6}$$

where $f(s)$ and $g(t)$ are arbitrary continuous functions. The function $f'(s)$ will be defined by the equation

$$f'(s) = \sum_i f(s'_i) \left(f(s, s'_{i+1}) - f(s, s'_i) \right),$$

and $g'(t)$ in the analogous way. Then the equation

$$U(f', g') = \sum_{i,j} f(s'_i)\, g(t'_j)\, \Delta_{ij} u(s', t') \tag{7}$$

will be satisfied. It follows from definition that $f'(s) = f(s'_i)$ in the region $s'_i < s \leqq s'_{i+1}$, and similarly for g' and g. Since U is linear in each argument and f and g are continuous, $U(f'_i, g')$ approaches $U(f, g)$ when the length of the greatest of the intervals approaches zero, and the right member of equation (7) approaches the Stieltjes integral in equation (6). Inequalities (2) and (5) imply that $V_2 u$ is equal to M. This completes the proof that when $U(f, g)$ is bilinear there is a unique function $u(s, t)$ which is regular and satisfies equation (6), and the variation of u is the least upper bound of $|U(f, g)|/mfmg$.

If this property is assumed for functionals linear in each of $n-1$ arguments, the proof just outlined can be modified to make it prove that functionals linear in n arguments have the same property. Thus the theorem holds for n-linear functionals.

This can be used to extend a theorem of Fréchet's[3] about functionals of the second order to those of the nth order. A functional of the nth order is defined as one that is continuous and satisfies the equation

$$U(f_1+f_2+\ldots+f_{n+1})-\sum_{r=1}^{n+1}U(f_1+\ldots+f_{r-1}+f_{r+1}+\ldots+f_{n+1})+$$
$$\sum_{r=1}^{n}\sum_{s=r+1}^{n+1}U(f_1+\ldots+f_{r-1}+f_{r+1}+\ldots+f_{s-1}+f_{s+1}+\ldots+f_{n+1})$$
$$\ldots+(-1)^{n+1}U(0)=0$$

identically. If $U(f)$ is also homogeneous, the functional defined by the equation

$$W(f_1,f_2,\ldots,f_n)=U(f_1+\ldots+f_n)-\sum_{r=1}^{n}U(f_1+\ldots+f_{r-1}+f_{r+1}+\ldots+f_n)+\ldots+(-1)^nU(0)$$

is easily proved to be linear in each of its arguments. If $f_i=f_2=\ldots=f$, it follows from definition and the condition of homogeneity, that

$$W(f,f,\ldots,f)=K_nU(f),$$

where

$$K_n=\sum_{r=0}^{n-1}(-1)^r\frac{n(n-1)\ldots(n-r+1)}{r!}(n-r)^n$$

But this expression can be proved equal to $n!$ which cannot vanish for any positive value of n, and since the n-linear functional W can be expressed as a multiple Stieltjes integral the homogeneous functional $U(f)$ of order n can be put in the same form.

[1] Riesz, *Ann. Sci. Ec. norm., Paris,* (Ser. 3), **28**, 1911, (36–43).

[2] *Ibid.*, **31**, 1914, (9–14).

[3] Fréchet, *New York, Trans. Amer. Math. Soc.*, **16**, 1915, (215–234).

THE CRYSTAL STRUCTURE OF CHALCOPYRITE DETERMINED BY X RAYS

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Communicated by A. A. Noyes, September 29, 1917]

Introduction.—This investigation of the atomic structure of crystals of chalcopyrite ($CuFeS_2$) was undertaken, as no study of a complex sulfide by the method of X-rays had previously been carried out. Moreover, comparatively few crystals of the tetragonal system, in which chalcopyrite crystallizes, have been examined; the only ones being certain oxides of the formula MO_2 studied by Vegard[1] and by Williams.[2] Yet the determination of the structure of crystals belong-

ing to other than the isometric system is likely to throw most light on the fundamental factors involved, such as the interatomic forces and the size and shape of the atoms themselves.

This research was carried out at the suggestion of Prof. A. A. Noyes with the aid of a grant made to him by the Carnegie Institution of Washington, for which we wish to express our indebtedness.

Description of the Apparatus and Procedure.—The method employed was in principle the same as that described by W. H. and W. L. Bragg,[3] but the apparatus was modified in several respects.

The electrical equipment for the X-ray excitation consisted of an interrupterless transformer of 20-kilowatts capacity. The X-ray tube was provided with a target of specially purified palladium, 2 mm. thick, silver-soldered onto a copper block, the face of which was set nearly perpendicular to the direction of the cathode rays. The electroscope used was one of the double-tilted form devised by Bumstead.

The crystal used in this investigation was selected from a number kindly loaned us by Prof. C. H. Warren of the Massachusetts Institute of Technology. The crystal was of the sphenoidal type,[4] approximately 8 mm. on the edges.

Summary of the Observations.—The following table summarizes the observations. The significance of the 'calculated' angles and intensities will be described below.

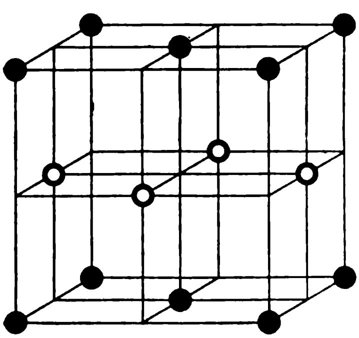
TABLE I

SUMMARY OF THE OBSERVED AND CALCULATED RESULTS

CRYSTAL PLANE	ORDER OF REFLECTION	ANGLE OF REFLECTION		ELECTROSCOPE DEFLECTION	RATIO OF INTENSITIES	
		Observed	Calculated		Observed	Calculated
(100)	I	6°25′	6°25′	5.5	100.0	100
	II	12°55′	12°55′	9.0	164.0	217
(001)	I	6°32′	6°32′	13.0	100.0	100
	II	13° 9′	13° 8′	33.5	256.0	222
(111)	I	5°32′	5°35′	49.0	100.0	100
	II	11° 7′	11°14′	6.5	13.0	4
	III	16°33′	16°58′	4.0	8.0	7
$(11\bar{1})$	I	5°41′	5°35′	69.0	100.0	100
	II	11°26′	11°14′	10.0	14.0	4
(110)	I	9° 7′	9° 6′	190.0	100.0	100
	II	18°28′	18°27′	45.0	24.0	20
	III	27°52′	28°19′	11.0	6.0	7
$(1\bar{1}0)$	I	9° 4′	9° 6′	61.0	100.0	100
	II	18°23′	18°27′	29.0	47.0	20
(101)	I	9° 7′	9°10′	200.0	100.0	100
	II	18°29′	18°35′	50.0	25.0	20
	III	28°25′	28°32′	9.0	4.5	7

Interpretation of the Results.—In determining the atomic structure of chalcopyrite, we will first attempt to fix the type of lattice on which it is constructed. For approximate purposes its crystals may be regarded as isometric, since the axial ratio $a: c$ (1: 0.985) is nearly unity.

The interplanar distance d (that is, the distance between like planes of atoms) is by the fundamental law of reflection ($n\lambda = 2d \sin \theta$) inversely proportional to the sine of the angle of reflection (θ). For the three fundamental planes (100) (101) (111) the angles were 6°25′, 9°7′, and 5°32′. The reciprocals of the sines of these angles stand to one another in the ratio 1 : 0.706 : 1.152. The ratios of the interplanar distances in the three possible types of isometric lattice, known as the cubic, the face-centered, and the cube-centered[5] are respectively 1 :0.707: 0.573; 1 : 0.707 : 1.146; and 1 : 1,414: 0.573. The lattice involved in the chalcopyrite crystal is therefore obviously the face-centered type; and we may infer that the heavy atoms, which predominate in determining the reflection, must be the points in this lattice.



COPPER AND IRON ATOMS IN CHALCOPYRITE LATTICE.

Since there is only a small difference in the atomic weights of iron and copper, their reflecting powers, like those of potassium and chlorine in potassium chloride, will not be greatly different. The atoms of iron and copper will therefore be practically indistinguishable in their effect on the X-rays. The observations then show that the basic lattice formed by the iron atoms must intersect that formed by the copper atoms in such a way as to form together a single face-centered lattice.

A study with the aid of a model of possible arrangements by which two different kinds of atoms, present in equal numbers, could together form a single face-centered lattice shows that there is only one such arrangement; namely, that shown in the accompanying figure, in which the iron atoms and copper atoms are represented by the solid and annular circles. It is evident, moreover, from the symmetries of the atomic arrangements that the vertical axis in the figure corresponds to the tetragonal or c axis in the crystal.

These conclusions are corroborated by the fact that the observed angles of reflections agree closely with the 'calculated' angles (given in the table), which were computed geometrically from the assumed loca-

tion of the atoms in the lattice, taking as a basis the observed angle for the (100) plane.

We may now proceed to determine the location of the sulfur atoms, which can be done with the aid of the intensity measurements. It is known that, when the reflection takes place from only one kind of plane, the reflections of the first, second, and third orders have intensities which stand to one another approximately in the ratios 100 : 20: 7, commonly called the normal intensity-ratios. It is evident, if there be secondary planes of lighter atoms intermediate between the primary planes of heavier atoms giving rise to a given reflection, that the reflection will be diminished or increased in intensity in correspondence with the difference in phase of the two trains of emerging rays waves. In case the secondary planes of atoms lie midway between the primary planes, there would be a phase-difference of half a wave-length for the first order, one wave-length for the second order, and $1\frac{1}{2}$ wave-lengths for the third order; hence there would result a weakening of the intensity of the first-order and third-order reflections (equal for the two orders) and a strengthening of that of the second order. In case the secondary planes are displaced one-fourth of the distance between the primary planes, the phase difference will be $\frac{1}{4}$, $\frac{1}{2}$, and $\frac{3}{4}$ of a wave-length for the first, second, and third orders, and hence there will be an increase in the intensities of the first order and third orders (equal in the two cases) and a relatively large decrease in the second order.

Let us consider now the relative intensities of the different orders for the different planes. We see that for the (100) plane the ratio of the observed intensities of the first and second orders has the value 100 : 164 in place of the normal one 100 : 20. This large relative weakening of the first order and strengthening of the second order shows that planes of sulfur atoms are located not far from midway between the (100) planes of iron and copper atoms. The reflections from the (001) plane show a similar reversal of the normal ratio; and lead to the corresponding conclusion that planes of sulfur atoms are located also not far from midway between the (001) planes of iron and copper atoms. The form and location of the lattice of sulfur atoms becomes thereby fixed, if we consider the additional fact that the number of sulfur atoms is equal to the number of iron and copper atoms. In the figure given above a sulfur atom would be located at the center of each alternate cube (that is, in four of the eight cubes there represented).

This conclusion is confirmed by the observations of the reflections from the other planes. The geometrical relations show that this location of the sulfur atoms involves that they lie in the (110) and (101)

planes of the iron and copper atoms; and that there is a plane of sulfur atoms in the (111) plane displaced one-fourth of the interplanar distance between the composite planes of iron and copper atoms. Correspondingly, the (110) and (101) planes show the normal ratios of intensities of the reflections for the first, second, and third orders; and the (111) plane shows the required decrease of the second-order reflection, and the expected normal ratio of the first-order and third-order reflections.

The relative intensities of the different orders can be calculated from the principle that the intensity of reflection from a plane of atoms is proportional to the square of the mass per unit-area.[6] Thus, the intensity of reflection from the (111) sulfur-atom plane would be to that from the (111) composite iron-copper atom plane as $(2 \times 32)^2 : (56 + 63.6)^2$ since there are two atoms of sulfur in an area equal to that in which there is one atom of iron and one atom of copper. The intensity of the resultant reflection will evidently be dependent both on the magnitudes of these two component reflections, and on the difference in phase in which they emerge. Algebraic expressions for the relative resultant intensities of the reflections of the different orders can be readily formulated. With the aid of these expressions the 'calculated' ratios of intensities given in the above table were obtained. It will be seen that there is a striking parallelism between the calculated and observed intensities.

Finally, we may further test the correctness of the deduced atomic structure by calculating the density of the substance and comparing it with the known density. Referring to the figure, it is seen that the space which it represents has associated with it two iron atoms, two copper atoms, and four sulfur atoms, or 2 of the atom-groups $CuFeS_2$. Since the mass of the hydrogen atom is 1.64×10^{-24} grams, that of these two $CuFeS_2$ groups is $2 \times \frac{183.6}{1.008}$ times as great, or 5.972×10^{-22} grams. The volume of the space in question is, however, equal to $8\,d^3 \times 0.985$, where d represents the distance between the (100) planes of copper-iron atoms (that between the (001) planes being $0.985\,d$). This distance d may be obtained from the law of reflection $\lambda = 2\,d \sin\theta$ by substituting for θ the observed angle of reflection (6°25′) for the (100) plane, and by substituting for λ its value 0.584×10^{-8} cm. as determined by W. L. Bragg[7] for a palladium target. The value of d is thus found to be 2.614×10^{-8} cm., and that of the volume in question 1.407×10^{-22} cm. The calculated density is therefore 5.972/1.407, or 4.24. The density of the mineral chalcopyrite, according to the

best determinations, lies between 4.1 and 4.3. That of the specimen used in this investigation is 4.19.

[1] Vegard, *Phil. Mag., London*, Nov., 1916 and Jan., 1917.
[2] Williams, *London, Proc. R. Soc.*, **93**, 1917, (418).
[3] Braggs' *X-Rays and Crystal Structure*, 1915, (22).
[4] Dana's *Mineralogy*, Fig. 10, 1892, (81).
[5] Tutton's *Crystallography*, 1911, (501); or Braggs' *X-Rays and Crystal Structure*, 1915, (91).
[6] Bragg, *op. cit.*, pp. 120–127.
[7] Kaye, *X-Rays*, 1917, (226).

THE ISOSTATIC SUBSIDENCE OF VOLCANIC ISLANDS

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Communicated October 9, 1917

Objection has sometimes been made to Darwin's theory of upgrowing coral reefs on subsiding foundations on the ground that the subsidence of the reef foundations should lower the ocean surface and lay bare a belt of recent marine deposits of smooth surface and simple shore line around the continental masses, while as a matter of fact the shore lines of continents are usually more or less embayed, as if the sea had recently advanced upon the unevenly eroded surface of the continental margins. The objection will, however, be found to have small weight when it is seen to rest upon the implied postulate that the embayments of continental coasts have as a rule been produced by a universal rise of the ocean surface, everywhere of the same amount and date, whereas their embayments testify to no such simple origin; and to proceed upon the unwarranted assumption that the subsidence of reef foundations requires the subsidence of broad areas of the ocean floors, whereas the local subsidence of the foundations themselves is all that is necessary. The postulate will first be examined and refuted; the assumption will then be considered and its alternative will be preferred.

The embayment of continental coasts is certainly of widespread occurrence, but when the embayments are closely examined they are found to be of dates and dimensions so diverse that they cannot be explained by a universal rise of the ocean. In the first place, the most pronounced embayments are the fiord coasts of quaternary glaciation; there is good reason to believe that the great troughs of such coasts were scoured out deep below sea level by huge glaciers, and that, far from the ocean having recently risen to submerge the troughs, the coasts have in several fiord regions risen from the ocean, as their elevated shore lines testify; these

coasts are embayed in spite of recent emergence, and cannot serve as witnesses to a recent rise of ocean level.

Furthermore, the supposition that coastal embayments result from a rise of the ocean around still-standing continental borders carries with it the implication that the coastal valleys, now submerged and embayed, were everywhere eroded during the same period of previous emergence, and hence that they should all be eroded to the same depth, and that their width should be proportionate to the weakness of the rocks in which they were carved during the emergence period. But as a matter of fact, the coastal embayments of the continents occupy valleys of depths and widths so various that they cannot be accounted for by these simple conditions: the diversity of the embayments demands many local and diverse movements of continental borders, even though a universal rise of ocean level has recently taken place, such as that which the final climatic changes at the close of the Glacial period should produce. The chief effect of such a rise of ocean level would only be to bring about a slight preponderance of embayed coasts along continental borders which had suffered local uplifts and depressions in about equal proportions.

Diverse movements of continental borders are demanded not only by the diversity of their embayments, but also by the not rare occurrence of emerged coastal plains of marine sediments. Thus the western side of the Adriatic is bordered by an emerged and more or less dissected coastal plain, while the eastern side is bordered by a partly submerged and elaborately embayed mountain border. Greece and the Bosphorus bear marks of recent submergence, but the coast of Palestine appears to bear marks of recent emergence. The small embayments of the Texas coastal plain cannot be explained as resulting from the same amount of submergence which produced the deep rias in the montainous coast of northwestern Spain; nor can the unlike valleys of those two coasts have been eroded during the same antecedent period of emergence. The elevated shore lines of the California coast have no equivalent on the volcanic islands of the Pacific. The recent submergence of the northeastern coast of New Caledonia following a long-enduring emergence, as indicated by the deep embayments of its strongly clift shores, demands a different succession of changes of level from those recorded on the coast of Peru or of Virginia.

The postulate that the embayed coasts of the world demand a universal rise of the ocean for their explanation is largely based upon the indoor study of coastal charts, and not upon the field study of the coasts themselves; and inasmuch as most of the coastal charts of the world have been made by hydrographers, who concerned themselves little

with the physiographic history of the coastal slope, such charts cannot be relied upon to present all the information that is pertinent to the problem under discussion. Thus if the coast of Maine were studied only from charts, its many embayments alternating with ragged promontories and outstanding islands, together with its small headland cliffs and its small bayhead deltas, might well be interpreted as indicating the recent submergence of a hilly region that had previously been eroded to subdued forms: but a study of that coast on the ground soon discovers that it bears a discontinuous cover of marine clays up to 100 or 200 feet above sea level; and one must therefore conclude that the embayments of today remain after the partial emergence of a previously more submerged region.

It occasionally happens, however, that a hydrographic chart presents evidence which suffices to correct a fase inference regarding recent submergence: thus one of the Solomon islands, which has a well-embayed shore line, is fronted for many miles by an elevated off-shore barrier reef, even crested and 80 or 100 feet in height: hence although the coastal embayments show that submergence has taken place, the reef shows that the last change of level was an emergence, less in amount than the previous submergence.

In view of these examples it is evident that the chart of an embayed coast, outside of the coral zone and not yet examined geologically, cannot be trusted as indicating that the last change of ocean level was upward. Therefore the inference that there has been a general rise of ocean level, independent of oscillations during the Glacial period, is not at present well supported; and until it is well supported, a certain amount of broad ocean-floor subsidence in the coral seas, as inferred by Darwin from his theory of coral reefs, may be regarded as not impossible, particularly as its effect in lowering the ocean may have been largely counteracted by equally broad ocean-floor uplifts in other regions.

Nevertheless, it is somewhat arbitrary to assume that the broad areas of ocean-floor subsidence should occur chiefly in the warmer zone, where coral reefs could be formed, and that the areas of compensating uplift should be chiefly in the cooler oceans, or in those parts of the warmer oceans where no islands rise. Hence the supposition of local subsidence of volcanic islands, which usually serve as reef foundations, deserves consideration as an alternative to broad ocean-floor subsidence. This supposition would be objected to by scientists of the older school, who believed that volcanoes occupy areas of elevation. Thus Guppy said: "In establishing the fact of the presence of active volcanoes in regions of barrier reefs and atolls, I shall be removing one of the principal stand-

points of the theory of subsidence;" he added that this "places the supporters of the theory of subsidence in a dilemna."[1] At about the same time Hickson wrote: "I am persuaded that . . . . the presence of such an atoll as Passiac [in North Celebes] so close to a region of quite recent and considerable volcanic activity is difficult to account for under this [subsidence] theory."[2] Murray went so far as to object to Darwin's theory of subsidence on the ground that even extinct volcanoes are not likely to subside. He wrote: "Generally speaking, all the volcanic regions which we know have in the main been areas of elevation, and we would expect the same to hold good in those vast and permanent hollows of the earth which are occupied by the waters of the ocean. . . . . Areas of local depression are to be looked for in the ocean basins on either side of and between groups of volcanic islands and atolls, and not on the very site of these islands."[3]

Much may be said against this obsolescent view, and in favor of the opposite view that volcanic action and subsidence may sometimes be closely associated. Strong testimony to this effect is offered by the repeated occurrence of volcanic eruptions in areas of subsidence during the geological evolution of Great Britain, as worked out by an exceptionally competent geologist whose conclusion is: "The study of the records of volcanic action in Britain proves beyond dispute that the volcanoes of past time have been active in areas of the earth's surface that were sinking and not rising. . . . . I do not wish to maintain that the downward movement was necessarily a consequence of volcanic ejections . . . . but I have sometimes asked myself whether it was not possibly increased as a sequel to vigorous action."[4] An interesting piece of evidence concerning the subsidence of the great volcanic island of Hawaii during the later stages of its eruptive growth is furnished by Branner, who gives good reason for thinking that the deep canyons in the northeastern sector of the island have been eroded in part of an older, deeply dissected, and now partly submerged volcanic mass, the remainder of which has been overwhelmed and buried under the much more recent lava floods that cover the major area of the island.[5]

The newest discussion of this problem is by Molengraaf, who calls attention to the results of recent gravity determinations, from which it appears that the volcanic islands of the Pacific "as far as they have been studied are not isostatically compensated, and, without exception, show a larger or smaller positive anomaly of gravity. . . . . These volcanic islands, rising . . . . as cones or groups of cones of considerable bulk, cannot always remain in existence; under the influence of gravity they will without exception yield and sink down slowly. . . . .

The yielding and slow sinking of the volcanic islands under the influence of gravity must be regarded as the cause of the downward movement of large amount and long duration which must be assumed in order to explain the formation of barrier-reefs and atolls in true oceanic regions."[6]

This appears to me an important suggestion, and one that is likely to remove the objections to Darwin's theory of coral reefs in so far as they are directed against a great subsidence of broad ocean-floor areas: for although Darwin himself was led by his coral-reef theory to infer the subsidence of such areas, it is clear from the original exposition of his theory that local subsidence of reef foundations will serve all its needs. It may be added that the accumulation of the great limestone masses of atolls upon slowly sinking volcanic foundations must aid and prolong their sinking; also that no comparable sinking of volcanic cones upon continents need be expected, not only because of the differences supposed to exist between the earth's crust in continental and oceanic areas, to which Molengraaf calls attention, but also because continental volcanoes suffer erosion, whereby their waste is carried away and widely distributed, while oceanic volcanoes, even if they rise for a time above sea level and suffer erosion, retain the waste from their summits on their flanks.

But it is particularly the relation of Molengraaf's hypothesis to changes of ocean level that I desire to emphasize. Let it be imagined that the ocean floor suffers no deformation apart from that associated with volcanic action, and that a thousand great volcanic cones are built up from it in the coral zone, one after the other and at such intervals of time that their formation stretches through the Tertiary and Posttertiary periods. The building of the first cone would cause a slight rise of ocean level. As the cone slowly subsided the ocean surface would return to its normal stand, were it not that the subsiding cone is reconstituted in an atoll as fast as it subsides,[7] and that other cones are built up as the first one sinks. Later formed cones would prolong the changes thus initiated, and the slow rise of the ocean would continue, particularly if the isostatic sinking of some of the cones were incompletely accomplished. Even if some of the cones sank so fast that reef-making corals could not reconstitute them, the net result of this process, after many cones had been built up and sunk again, would be, not a lowering of the ocean surface such as according to the usual interpretation of the theory of subsidence has been supposed to accompany the upgrowth of coral reefs, but a slow, small, and long continued rise of the ocean surface.

The rise of the ocean surface thus caused would be much less in total amount than the sinking suffered by any one of the volcanic reef founda-

tions, and hence less than the inferred great thickness of many atoll masses, but it would presumably be sufficient to cause a moderate preponderance of submergence on continental coasts which themselves suffer many diverse movements of upheaval and depression. It is not, however, to be supposed that general warpings and deformations of the ocean floor, upward and downward, should be left out of consideration; such movements have surely taken place to a less or greater degree, particularly in the western Pacific, where coral reefs border continental islands. The integrated effect of all these causes of change in the level of the ocean surface cannot now be determined, because so little is known regarding the various factors of the problem: but nothing in the little that is known and in the much more that may be fairly inferred should be regarded as discountenancing the theory of upgrowing reefs on subsiding foundations, essentially as Darwin supposed. His primary theory of coral reefs holds good, although his supplementary theory of broad ocean-floor subsidence needs modification.

[1] Guppy, H. B., *Scot. Geogr. Mag.*, **14**, 1888, (121–137); see p. 135, 136.

[2] Hickson, S. J., *A naturalist in Celebes*, London, 1889; see p. 42.

[3] Murray, J., *Proc. Roy. Soc. Edinb.*, **10**, 1880, (505–518); see p. 516.

[4] Geikie, Sir A., *The ancient volcanoes of Great Britain*, London, 1897; see vol. **2**, p. 470.

[5] Branner, J. C., *Amer. J. Sci.*, 16, 1903, (301–316); see p. 301–303.

[6] Molengraaf, G. A. F., *Proc. k. Akad. Wet. Amsterdam*, **19**, 1916, (610–627); see p. 619–620.

[7] This statement depends on the fact, certified by chemists, that the withdrawal of limestone from solutions in water diminishes the water volume by only a small portion of the volume of the withdrawn limestone.

ON THE DEFORMATION OF AN N-CELL

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Communicated by E. H. Moore, October 8, 1917

I propose to prove that *any (1 − 1) continuous transformation of an n-cell and its boundary into themselves, which leaves all points of the boundary invariant, is a deformation.*

For the purposes of this proof the n-cell may be taken to be the interior of an n-dimensional cube. A deformation is a $(1 - 1)$ continuous transformation F_1 which is a member (corresponding to $x = 1$) of a one-parameter continuous family of $(1 - 1)$ continuous transformations F_x $(0 \leq x \leq 1)$ such that F_0 is the identity. It is understood that each F_x is a transformation of the n-dimensional cube into a set of points of an n-dimensional Euclidean space in which the n-dimensional cube is situated.

The theorem is easy in the one-dimensional case. It has been proved in the two-dimensional case by H. Tietze (*Palermo, Rend., Circ. Mat.*, **38,** 1914, p. 247) and more simply, by H. L. Smith (in an article soon to appear in the Annals of Mathematics). No proof has been published so far as I am aware, for the higher cases.

The proofs by Tietze and Smith establish a stronger theorem than that stated above, for they show the existence of a family of transformations F_x each of which carries the square into itself and leaves all points of the boundary invariant. This restriction on the transformations F_x, that each of them shall carry the square into itself, is not needed in some of the important applications of the theorem; and without this restriction the theorem can be proved very easily.

The proof below is stated for the two-dimensional, but applies without change to the n-dimensional, case.

Let S_1 be a square, $ABCD$, whose sides are of length unity, it being understood that a square, unlike a cell, includes its boundary. Let T_2 be a translation parallel to the side AB which carries the side AD into the side BC, T_3 a translation parallel to the side BC which carries the side AB into the side DC, and T_4 the resultant of T_2 and T_3. Let S_2, S_3, S_4 be the squares into which S_1 is carried by T_2, T_3, T_4 respectively. Thus S_1, S_2, S_3, and S_4 together constitute a square whose sides are of length 2.

Let F_1 be a $(1 - 1)$ continuous transformation of S_1 into itself which leaves all points of the boundary of S_1 invariant. The transformation T_2F_1 (the resultant of F_1 followed by T_2) carries S_1 into S_2. I shall first show that T_2F_1 is a deformation and it then follows easily that F_1 is also a deformation.

The rectangle composed of S_1 and S_2 can be carried into the rectangle composed of S_3 and S_4 by a transformation Λ which for points of S_1, is the same as T_3 and for points of S_2, is the same as $T_4F_1^{-1}T_2^{-1}$. Since T_3 and $T_4F_1^{-1}T_2^{-1}$ have the same effect on the common points of the boundaries of S_1 and S_2, the transformation Λ is uniquely defined, $(1 - 1)$, and continuous.

The transformation $\Lambda.T_2F_1.\Lambda^{-1}$, as applied to S_3 is the same as $T_4F_1^{-1}T_2^{-1}T_2F_1T_3^{-1}$, which is the translation $T_4T_3^{-1} = T_2$, carrying S_3 into S_4; denote this translation T_2 by T_1. Let T_x $(0 \leq x \leq 1)$ denote the translation carrying S_3 a distance x in the direction of translation of T_1.

The existence of the family of translations T_x $(0 \leq x \leq 1)$ shows that T_1 is a deformation. But since $\Lambda.T_2F_1.\Lambda^{-1} = T_1$, $\Lambda^{-1}T_1\Lambda = T_2F_1$. Hence the existence of the set of transformations $\Lambda^{-1}T_x\Lambda$ $(0 \leq x \leq 1)$ shows that T_2F_1 is a deformation.

Any one of the transformations $\Lambda^{-1}T_x\Lambda$ effects a translation on the three sides, AB, CD, DA of S_1 and carries the side BC into the curve to which T_2F_1 carries the linear segment in which the square is met by a line parallel to AD at a distance x from AD. Since if T_x is the translation which has the same effect as $\Lambda^{-1}T_x\Lambda$ on the point A, the transformation $T_x^{-1}\Lambda^{-1}T_x\Lambda$ leaves all points of the three edges AB, CD, DA of S_1 invariant. Denote $T_x^{-1}\Lambda^{-1}T_x\Lambda$ by F_x. The set of transformations F_x $(0 \leqq x \leqq 1)$ is obviously a continuous one-parameter family of $(1-1)$ continuous transformations; F_0 is the identity; and F_1 the given transformation already denoted by F_1. Hence F_1 is a deformation.

The last paragraph can be replaced by the observation that since the product of two deformations is a deformation, F_1, which is the product of T_2^{-1} and T_2F_1, must be a deformation. It seems worth while, however, to indicate, as has been done, something of the nature of the family of transformations F_x which the process sets up.

A THEOREM ON SERIES OF ORTHOGONAL FUNCTIONS WITH AN APPLICATION TO STURM-LIOUVILLE SERIES

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Communicated by E. H. Moore, October 10, 1917

1. *The Theorem.*—An infinite set of continuous functions $u_1(x)$, $u_2(x)$, . . . is *closed* on the interval $0 \leqq x \leqq 1$ if there exists no continuous function $f(x)$ not identically zero for which $\int_0^1 f(x)\, u_n(x)\, dx$ vanishes for all n; the set is *normalized* if $\int_0^1 u_n^2(x)\, dx = 1$ for all n; it is *orthogonal* if $\int_0^1 u_m(x)\, u_n(x) dx = 0$ for $m \neq n$. Most of the series of mathematical physics are linear in closed normalized orthogonal sets of functions.

THEOREM. *If $u_1(x), u_2(x)$, . . . form a closed normalized orthogonal set of functions, and if $\bar{u}_1(x)$, $\bar{u}_2(x)$, . . . form a second normalized orthogonal set such that*

$$\sum_{n=1}^{\infty} (u_n(x) - \bar{u}_n(x))\, u_n(y) \qquad (0 \leqq x, y \leqq 1)$$

converges to a function $H(x, y)$ less than 1 in numerical magnitude in such wise that the series multiplied through by an arbitrary continuous function $f(x)$ can be integrated term by term as to x and yields a uniformly convergent series, then the set $\bar{u}_1(x)$, $\bar{u}_2(x)$, . . . is closed also.

Proof. If the set $\bar{u}_1(x)$, $\bar{u}_2(x)$, . . . is not closed there exists an f not identically zero such that $\int_0^1 f(x)\, \bar{u}_n(x)\, dx$ vanishes for all n. In

this event, if we multiply through the equation of definition for H by $f(x)$ and integrate as we may by hypothesis, we find

$$\int_0^1 f(x)\, H(x, y)\, dx = \sum_{n=1}^{\infty} \int_0^1 f(x)\, u_n(x)\, dx \cdot u_n(y),$$

where the series on the right-hand side will converge uniformly by hypothesis and so represents a continuous function.

This series on the right has the value $f(y)$. In fact the difference

$$f(y) - \sum_{n=1}^{\infty} \int_0^1 f(x)\, u_n(x)\, dx \cdot u_n(y)$$

is a continuous function $\varphi(y)$ such that $\int_0^1 \varphi(y) u_n(y)\, dy$ vanishes for all n, precisely because the set $u_1(x), u_2(x), \ldots$ is normalized orthogonal. But the set $u_1(x), u_2(x), \ldots$ is closed by hypothesis; hence we infer that φ vanishes identically. Hence the right-hand member of the preceding equation has the value $f(y)$. That equation may now be written

$$\int_0^1 f(x)\, H(x, y)\, dx = f(y).$$

If the maximum numerical value of $f(x)$ for $0 \leqq x \leqq 1$ is $F \neq 0$, and if this value is taken on for $y = y_0$, we get

$$\left| \int_0^1 f(x)\, H(x, y_0)\, dx \right| = F.$$

But this is impossible since by hypothesis $|H| < 1$ and $|f| \leqq F$.

The set $\bar{u}_1(x), \bar{u}_2(x), \ldots$ is therefore closed also.

2. *An Application.* The Sturm-Liouville series, in a specialized but typical form, arises from the set of functions $u_1(x), u_2(x), \ldots$ which are the solutions of a linear differential equation of the second order

$$u'' + (\lambda - g(x))\, u = 0$$

satisfying boundary conditions $u(0) = u(1) = 0$, for the ordered set of parameter values $\lambda_1, \lambda_2, \ldots$ of λ respectively. We will assume that $g(x)$ and $dg(x)/dx$ are real and continuous.

By means of the methods of Sturm it is proved that $u_1(x), u_2(x), \ldots$ may be taken to form a normalized orthogonal set in which $u_n(x)$ will vanish precisely n times within the interval $0 \leqq x \leqq 1$. And the methods of Liouville give the asymptotic form of $u_n(x)$; it will be convenient for us to use the formula

$$u_n(x) = \sqrt{2}\left[\sin n\pi x + \frac{\varphi(x)\cos n\pi x}{n} + \frac{M_n(x, g(x))}{n^2}\right]$$

in which M_n is a continuous functional of x and $g(x)$, bounded as long as $g(x)$ and $dg(x)/dx$ are bounded uniformly for all n.

For the satisfactory investigation of the representation of an arbitrary function in Sturm-Liouville series it is necessary to know further that the set $u_1(x), u_2(x), \ldots$ thus obtained is closed. A very simple proof and the first is due to Stekloff.[1] His proof uses, however, the theory of functions of a complex variable. There is indeed a special case, namely the case when $g(x)$ vanishes identically and $u_n(x) = \sqrt{2} \sin n\pi x$, when a short proof is possible by an elementary method.

The theorem proved above makes possible an immediate demonstration that the set is closed, once the asymptotic formula above and the fact for the special case are granted. Let us replace $g(x)$ by $\sigma g(x)$ where σ is a real parameter varying from 0 to 1. If then $u_1(x), u_2(x), \ldots$ denote the members of the set for any particular σ and $\bar{u}_1(x), \bar{u}_2(x), \ldots$ denote the members for another value of σ, say $\bar{\sigma}$, it will suffice to show that the series for H will have the properties demanded in the theorem for $|\sigma - \bar{\sigma}| \leqq \delta > 0$. For in this event since the set is closed for $\sigma = 0$ (the special case), it will be closed for $\sigma \leqq \delta$ by an application of the theorem; by successive further applications of the theorem it is shown in the same way that the set is closed for $\sigma \leqq 2\delta$, $\sigma \leqq 3\delta, \ldots$, so that finally we infer that the theorem is true for $\sigma = 1$, i.e. for the given set.

In virtue of the explicit formula for $u_n(x)$ we have

$$u_n(x) - \bar{u}_n(x) = \sqrt{2}\left[\frac{(\sigma - \bar{\sigma})\,\varphi(x)\cos n\pi x}{n} + \frac{M_n(x, \sigma g(x)) - M_n(x, \bar{\sigma} g(x))}{n^2}\right],$$

$$u_n(y) = \sqrt{2}\left[\sin n\pi y + \frac{\varphi(y)\cos n\pi y}{n} + \frac{M_n(y, \sigma g(x))}{n^2}\right].$$

Multiplying these two expressions on the right together we obtain the typical term of the H series. This series breaks up at once into six (2×3) other series all of which converge uniformly to a small value for $|\sigma - \bar{\sigma}|$ small save the series obtained from the leading terms in both expressions, namely

$$2(\sigma - \bar{\sigma})\,\varphi(x)\sum_{n=1}^{\infty}\frac{\cos n\pi x \sin n\pi y}{n}.$$

If we omit the small factor $(\sigma - \bar{\sigma})\,\varphi(x)$, this series may be written

$$\sum_{n=1}^{\infty}\frac{\sin n\pi(x+y) - \sin n\pi(x-y)}{n}.$$

Now the sum of any number of terms of a series Σ (sin $n\pi z)/n$ is known to remain uniformly bounded,[2] and the series is known to converge everywhere, uniformly save in the immediate vicinity of the values $z = 0$, $\pm 2\pi$, $\pm 4\pi$, etc. Hence the series displayed above converges uniformly for all x, y save in the immediate vicinity of $x = y$, where, however, the sum of any number of terms of the series is bounded. Hence all six types of series will converge to values small numerically for $| \sigma - \bar{\sigma} |$ small, and, when multiplied through by any continuous function may be integrated term by term as to x, yielding uniformly convergent series in y. Thus the H series will have the stated properties for $| \sigma - \bar{\sigma} | \leqq \delta > 0$.

3. *Generalization.*—The theorem suggests at once a theorem in General Analysis as defined by E. H. Moore.[3] If we employ a quasi-geometrical terminology this generalization may be stated as follows: *any set of orthogonal vectors in a functional space lying near enough to a complete set of orthogonal vectors in that space is itself complete.* Another still wider generalization suggests itself: *any set of vectors in a functional space lying near enough to a complete set of vectors admitting a reciprocal set is itself complete and admits a reciprocal set.*[4] This second generalization evidently plays the same part in relation to biorthogonal sets that the first does for orthogonal sets.

[1] See Kneser, *Die Integralgleichungen und ihre Anwendungen in der mathematischen Physik*, Braunschweig, 1911, pp. 84–95.

[2] See D. Jackson, *Rend. Circ. Mat. Palermo*, **32,** 1911, (257–262).

[3] See *Bull. Amer. Math. Soc., New York*, **18,** 1911–1912, (334–362).

[4] For a theorem of this type see an article of mine, *Paris, C.-R. Acad. Sci.*, **161,** 1917, (942–945).

LOW-TEMPERATURE FORMATION OF ALKALINE FELDSPARS IN LIMESTONE

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Communicated by W. M. Davis, October 5, 1917

Recent monographs by A. Heim[1] and D. Trümpy,[2] dealing with certain rock formations in Switzerland, put new emphasis on an important problem in minerogenesis. At different horizons of the Jurassic limestones of the Churfirsten-Mattstock mountain group, Heim has found abundant crystals of albite which have evidently developed *in situ* and are not of clastic origin. The crystals are automorphic, with maximum lengths of 0.2 mm. and average lengths much less (see fig. 150 in Heim's memoir). Minute crystals of ankerite are associated. Both albite and ankerite are regarded by Heim as due to crystallization on the sea-floor, during

the deposition of the limestone mud, which he interprets as likewise a chemical precipitate. The calcareous matrix has, in fact, the uniform, exceedingly fine grain characteristic of chemically precipitated limestone, though it encloses fragments of fossil shells and skeletons.

Trümpy, in the western Rhätikon, found similar albites, new crystallizations, in Jurassic limestone. These crystals are generally less than 0.03 mm. long and seldom as much as 0.05 mm. long. They are associated with colorless to somewhat brownish foils of mica with the same order of dimensions. Trümpy also describes secondary albite and orthoclase crystals, 0.05 to 0.08 mm. long, in the Tertiary (Flysch) limestones of the same district. The feldspars enclose roundish granules of calcite identical in size and form with the normal constituents of the mud-like limestone. In many cases the feldspar is merely a 'Bindemittel' for such grains of calcite. Trümpy agrees with Heim that the feldspars grew, 'schwebend,' in the calcareous mud.

Both Heim and Trümpy found no difficulty in distinguishing these new feldspars from the quite different clastic feldspars occasionally seen in the same thin sections. They have good evidence that the new feldspars were not developed by dynamic or contact metamorphism of the limestones—a conclusion borne out by the extremely fine grain of the calcareous material.

Thirty years before, Kaufmann[3] described similar albites, "doubtless formed *in situ*", as occurring in Jurassic, Lower Cretaceous, Upper Cretaceous, and Eocene limestones of the "Emmen- und Schlierengegenden" of Switzerland. The maximum length of the albite crystals in any of the formations is 0.08 mm. Kaufmann noted that they abound specially in the purer limestones.

Independently Lory[4] had found non-clastic albite and orthoclase crystals in most of the non-metamorphosed Jurassic limestones of the French Alps. Such feldspars are described as particularly abundant in nodular and geodic concretions. Pyrite crystals and minute, doubly-terminated quartz crystals accompany the feldspars. All the crystals are explained as growths in limestone muds charged with organic (bituminous) matter. A year later Lory[5] noted the occurrence of albites and bipyramidal quartzes in Eocene limestone at Montricher, near Saint Jean-de-Maurienne. He attributed the formation of the feldspars to special conditions which were not stated, but he excluded metamorphism as an explanation. He found orthoclase crystals in the cast of an Ammonite.

Foullon[6] reported the fine-grained to dense Eocene limestone of the Aegean island of Rhodes to contain 1.1 to 1.2% of well crystallized sili-

cates, including very pure albite. The largest feldspars seen were from 2.5 to 3.0 mm. in length. He suggested that the albite crystals originated, as tiny individuals, at the contact of sea-water and the calcareous mud at the sea-bottom; and that these grew to present size in the loose sediment as deposition went on. Only on such a hypothesis, he held, could one explain the automorphic character of the crystals. Since "certainly considerable time was necessary for their formation before being completely enclosed in the mud," he regards these crystals as proving the slowness of the calcareous precipitation. Foullon notes the absence of appreciable lime in the feldspar molecule itself.

Cayeux[7] stated that similarly authigenic[7] orthoclase is to be found in all the named horizons of the Chalk of the Paris basin. The crystals are always minute, averaging 0.04 to 0.05 mm. With constant crystal forms, they occur singly, never in groups. It is observed that orthoclase and glauconite tend to exclude each other in the various limestones. Cayeux assumes a community of origin for both minerals, described as "secondary." After examining his material, Michel Lévy, Lacroix, and Termier all agree that the orthoclase must have formed *in situ*.

Grandjean[8] studied the chalk of Meudon, near Paris, concluding that its feldspar (probably microcline, rather than orthoclase) was formed on the floor of the Cretaceous sea, "contemporaneously with the deposition of the chalk." The feldspar "grew rapidly; it probably ceased to grow after it was once buried." He continued: "it is thoroughly [infiniment] probable that feldspar is forming on the present sea-bottom."

A review of these European studies leads to the following generalizations:

1. Authigenic alkaline feldspars appear in non-metamorphosed limestones of Triassic, Jurassic, Cretaceous, and Eocene age and specially characterize many horizons in the Alpine Jurassic and Paris-basin Cretaceous.

2. Albite or orthoclase may occur alone or they may be associated in the same layer of limestone. Pyrite, bituminous matter, and bipyramidal quartz are not uncommon companions of the feldspars. One Jurassic limestone exhibits authigenic mica.

3. These feldspars in the European limestones typically appear as single crystals, with good crystal form and separated by the calcareous matrix; their lengths range generally from 0.03 to 0.08 mm.

4. All the calcareous matrices have the grain and texture of chemical precipitates. However, embedded in those originally muddy materials are the strongly contrasted fragments of organic origin, shells and skeletons.

5. All ten of the authors who have reported on the rocks mentioned agree that the albite or orthoclase has crystallized *in situ* without any help from metamorphism. Foullon, Grandjean, Trümpy, and Heim definitely express the view that the feldspars developed, as *chemical precipitates on the sea-floor* before or immediately after the burial of the corresponding laminae of sediment by the younger laminae. According to Cayeux, Lory held the same belief for the orthoclase crystals of the Jurassic limestones. The consensus of opinion is, then, that the authigenic feldspars and quartz attained their full growth well before normal diagenesis was completed, that is, while the calcareous muds were still unconsolidated.

These European researches are important from several points of view. For students of sediments they indicate a group of facts and problems which have been almost wholly untouched in American laboratories. They seem to throw light on the origin of an important dolomite in the Rocky Mountains. Finally, they furnish proofs of the crystallization of albite and orthoclase at temperatures much lower than have generally been assumed as possible.

In order to complete the summary of the recorded cases, that of the Rocky Mountain dolomite may be noted. It appears to represent the only known American parallel to the peculiar European limestones. The dolomite, named the Waterton formation, was discovered in 1905 by the writer,[9] at Waterton lake just north of the boundary between Montana and Alberta. The formation, at least 60 meters thick, is the lowest exposed member of the great Rocky Mountain geosynclinal and is overlain by 4000 meters of other sediments referred to the Beltian (and probably Cambrian) series. Many hundreds of meters of still younger strata overlay those rocks before the composite geosynclinal was finally upturned, in the Tertiary era. The whole sedimentary cover on the Waterton dolomite was probably more than 6000 meters in thickness; yet the dolomite is lacking in signs of metamorphism, even that of the static kind. Igneous metamorphism can be excluded with equal definiteness.

The dolomite is chiefly composed of carbonate grains, which average about 0.02 mm. in diameter and seldom reach 0.05 mm. in diameter. These grains either interlock or occur as sharp rhombohedra, representing the crystal form so characteristic of true dolomite. Many laminae of the rock, up to 1 mm. or more in thickness, are charged with disseminated grains of glass-clear orthoclase, 0.01 to 0.05 mm. in diameter and without good crystal form. A few irregular grains of quartz, with similar dimensions, are usually associated. Other laminae are very rich in

clumps and lenticular masses of similar, minute crystals of orthoclase, which are interlocked and thus without crystal form (see fig. 7 of the writer's memoir). The clumps and lenses of feldspars regularly enclose many rhombohedra of dolomite. Occasionally in thin section one sees entire laminae, up to 1 or 2 mm. in thickness, composed of about equal parts of orthoclase and dolomite. The feldspar is clearly not of clastic origin.

Analysis of the rock gave the following result, which has been thoroughly checked, the alkalies being subjected to four closely accordant determinations by two different analysts.

SiO_2	30.46	K_2O	5.77
Al_2O_3	6.86	H_2O	1.42
Fe_2O_3	4.53	CO_2	22.55
FeO	1.89		
MgO	10.07		99.95
CaO	16.02	Sp. gr.	2.749
Na_2O	0.38		

The estimated composition of the rock (assuming the iron oxides to represent magnetite) is, by weight:

	per cent		*per cent*
Orthoclase	34.5	$MgCO_3$	21.2
Albite	3.1	$CaCO_3$	28.6
Quartz	6.0		
Magnetite	6.3		99.7

The albite is probably in solid solution with the orthoclase. So far as known by published analysis no other non-metamorphosed dolomite or limestone even approaches the Waterton dolomite in its abundance of the alkalies; feldspar makes up about 40% of its volume.

When reporting on this singular rock the writer was not aware of the relevant observations of Lory, Foullon, Cayeux, and others. In 1912[*] the dolomite remained frankly a puzzle. The theoretical conclusions of Heim, Trümpy, and Foullon are therefore of value in suggesting a partial explanation of the still unique American rock. The Alberta formation differs from the European analogues in having a dolomitic base and in failing to exhibit good crystal form for the feldspar. This anhedral character may be connected with the specially high proportion of orthoclase, the numerous little crystals interfering with one another as they grew in the calcareous mud. The writer has not the slightest doubt as to the chemical origin of the carbonate material nor as to the authigenic nature of the orthoclase; he is inclined to refer the crystallization of the feldspar to the early period when the sediment was not yet consolidated nor deeply buried.

If Foullon, Grandjean, Trümpy, and Heim are correct in regarding these albite and orthoclase crystals as chemically precipitated *at* the sea floor, the formation temperatures must have been well below 100°C. If, on the other hand, the crystals developed only after burial of their calcareous matrices, the formation temperatures were nevertheless comparatively low. The Paris-basin rocks were never buried more than a few hundred meters. Assuming a burial of 1000 meters and the present thermal gradient (3° per 100 meters of depth), the formation temperatures could hardly have reached 100°. Though the burial was deeper for other cases (Alberta and the Alps), formation temperatures of much more than 200° cannot well be assumed, even if the crystallization were delayed until the maximum sedimentary covers were completed.

In any case the Paris-basin limestones apparently prove that albite, orthoclase, and quartz are crystallized in calcareous muds at temperatures lower than 100° and probably lower than 70°. Since this paper was practically completed for publication, it was found that Doelter[10] had already assumed a formation temperature of 'perhaps 100°' for the potash feldspars of the Paris-basin Chalk.

The lowest recorded temperature at which orthoclase has been artificially prepared is 300°. That feat was accomplished by Chrustschoff,[11] who developed both orthoclase and quartz in an aqueous solution of dialyzed silica with alumina and caustic potash, kept for several months at the temperature mentioned. Friedel and Sarasin[12] using analogous solutions in bombs, produced albite, orthoclase, and quartz in much shorter periods, but they employed a temperature of 500°. These experiments suggest that time may be one of the important factors aiding the crystallization of alkaline feldspars at very low temperatures.

What the exact chemical conditions for these noteworthy precipitations in calcareous mud were, is a question now impossible to answer. Perhaps the abundance of lime in the muddy matrices affected the solubility of the alkalies in sea-water;[13] one should further consider the possible influence of decaying animal matter on the various solubilities involved. The astonishing concentration of potash feldspar in the Waterton dolomite accentuates the difficulty of the problem.

[1] Heim, A., *Beitr. geol. Karte Schweiz*, N. F., Lief **20**, 1916, (514, 543, 561, 567).

[2] Trümpy, D., *Ibid.*, Lief., **46**, 1916, (83, 108).

[3] Kaufmann, F. J., *Ibid.*, (*Ser.* 1), Lief., **24**, 1886, (583).

[4] Lory, C., *Paris, C.-R. Acad. Sci.*, **103**, 1886, (309).

[5] Lory, C., *Ibid.*, **105**, 1887, (99).

[6] Foullon, H. B., *Sitzber. Wiener Ak.*, **100**, Abt., 1, 1891, (162, 169).

[7] Cayeux, L., *Paris Mém, Soc. Géol.*, **4**, No. 2, 1897, (259, 279, 303, 432–434). F. Grandjean, *Paris, C.-R. Acad. Sci.*, **148**, 1908, (723), holds that these potash feldspars described

by Cayeux have the optical properties of microcline. Termier found authigenic albite crystals in the Flysch limestone of Briançonnais. Compare Rosenbusch, H., *Elemente der Gesteinslehre*, 3te Aufl., Stuttgart, 1910, (520). "Authigenic" means "formed *in situ*."

[8] Grandjean, F., *Paris, C.-R., Acad. Sci.* **148**, 1908, (723).

[9] Daly, R. A., *Geol. Surv. Can., Memoir* No. 38, 1912, (50).

[10] Doelter, C., *Handbuch der Mineralchemie*, Dresden, Bd. 2, 2te Hälfte, 1915, (556).

[11] Chrustschoff, K., *C.-R. Acad. Sci.*, **104**, 1887, (602).

[12] Friedel, C., and Sarasin, E., *Ibid.*, **92**, 1881, (1374).

[13] Cf. Zschimmer, E., in C. Doelter's *Handbuch der Mineralchemie*, **1**, 1912, (910).

THE INTERFEROMETRY OF SMALL ANGLES, ETC. METHODS BY DIRECT AND REVERSED SUPERPOSED SPECTRA

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Communicated September 21, 1917

1. *Introductory.*—It occurred to me that a number of the methods treated in my papers on direct and reversed spectrum interferometry might be used directly for the measurement of small angles and possibly of the distance of the source of light. Such a procedure would have an apparent advantage, at least theoretically, of not calling for the preliminary superposition of the images of distant objects, as the superposition is inherent in the method itself. But there are large constants involved which make the result very problematical, unless these constants can be removed by a compensator. It is very questionable, moreover, whether, appreciable interferences can occur, and another difficulty which hampers the method is the decrease in the size of objects as their distances increase. A progressive investigation with the object of ascertaining to what degree the experiment is feasible is nevertheless worth while. It will be convenient therefore first to develop the methods without reference to the ulterior conditions which limit the interferences and this method has been pursued.

2. *Method with Prism.*—Figure 1 is a sketch of one of the methods in which S is the distant source of light, from which rays d and d' strike the mirrors m and n, are thence reflected to the silvered sides of the right angled prism P. After leaving it the rays enter the spectro-telescope at T in parallel. If the proper angles are selected the prism P may be replaced by one of any angle or by a reflecting grating.

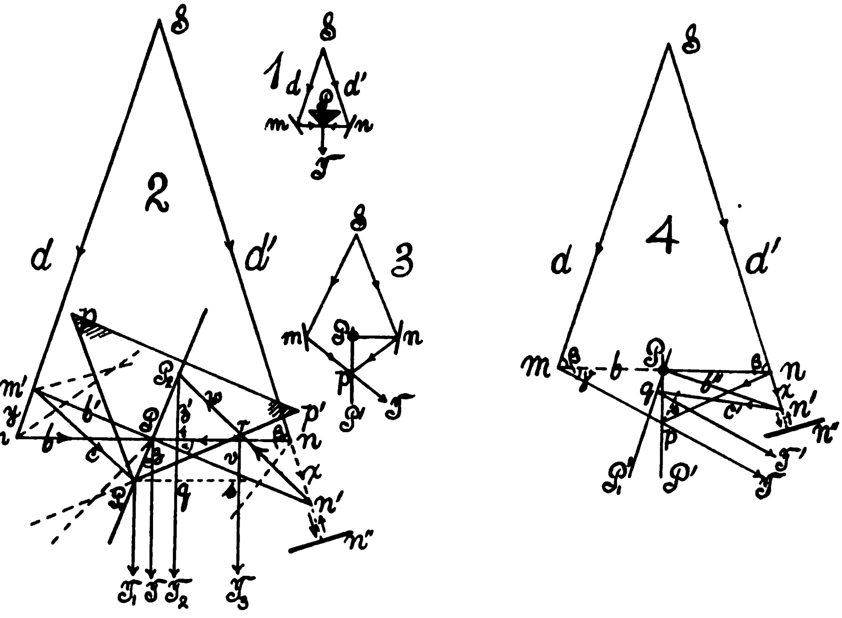
Suppose now the system mPn is securely attached to a rigid metallic beam or rail capable of rotating around a vertical axis at its center (P). This is indicated in figure 2 where the direction of rays and the normals

* Advance note from a Report to the Carnegie Institution of Washington, D. C.

of mirrors have been drawn and where the angle of rotation α has placed mPn into the position $m'Pn'$. The result is that a part y of the ray d is cut off on the left side and a part x added to the ray d' on the right side, so that the path difference which may be assumed to have been zero originally is now appreciably incremented, but not symmetrically for both sides.

It may be shown however that the rays $n'P_2T_2$ and $m'P_1T_1$ still enter the telescope in parallel and that therefore the conditions of interference have not been disturbed. This is the interesting feature of the method.

It will contribute to a more adaptable design of the apparatus for



general interferometry, if the ray Sn'' may also be reversed by reflection parallel to itself from a normal mirror n'', allowing a small lateral offset, similar on both sides for clearance of the mirrors. Again half silvers may be used at m and n for transmission and reflection, which method is probably best. These details will here be disregarded. If small angles are to be measured the direct ray method is enormously more sensitive.

3. Equations.—To derive the equations certain intercepts of the rays figure 2 in addition to x and y, may be defined. $P_1\ P_2$ is the trace of the vertical plane of symmetry of the right angled prism, if rotated at an angle α to the right. In this case the reflected ray $n'P_2q$ on the

right corresponds to the reflected ray $m'P_1$ on the left both terminating in the common wave front P_1qs before entering the telescope.

$$
\begin{aligned}
\text{Let } n'P_2 &= c' = b \sin\beta \,/\, \cos\alpha \sin(\beta-\alpha)\\
m'P_1 &= c = b \sin\beta \,/\, \cos\alpha \sin(\beta+\alpha)\\
P_2t &= z' = b \sin\alpha \sin\beta \,/\, \sin(\beta-\alpha)\\
tq &= z = b \sin\alpha \sin\beta \,/\, \sin(\beta+\alpha)\\
\text{and } nn' &= x = b \sin\alpha \,/\, \sin(\beta-\alpha)\\
mm' &= y = b \sin\alpha \,/\, \sin(\beta+\alpha)
\end{aligned}
$$

since the orginal angles at the ends of the base are β and the rotation α. The angles between incident and reflected rays are respectively $\beta - 2\alpha$ at n', $\beta + 2\alpha$ at m', $90° - 2\alpha$ at P_2 and $90° + 2\alpha$ at P_1. Finally b has changed to b' on the left and b'' on the right.

The rays however do not reach the plane of symmetry but are reflected by the faces of the right angled prism and this may be sketched in apart from size, in the rotated position (angle α) at P_1pp'. The path of the reflected rays from n' is now $n'rs$ and from m', $m'P_1$, before they meet in the common wave front P_1qs. Hence the intercepts

$$
\begin{aligned}
rP_2 &= w = (z + z') \,/\, \cos\alpha\,(\sin\alpha + \cos\alpha)\\
rs &= v = (z + z')\,(\cos\alpha - \sin\alpha) \,/\, (\sin\alpha + \cos\alpha)
\end{aligned}
$$

will enter in treating the path differences. On the left the rays have not been disturbed.

If we take the direct case first the orignal path difference SnP and SmP may be regarded zero or n and m in the same phase. On rotation therefore (angle α) the path difference is equivalent to the equation

$$n\lambda = c' - c - (w - v) + x + y$$

If the above equivalents are inserted, this equation though very complicated, may to the second order of small quantities, be reduced to a form which, since α and $90° - \beta$ are small angles for practical purposes may be abbreviated to (n, order of interference).

$$n\lambda = 2b\alpha - 2b\alpha^2 + 2b^2\alpha \,/\, d$$

The three terms correspond to the xy, wv and cc' effect.

In the case of reversed ray (fig. 2) we may consider the points m' and n' in the same phase. Hence the original path difference ($\alpha = 0$) is $x - y$. The path difference after rotation $c' - w + v - c$. The total change of path difference due to rotation is thus given by

$$n\lambda = c' - c - (w - v) - x + y$$

This differs from the preceding by the deduction of $2x$. The rays again terminate in the common wave front P_1qs to enter the telescope. Here the long rigorous equation reduces practically to

$$n \lambda = 2 b \alpha^2 - 2 b^2 \alpha / d$$

The wv effect predominates, the cc' effect is intermediate and the xy effect very small if d is large, as already instanced. If $\alpha = 1°$, $b = 1$ meter, $d = 1$ kilom., $\lambda = 6 \times 10^{-5}$ cm., the three terms of the first equation give respectively 6×10^4, 10^3, and 60 fringes.

To the first order of small quantities the equations may be written, $n \lambda = 2 b \alpha$ (cosec β + cot β) and $n \lambda = b\ 2\ \alpha \cot \beta$. They may also be obtained geometrically by letting fall a normal from n' to the near prism face, prolonging T_3 backwards and using the isosceles triangle obtained. In this case $x + 2 (c' - w) \cos^2 (45° - \alpha) - (z' - z) + y - c$ is the path difference and reduces to the above equation. So long as the nose of the prism is near the axis of rotation, the equation of first order quantities need not be modified.

A very essential correction however is still needed. In the practical apparatus the mirrors m and n rotate on a rigid radius, b, whereas in the diagram (if $\delta = \beta - \alpha$ and $\sigma = \beta + \alpha$), b elongates on the right and contracts on the left to $b'' = b \sin \beta / \sin \delta$, and $b' = b \sin \beta / \sin \sigma$. Hence the mirrors on the right and left are displaced normally by $(b'' - b) \cos \beta / 2$ and $(b - b') \cos \beta / 2$. The path difference introduced is thus $(b'' - b) (\cos \alpha + \cos \delta) + (b - b') (\cos \alpha + \cos \sigma)$ which to the first order of small quantities may be written $2 b \alpha (1 / \sin \beta - \sin \beta + \cot \beta)$. If this quantity is deducted from the right of the above equation for path difference and direct rays there remains simply $\eta \lambda = 2 b \alpha \sin \beta$. This therefore is the equation to be used in interpreting the observations so that generally $2 \Delta N \cos i / \Delta \alpha = 2 b \sin \beta$ where $i = \beta / 2$ for the micrometer at n.

In the case of reversed rays the conditions on the left remain the same as before; whereas on the right the mirror is set at an angle $\beta / 2$ to the rail. Hence the normal displacement is $(b'' - b) \sin \beta / 2$ and the angle of incidence $90° - (\beta / 2 - \alpha)$. Thus the full path difference here to be deducted is $(b'' - b) (\cos \alpha - \cos \delta) + 2 (b - b') (\cos \alpha + \cos \sigma)$ which to the first order of small quantities reduces to $2 b \alpha \cot \beta$. Hence the equation for reversed rays is simply $2 \Delta N / \Delta \alpha = 0$ and we have an interesting appearance of terms of the second order only, which I will here omit. In general glass paths may be compensated at pleasure.

The *observations* made with the present apparatus, though quite interesting, are beyond the purpose of the present note. The two

corresponding rays d and d' were obtained by cleaving the white beam from a collimator symmetrically, by a knife edged prism with silvered faces.

4. *Interferences from Rough Surfaces.*—The question now at issue is whether these interferences can be retained when the collimator is removed and the light comes directly from a ground glass surface or a Nernst filament. The spectrum fringes go at once when the slit is widened. Not so the achromatic sets. After obtaining this result with sunlight and ground glass, I replaced both by the light from a Nernst filament, under the impression that ground glass might, to a small degree, behave like plate glass. Having produced the achromatics as usual with the collimator, its objective was first removed and it was then seen that the two washed slit images were no longer superposed. Bringing the images together (by rotation of the mirror n around a vertical axis), a position was soon found in which the achromatic fringes appeared brilliantly in a white field, quite out of focus. The slit could now be widened or removed altogether, but the fringes persisted though with less brilliancy. It is thus possible to obtain these fringes directly from a Nernst filament or a narrow vertical strip of sunlight. They are so mobile with changes of Δ N and Δ α, that to find them it is necessary first to produce the spectrum fringes with collimator and spectro-telescope, then to find the achromatics on removing the spectroscope, next to remove the objective of the collimator and adjust for superposed images and finally to remove the slit. They practically cover the whole width of the washed slit image with streamers extending laterally into the glare some five times further. Slit images may even be slightly separated while each alone retains the achromatic fringes, a rather puzzling phenomenon. One may note that the slit images here are not reversed.

Experiments made as to the nature of these achromatic fringes showed that they are probably Fresnellian interferences. To prove this the objective of the collimator was removed and strong fringes obtained by passing the two vague images of the slit gradually over each other, horizontally. The fringes in this motion passed from horizontal maxima of size gradually to vertical hair lines as the images slid from contact of their nearer edges to contact of their further edges. The coarse fringes were even strongly present in the narrow dark gap between slit images prior to contact. The telescope was now focussed on the slit so that sharp linear images appeared. The fringe then vanished, but it appeared that the coarse fringes corresponded to coincident sharp slit images when observed out of focus, and the fine hair lines to sharp slit images far apart also seen out of focus. The whole

phenomenon thus depends on the distance apart of two sharp lines of light and the interferences are observable before or behind their focal plane.

5. *Reversed Rays.*—If the necessary excess of path on the right is compensated by a glass column on the left (usually 10 or 15 cm. long), the spectro-telescope on adjustment shows a field of concentric *half* ellipses, all terminating in a vertical axis. These interesting phenomena are unfortunately not available for measurement as the terminator is not sharp enough and as their motion is necessarily sluggish in view of the large excess of glass path on one side. The achromatic fringes are not producible. For practical work the glass path must be replaced by an air path obtained by aid of an *offset* consisting of two pairs of parallel plates at right angles to each other. These not merely compensate without changing the direction of rays, but on rotating the plates as a whole around a horizontal and a vertical axis, they additionally serve for producing ellipses of any size and of centering them. Achromatie fringes are now brilliantly producible. The variety of observations made will however have to be given elsewhere.

6. *Plate Method.*—In view of certain difficulties encountered in the use of reflecting prisms, in particular the loss of rays at the edge, the method of figure 3 enlarged in figure 4 was devised. In this the prism is replaced by a half silver plate PP'. Hence the rays issuing at S and reflected by the opaque mirrors at m and n, are thereafter respectively transmitted and reflected by the half silvered plate PP' and then reach the spectro-telescope at T together. When the path differences are sufficiently equal, elliptic interference fringes will be seen in the spectrum. When first found they are usually very fine straight lines; but they may be rectified by plate compensators in the beams d and d' or mp and np, though the operation is not easy. Leaving these details for further consideration elsewhere, the procedure for angular measurement may advantageously be treated here. For this purpose the half silver P and one opaque mirror, n for instance are mounted on a rigid bar with an axis at P. The other mirror m is to remain fixed. If the bar is now rotated over a small angle α (fig. 4), the mirror at n is displaced to n' and the ray Sn prolonged (intercept x) is now reflected from n' to q and thence along T' into the spectro-telescope, parallel to its original direction or to the other ray mp. Hence the interferences remain intact but many fringes pass during the transfer. The *persistance* of parallelism is easily seen to be the essential feature of the method.

To control the fringes either the mirror at n (or at m) may be displaced on a micrometer screw normal to itself, or the half silver plate at P may be displaced parallel to itself. If the angle of incidence at n is i and the normal displacement of n is e the path difference intro-

duced will be $2e \cos i$. Similarly if the normal displacement of the plate P is e' and the angle of incidence i', the path difference will be $2e' \cos i'$.

As in the preceding experiment the mirror at n may be a half silver, so that the ray, d, passes through it and may then be returned in its own path by a mirror at n'', on a fixed standard but provided with a micrometer. The displacement of this mirror parallel to itself over a distance e, introduces the path difference $2e$, while during this motion the rays np or $n'q$ are now stationary. Beams of light do not pass through each other and the interferences are kept at full intensity throughout. The glass path at n compensates the glass path PP' The air path excess $2nn''$ on the right must be specially compensated by an offset in d, as explained above.

7. *Equations.*—The rigorous equations for this case are cumbersome. If in figure 4, m and n are in the same phase and Pp is symmetrical, there will be no path difference at p. When Pn is rotated over an angle α into Pn', the path on the right becomes $nn' + n'q + qs$ while (ps, wave front) the path on the left remains mp as before. The path difference is thus the difference of these quantities to which however the increased glass path at PP' would have to be deducted. If the angle SnP is β and Pnp γ, the values of the branch paths may be found to be (since $nP = mP = b$) is $\beta - \alpha = \delta$ and $\gamma - \alpha = \tau$

$$mp = np = b \,/\, \cos \gamma$$
$$nn' = b \sin \alpha \,/\, \sin \delta$$
$$n'q = b \sin \beta \,/\, \sin \delta \sin \tau$$

Hence qs and the path difference are complicated expressions which need not be inserted here.

If α is small, so that differential expressions may be introduced, the rigorous equation (to an approximation of the second order in α) is finally equivalent to $n\lambda = b\alpha\,(1 + \cos(\beta - \gamma))\,/\sin\beta$. If $\beta = 90°$, $n\lambda = b\alpha + p\alpha\cos\gamma$, where p is the distance Pp. The same expressions may be obtained geometrically by prolonging $n'P$ and $T'q$ and treating the isosceles triangle produced.

For the case where the ray Sn prolonged returns on itself as from n'', in figure 4, n being a half silver plate, the quantity $nn' = 2x = 2\,b\,\alpha\,/\sin\delta$ must be deducted. Hence $n\lambda = b\alpha\,(1 - \cos(\beta - \gamma))\,/\sin\beta$. When $\gamma = 0$, this equation coincides with the case of the prism method apart from the factor 2.

It is finally necessary to apply the correction for the occurrence of a constant radius of rotation, whereby the mirror n is both rotated and displaced. If the distances $Pn = b$ and $Pn' = b''$, the normal displacement is $e\,(b'' - b)\cos(\beta - \gamma)\,/\,2$. The angle of incidence being

$(\beta + \gamma) / 2 - \alpha$, the corresponding partial path difference works out as $(b'' - b)$ $(\cos (\beta - \alpha) + \cos (\gamma - \alpha))$; or finally in full as $b\ \alpha\ (\cos^2 \beta + \cos \beta \cos \gamma) / \sin \beta$. Subtracting this from the first equivalent of $n\ \lambda$ above, the equation to be used for non-reversed rays becomes $n\ \lambda = b\ \alpha\ (\sin \beta + \sin \gamma)$ when α is small. As the angle of incidence at the mirror at n is $(\beta + \gamma) / 2$, and if $\Delta\ N$ is the displacement of this micrometer, the practical equation is thus $2\ \Delta\ N \cos (\beta + \gamma) / 2 = b\ (\sin \beta + \sin \gamma)\ \Delta\ \alpha$.

For reversed rays the normal displacement is $(b'' - b) \sin (\beta - \gamma) / 2$ and the path difference for small α therefore $b\ \alpha \cot \beta\ (\cos (\gamma - \alpha) - \cos (\beta - \alpha))$. Subtracting this from the corresponding equivalent of $n\ \lambda$, the equation for reversed rays is thus $n\ \lambda = b\ \alpha\ (\sin \beta - \sin \gamma)$ or in the practical form as the incidence is now zero, $2\ \Delta\ N = b\ (\sin \beta - \sin \gamma)\ \Delta\ \alpha$.

All equations contain the distance of the remote object at S, in $\sin \beta$, so that d occurs as a second order quantity.

Observations (also to be omitted here) were made in great variety with this apparatus, a knife edge prism at S cleaving the white beam from a collimator. To obtain strong full spectrum ellipses, the rays T and T' must not only be parallel but interpenetrate so far as possible. If p and q are a few millimeters apart all fringes vanish. For this adjustment the compensator is again convenient and for reversed rays it must be of the offset type described. One may notice that the slit images are here mirror images of each other. Nevertheless brilliant achromatics may be obtained even from rough surfaces, if narrow, by the succession of operations given above. Both these and the spectrum fringes are available for measurement. The achromatics are most serviceable if tranverse to the slit image as they then rise and fall with the play of the micrometer. To obtain them, the center of ellipses must be placed in the vertical through the telescopic field; but above or below it. In other words the spectrum fringes are to be horizontal. Even when (as in the preceding section in view of the half ellipses) horizontal spectrum fringes are precluded, the achromatic fringes will be found by adjusting as if the former were to appear. The best achromatics consist of but one or two fringes, sharply in black and white, with three or four much fainter fringes on either side. These occur frequently, but how to differentiate them systematically from the other groups of 10 or 20, more nearly uniform and therefore less serviceable fringes, I have yet to learn. All must be treated with caution, however, for they move through the field of the telescope so rapidly that if lost it is usually expeditious to seek for them again through the spectrum fringes.

VOLUME 3 DECEMBER, 1917 NUMBER 12

PROCEEDINGS

OF THE

NATIONAL ACADEMY OF SCIENCES

OF THE

UNITED STATES OF AMERICA

Publication Office: Williams & Wilkins Company, Baltimore
Editorial Office: Massachusetts Institute of Technology, Cambridge
Home Office of the Academy: Washington, D. C.

Entered as second-class matter at the Post Office at Baltimore, Md., under the Act of August 24, 1912

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PROCEEDINGS
OF THE
NATIONAL ACADEMY OF SCIENCES

Volume 3 DECEMBER 15, 1917 Number 12

INCOMPATIBILITY OF MUTANT RACES IN DROSOPHILA

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Communicated by C. B. Davenport, October 17, 1917

Perhaps the most characteristic difference between ordinary mutant races in the laboratory or experimental garden, and incipient species in nature, is the difference in their ability to hybridize. Mutant races usually exhibit complete inter-fertility, while species, as a rule, are inter-sterile. If this distinction, based on fertility of mutants, were found to be a fundamental one (i.e., of universal application) it would offer a serious objection to the hypothesis of evolution through mutations, but, conversely, if it were not found to hold universally the objection would be removed or greatly minimized. The accompanying observations seem to indicate that the distinction does not hold universally.

In our cultures of Drosophila two cases have arisen in which mutant races exhibit an incompatibility that is indistinguishable, so far as we can see, from that found in nature between distinct species.[1] One of our cases appeared in cultures of Drosophila virilis at Cold Spring Harbor, the other in cultures of Drosophila melanogaster (ampelophila) at Columbia University.[2] Each case involves two mutants (apparently allelomorphs) that either refuse to cross or else give sterile hybrids—a situation comparable to that in the familiar case of the ass and horse. The essential data from out experiments are as follows:

1. *In Drosophila Virilis.*—Among eight sex-linked mutant characters in this species are two called 'glazed' and 'rugose,' that appeared independently, but almost simultaneously, over a year ago. Pure stock of each race breeds readily, and in this form both have been kept in the laboratory for over fifteen generations; likewise, each has been crossed with several other mutants as well as with the normal, and has shown a high degree of fertility in all cases. But when mated together their

behavior is very different; 'glazed' females by 'rugose' males have entirely failed to give offspring, and the reciprocal cross, although easily obtained, gives sterile hybrids (i.e., sterile females—these being the only hybrid individuals). In other words, it has been impossible thus far to get an F_2 generation from the cross, no matter how the mating was made.

No accurate record has been kept of the earlier experiments with the two forms, but at least a score or more of matings were made at different intervals during six or eight generations. Among the more recent attempts, in which a record was kept of all F_1 matings, the following may be cited: One experiment began with about a dozen reciprocal matings between glazed and rugose, using several flies in each bottle. At least three of these, in which glazed males and rugose females were used, gave many offspring. Fourteen large, vigorous females were selected from the latter and mated singly to several males—eight to rugose males, six to normal males. They were all given the best possible cultural conditions and treated exactly like other mutants of various kinds that were mated at the same time. Practically all of the latter gave abundant progeny, but not a single offspring appeared in any of the fourteen bottles containing glazed-rugose hybrids, though the flies lived for a long time (much longer than the ordinary length of one generation). Another experiment was made in a different way. This time females heterozygous for glazed were mated to rugose males, and females heterozygous for rugose were mated to glazed males. These gave daughters half of which were heterozygous for *either* rugose or glazed, and half of which were heterozygous for *both* rugose and glazed. Forty-five females from such cultures were mated singly to various males (mostly normal). Twenty of the females were from the first mating, twenty-five from the second. Out of the twenty females nine gave abundant offspring and eleven gave none; out of the twenty-five females fourteen gave offspring, eleven gave none; or, out of the total of forty-five, twenty-three gave offspring and twenty-two did not. In three cases the female died before the records were made, so the numbers are subject to that much error, but in all of the others the flies lived to twice or three times the age at which offspring are ordinarily produced. Of the twenty-three females that did give progeny, every one proved to be heterozygous for only one of the characters concerned—i.e., those from the first mating were heterozygous for rugose but not glazed; those from the second mating were heterozygous for glazed but not rugose—leaving no doubt that the sterile females were those carrying both glazed and rugose.

It is difficult to avoid the conclusion that we are dealing here with a complete, or at least a high degree of incompatibility. That it is spe-

cific for the two mutants in question, and is not an ordinary case of sterility, is shown by the fact that both mutants are fertile and give fertile hybrids with other mutants and with the normal. Furthermore, the sterility of the hybrids in question is in no way dependent upon the males used, at least so far as normal, rugose and glazed are concerned, for all three kinds have been tried.

2. *In Drosophila Melanogaster* (*Ampelophila*).—The case found in *D. melanogaster* may be summarized as follows: A mutant form, 'notch' wing, crossed to the fully fertile mutant 'facet' eye, gave F_1 hybrids which were completely sterile with the common parent type and with one of the mutant forms, facet.

The sex-linked mutant, notch, is principally characterized by a notch of definite type but variable extent in the tip of the wing. Notch is dominant in the female, and in the male acts as a lethal. The locus of notch was found to be at approximately 2.6.[3] In an attempt to locate the gene for notch more accurately, it was decided to use in a linkage experiment the recessive mutation facet[3] whose locus had been found to be at approximately 2.2, very close to the supposed position of notch. Accordingly, when notch reappeared (December, 1915), the single notch female, which appeared as a mutation in a pedigreed culture, was mated to yellow facet males (yellow is a recessive body color whose locus is at 0.0).

The F_1 generation furnished a surprise, for every one of the daughters that was notch was also facet! Two explanations of this peculiar 'compounding' of notch and facet are open to us: either the notch mutation is a deficiency[4] for facet, or the two mutant forms are allelomorphic.

It had been the intention to backcross a number of the expected F_1 notch females to yellow facet males in order to secure sufficient linkage data upon the relative positions of these factors. When it was found that facet showed an apparent dominance with notch this plan presented difficulties. Nevertheless, nine of the notch-facet daughters were mated to yellow facet males as originally planned, while six more were mated to yellow notch-facet males. *Not one of these fifteen females produced offspring.* It was then realized that this relation was unusual, and a second effort was made to secure offspring. Fresh males were given to the remaining females (three having died) and they were transferred to fresh culture bottles. The discarded parent bottle was also rescued and three additional notch-facet females—the last to hatch—were secured and likewise mated to yellow facet males. No offspring were produced. Since the death of these females 'without issue' would mean the loss of the mutation and would shut the door on the solution

of the problem, a third, but equally unsuccessful, effort was made by transferring such females as were still alive to fresh culture bottles.

So many matings were attempted that it is impossible to regard their failure as accidental or as due to poor cultural conditions. It must be considered established that the fertility of the notch-facet hybrid was of a different order from that of either parent race.

Notch has reappeared on at least three other occasions, and in one of these cases the facet test has been made. The new notch-facet test gave the peculiar notch-facet compound, but this compound was fully fertile. The original facet cross had been made to a notch which arose by a separate and independent mutation, and was probably not identical with the notch of the later test but was an allelomorph. The original notch seemed to be a somewhat more extreme type than the others. Furthermore, a parallel case (in D. melanogaster) has been found, in which two allelomorphs that differ little in appearance differ markedly in their fertility relations. The mutation lethal 2 gave an aberrant linkage result[8] which led to tests for deficiency by mating lethal 2 females to males of mutations whose loci were known to be close to that of lethal 2. In the test by the recessive 'club' the same apparent dominance was found as in the case of notch-facet;—that is, females heterozygous for lethal 2 mated to club males gave half the daughters club and half normal, and it was easy to demonstrate that the ones that were club were also the ones that were lethal 2, for these lethal 2-club hybrids were fertile. In other crosses not involving club very rarely a male having the lethal 2 gene was able to hatch, and these males, which showed all the characteristics of club, were completely sterile.

3. If we imagine such cases as these to occur in nature it is evident that they might be of evolutionary significance. For instance, suppose that the two mutants 'glazed' and 'rugose' of D. virilis appeared in the wild state. Their establishment would depend, of course, upon the viability of the mutants, and in this particular instance one of them (glazed) would probably be eliminated, although the other, judging from its vigor, would stand a good chance of surviving. But the consideration of viability need not affect the case as an illustration, for it must be assumed *a priori* that any variants, to be of evolutionary significance, must be viable. Supposing, then, that the two mutants in question appeared as viable forms in nature; the result would be a composite species consisting of three types, two of which were fertile with the third but infertile with one another. If for any reason the third type (in this case the normal) were to be eliminated, the first two would become distinct species, even though they differed in only one or two external characters to begin with.

This scheme involves an obvious difficulty, in that it assumes the elimination of what would probably be the most numerous and widespread of the three types—the normal or parent type—while the probabilities greatly favor the elimination of one or both mutant types. If we slightly alter the conditions of the case, however, the difficulty is reduced. Supposing that instead of the two mutant races arising from the normal stock, only one (a) has this origin, while the second (b) is derived in turn from the first one, and that, instead of the two mutants being incompatible with each other, the second is incompatible with the normal. In this case the first mutant (a) acts simply as an intermediate step between the two incompatible forms, normal and mutant (b), and if it is eliminated the other two stand as distinct species. Here we have a case which differs in no essential respect from the former, but which is less improbable in that it involves the elimination of one of the mutant forms instead of the more widespread parent stock.

The same sort of scheme applies equally well to the case in D. melanogaster, so far as illustrative purposes are concerned. In this particular instance it happens that one of the mutant characters (notch) is a dominant that cannot be obtained in a homozygous condition because of its lethal effect, and hence the mutant could never make a pure race; but there is no necessary connection between lethal action and incompatibility.

In connection with these experimental data it is interesting to note two other lines of evidence, taken directly from wild flies. First, with respect to the question of whether or not mutant races can survive in nature, it may be recalled that the possibility of such survival is practically demonstrated by such cases as that described by Sturtevant in Drosophila repleta.[5] Two forms of this fly are found existing side by side in the wild state, and one of them is a typical sex-linked recessive to the other. There is every reason to believe that one of these has arisen from the other by mutation, and bears the same relation to it that any one of the above mutants does to the normal of its species. If we suppose that either of these forms in D. repleta gives rise to a third form that is incompatible with the other, and then becomes eliminated itself, we have all of the necessary steps in the formation of a new species; and the case differs in no essential respect from the hypothetical ones outlined above.

Another line of evidence (also from D. repleta) bears more especially upon the question of incompatibility in wild races. Here, instead of two wild forms that interbreed freely but are unlike in appearance, we have two varieties that refuse to interbreed, but are extremely similar in

appearance. They are so similar, indeed, that it is very doubtful whether they would have been recognized, even as varieties, if cytological examination had not shown them to differ in respect to their chromosomes,[6] and if they had not been bred in the laboratory to test their compatibility. Here is an apparently clear case of incipient species formation. It seems practically certain that these two varieties, or species, have had a common origin, and that their individuality at the present time is due mainly to their incompatibility. Either of them would, in our opinion, pass for a mutant race of the other; and it is not difficult to imagine that they might have become differentiated from one anther in some such way as that outlined above; i.e., by mutations that brought about incompatibility. With the incompatibility once established they are now free to diverge more and more until they become clearly differentiated from one another.

4. To recapitulate: The evidence from two cases of incompatibility between mutants in laboratory cultures, together with evidence from what appear to be mutant forms and incompatible varieties in nature, tends to remove one of the most serious objections to the mutation hypothesis, and lays emphasis upon the possible evolutionary importance of mutations involving incompatibility.

[1] In each case the two respective mutants appeared in pedigreed laboratory cultures, leaving no doubt about their being typical 'mutants.'

[2] For additional data on the mutants in D. virilis see Metz, C. W., *Genetics, Princeton*. 1916, 1 (591–607); for data on those in D. melanogaster see Morgan and Bridges, *Washington, Carnegie Inst., Pub.*, No. 237, 1916.

[3] Morgan and Bridges, *op. cit.* 1.

[4] Bridges, C. B., *Genetics, Princeton*, 2 (445–465).

[5] Sturtevant, A. H., *Amer. Nat., Lancaster, Pa.*, **49**, 1915 (190–192). Other cases of a similar sort could be added; this one is used because it is taken directly from a Drosophila.

[6] Metz, C. W., *Amer. Nat., Lancaster, Pa.* **50**, 1916, (597).

ABSORPTION EFFECTS IN THE SPIRAL NEBULAE

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Communicated by W. W. Campbell. October 18, 1917

A study of the negatives of spiral nebulae obtained with the Crossley Reflector has shown that the phenomenon of dark lanes caused by occulting or absorbing matter is much more frequent than had previously been supposed. A paper of considerable length on this subject, in which the evidence is supplied chiefly by half-tone illustrations of seventy-seven spirals, will be published soon by the Lick Observatory. An abstract of that paper follows.

A first division of the evidence is supplied by those spirals which are seen edgewise, or nearly so, and show indubitable evidence of an absorbing lane (cf. fig. 1 for three typical examples of this class). Twenty-nine such objects have been photographed, and the published descriptions of nine additional edgewise spirals, not yet photographed, clearly indicate that they also should be included.

That thirty-eight of the larger edgewise spirals should show clear evidence of bands of obstructing matter must be regarded as establishing that this phenomenon is a very common one, and is probably the rule rather than the exception.

A second division of the evidence is afforded by that large group of spiral nebulae whose principal planes make a slight but appreciable angle with our line of sight. Any actual irregularities due solely to

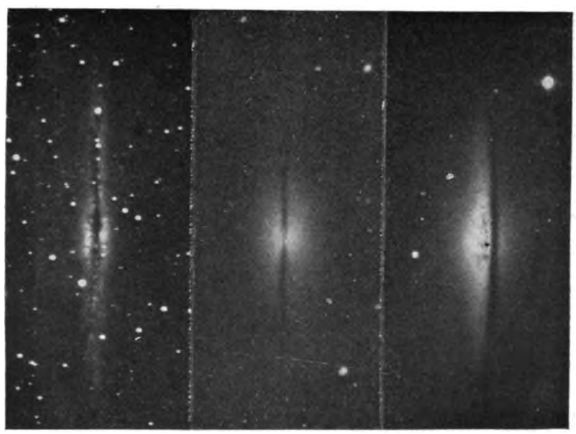

FIG. 1

asymmetry of form in the spiral nebulae should, taking a sufficiently large number of cases, show asymmetrical effects oriented at random with regard to the position of the major axis of the projected (elliptical) images. This, however, is not the case. In the relatively frequent cases where the greatly elongated spirals show any lack of symmetry, such asymmetry is almost invariably with reference to the major axis of the ellipse. This asymmetry manifests itself frequently in 'lanes' prominent on one side of the major axis and faint or invisible on the other, in a fan-shaped nuclear portion, in an apparent displacement of the nebular material on one side of the major axis, or in various combinations of these effects. Fifteen elongated nebulae show prominent dark lanes on one side of the major axis. Contributory evidence is afforded by thirty-one spirals in which the nebular matter is markedly

fainter on one side of the major axis of the projected ellipse, or in which it appears to extend farther from the apparent center on one side of the major axis. A fan-shaped nuclear portion is prominent in thirteen elongated nebulae. Figure 2 shows a typical example of this group.

It is not impossible that these effects may be due to the same general cause which produces analogous effects in our own Galaxy, though there is manifestly a good deal of assumption in postulating the same character of occulting material in the vicinity of objects so different in spectrum, in space distribution, and in space velocity, as the spirals and, for example, the great diffuse nebulosities. It will be of interest, however, to mention some probable intra-galactic manifestations of occulting material.

a. Many diffuse nebulosities show a marked falling off in the number of faint stars in their immediate vicinity. Very faintly luminous or non-luminous matter in the peripheral regions of these nebulae seems to offer the only possible explanation.

FIG. 2

b. The 'Coal Sacks' and other starless regions in or near the Milky Way seem to be best explained as due to the interposition of great expanses of occulting material between these areas and our own position in space.

c. Professor Barnard has described many small starless regions, which he believes to be 'dark nebulae,' and a number of these are available for study on negatives taken with the Crossley Reflector. It is impossible to believe that these are actual 'holes' in the Milky Way. As Campbell has pointed out, the age of these must be of the order of hundreds of millons of years, and the random motions of the stars would long since have obliterated the clear-cut edges, if not the entire phenomenon, if they ever had the character of 'holes.'

d. Similar dark patches are seen projected on the luminous background of many of the giant diffuse nebulosities. It is impossible to conceive that these clear-cut spots are 'holes' extending through a mass of nebular matter for distances measured in light years.

e. About twenty-five spectroscopic binaries are known in which the H and K lines of calcium do not partake at all of the periodic shift shown by the other spectral lines, or give a markedly smaller range of radial velocities. This phenomenon is well explained by the interposition of a cloud of invisible calcium vapor between us and the binary. All but one of these stars are located in or near the Milky Way, and several are in or near dark rifts of the Milky Way.

f. There exists a sidereal arrangement for which no adequate explanation has yet been found—the peculiar grouping of the spiral nebulae about the galactic poles, and the entire absence of these bodies in the Milky Way structure. It has been suggested that occulting matter in the peripheral regions of our Galaxy cuts off from our view all spirals lying in or near our galactic plane, and presumably far outside of our stellar system.

g. There is strong evidence that much absorbing material exists in the outer strata of planetary nebulae.

In the case of the edgewise and nearly edgewise spirals, the evidence for occulting material seems unquestionable. Vacant spaces dividing these objects into two similar and parallel nebular forms would appear to be mechanically inconceivable. For nebulae whose planes make larger angles with the line of sight, the alternative hypothesis that these appearances are mere open lanes encounters several difficulties. Such lanes should show most clearly at the ends of the major axes of the elongated nebulae, for there the foreshortening effect due to the inclination of the nebular planes would be least. But the photographs leave no doubt that these dark lanes are invariably parallel to the major axis, and are most apparent near the ends of the minor axis.

A possible phase effect might be proposed as an explanation of the differences of intensity on opposite sides of the major axis. But such a theory demands that the light of the spirals be due to reflection. Against this theory may be urged:

a. All attempts to detect evidences of polarization in the spiral nebulae have given negative results.

b. The reflection hypothesis demands that the light of the outer portions of the nebulae should come ultimately from a central nucleus, or star, or aggregation of stars of sufficient brightness to illuminate the outer portions, and that these outer portions should in general be fainter than we find them. Many nebulae have no such central condensations, and in others the nucleus is so faint as to be entirely inadequate. Many others show patches of nebular matter in the outer regions of the nebulae which are as bright as or brighter than the central part,—an improbable result on the reflection hypothesis.

If we could observe our own Galaxy from a sufficient distance, it would probably have many resemblances to a spiral nebula (compare Easton's work on the Milky Way as a spiral). The evidence adduced that rings or whorls of occulting matter are of very frequent occurrence in the spirals is a point of great weight in connection with the evidences of similar matter in our Galaxy. In particular, the results may be regarded as bearing very directly on the only hypothesis which seems to explain the peculiar grouping of the spirals: that the invisibility of spiral nebulae in our Galaxy and their scarcity in the regions contiguous to our Galaxy are due to the presence of occulting matter in the outer confines of our stellar system.

THE SYNERGETIC ACTION OF ELECTROLYTES

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Communicated by G. H. Parker, October 20, 1917

Numerous cases are reported in which a mixture of toxic salts is less harmful than either salt used by itself. This has been called *antagonism* since one salt antagonises the action of the other. Theoretically the opposite action may exist, in which one salt increases the toxicity of the other. I suggest that this be called *synergy*. Very few cases of this are reported[1] and in some instances there is difficulty in deciding whether they really belong in this category because we lack data to show what the result would be if each salt acted independently of the other. Such data can be secured only by studying the effect of each salt separately and at various concentrations.

Studies of this sort have been made by me during the past summer at Woods Hole. The marine alga, *Liminaria Agardhii* Kjellm., furnished the experimental material. The behaviour of this plant with reference to antagonistic salts has been studied by Osterhout[2] who employed the method of electrical conductivity for this purpose. This method was used by me in his investigations. As the details of the procedure are the same as in the experiments of Osterhout it is unnecessary to describe them here.

The salts used were the purest obtainable and the distilled water was not toxic to delicate test objects.

As I was primarily interested in the effects of anions the action of a series of sodium salts was investigated. These included the chloride, iodide, bromide, nitrate, acetate, sulphocyanide, sulfate, citrate, and

tartrate of sodium. The effects of the single salts and of various mixtures were studied. Of especial interest are those combinations of salts which show increased toxicity in the mixtures. As an example of these we may take the effects of sodium chloride and sodium citrate.

Solutions of these salts were made of the same electrical conductivity as the sea water (about 0.52M sodium chloride and about 0.58M sodium citrate). On placing tissue in the pure chloride we find that after about three hours the electrical resistance falls to about 10% of the original resistance and there remains stationary. This represents the death point. In the solutions containing citrate we find that before the resistance falls as low as 10% the tissue softens so that it can not be manipulated. Consequently the experiments with citrate are never continued for more than twenty minutes, at which time the resistance of the pure sodium citrate reaches about 17% of the original. The drop in resistance occurs much more rapidly in the sodium citrate than in the sodium chloride.

TABLE 1

PER CENT OF ORIGINAL RESISTANCE

TIME IN MINUTES	100 cc. Cl + 0 cc. CITRATE	75 cc. Cl + 25 cc. CITRATE	63 cc. Cl + 37 cc. CITRATE	50 cc. Cl + 50 cc. CITRATE	37 cc. Cl + 63 cc. CITRATE	25 cc. Cl + 75 cc. CITRATE	0 cc. Cl + 100 cc. CITRATE
	per cent	*per cent*	*per cent*	*per cent*	*per cent*	*per cent*	*per cent*
5	92	67	55	48	47	45	36
10	85	47	39	31	30	29	24
15	82	37	30	24	21	21	20
20	80	30	21	17	16	17	17

The above represents the average of ten experiments.

Table 1 (in which the resistance is expressed as per cent of the original resistance in sea water) shows the fall of resistance in the various solutions. The table shows that at 23°C. (the temperature varied between 22° and 24° during the experiment) the resistance in sodium citrate fell in the course of the twenty minutes to 17% of the original resistance while during the same time in sodium chloride it had fallen to only 80%. In the mixtures intermediate conditions are observed.

The significance of these results is shown more clearly by the figure (fig. 1). Here is plotted the set of values obtained for the fifteen minute curve. The curve, *A*, is the curve of resistances found as shown in the table. The curve, *B*, is the curve of the additive effect which is obtained by diluting the citrate with water and to this adding the comparative dilution effect of the chloride according to the method outlined by Osterhout.[3]

In the case of most cations (such as sodium and calcium) an antagonism curve is obtained but here just the opposite effect is seen. That is to say the chloride and citrate ions neither antagonise each other nor remain without effect upon each other, but the presence of the two ions in some way increases the action of both so that the resistance is much lower at any given instant than it would otherwise be from mere additive effects. It is for this and similar effects that the author proposes to use the name synergy, which is hence the antithesis of antagonism. In

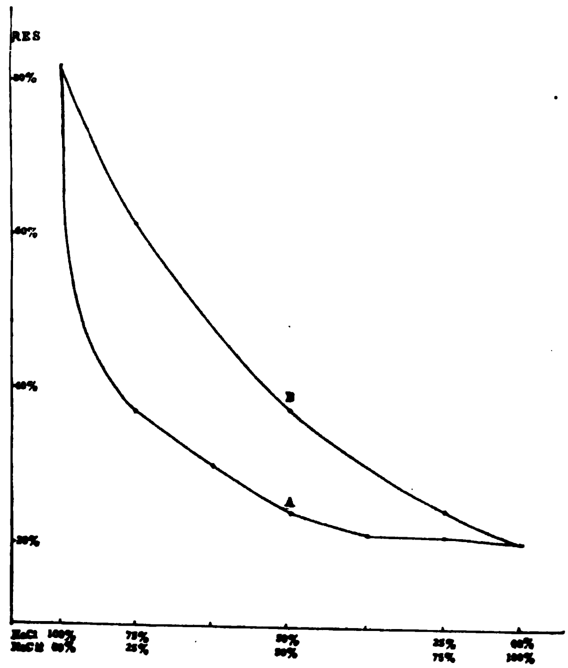

FIG. 1. THE SYNERGETIC ACTION OF ELECTROLYTES

Shown by curves of the electrical resistance of *Laminaria*, after fifteen minutes in sodium chloride, in sodium citrate and in mixtures of these (the proportions are indicated on the abscissae). Curve A, observed values. Curve B, values expected on the supposition that neither salt influences the action of the other (additive effect). Synergy is measured by the vertical distance between the curves.

figure 1, therefore, the synergetic action is that shown by the distance between the curves A and B measured vertically, e.g., the distance AB. Any other set of values (e.g., the five, ten, or twenty minute curves) shows similar results.

That this is not a specific effect for these two salts is shown by like results with citrate combined with iodide, sulfocyanide, nitrate, and sulfate, the data for which will be published later. Certain other experi-

ments also indicate that other combinations of anions other than those in which citrate is used also give synergy but more work remains to be done to establish this point. Citrates, of all the salts tried, certainly give the most pronounced synergy.

Studies concerning the causes of the effects mentioned above and a more complete treatment of the subject are in progress, all of which it is hoped may appear in the near future.

[1] Lipman, C. B., *Bot. Gaz., Chicago*, **48**, 1909, (105); *Centralbl. Bakt., Jena*, **36**, 1912, (390). Loeb, J., *J. Biol. Chem., New York*, **28**, 1916, (175).

[2] Osterhout, W. J. V., *Science, New York*, N. S., **35**, 1912, (112).

[3] Osterhout, W. J. V., *Bot. Gaz., Chicago*, **60**, 1915, (228).

APPETITES AND AVERSIONS AS CONSTITUENTS OF INSTINCTS

By Wallace Craig

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Communicated by R. Pearl, October 18, 1917

The overt behavior of adult animals occurs largely in chains and cycles, and it has been held[1] that these are merely chain reflexes. Many years of study of the behavior of animals—studies especially of the Blond Ring-Dove (*Turtur risorius*) and other pigeons—have convinced me that, though innate chain reflexes constitute a considerable part of the instinctive equipment of doves, few or none of their instincts are mere chain reflexes. On the contrary, each instinct involves an element of appetite, or of aversion, or both.

An *appetite*, so far as externally observable, is a state of agitation which continues so long as a certain stimulus, the appeted stimulus, is absent. When the appeted stimulus is at length received it releases a consummatory reaction, after which the appetitive behavior ceases and is succeeded by a state of relative rest, a state of satisfaction. The appetitive behavior serves to bring about the appeted situation by trial and error. The appetitive state includes a certain *readiness* to act. When most fully predetermined this has the form of a chain reflex. But in the case of many supposedly innate chain reflexes, the reactions of the beginning or middle part of the series are not innate, or not completely innate, but must be learned by trial. The end action of the series, the consummatory action, is always innate. One evidence of this is the fact that in the first manifestation (also, in some cases, in later performances) of many instincts, the animal begins with an *incipient consummatory action*, although the appeted stimulus, which is the adequate stimulus of the consummatory reaction, has not yet been received.

Thus the young dove when learning to drink makes drinking movements while searching for the water; and when its instinct to fly has ripened, it may make feints of flying, flapping its wings vigorously, and even aiming at an objective point, before it has dared to launch into the air. There are all *gradations* between a true reflex and a mere readiness to act, mere facilitation. In many cases the bird needs to *learn* to obtain the adequate stimulus for a complete consummatory reaction, and thus to satisfy its own appetites.

An *aversion* resembles an appetite in that it is a state of the organism characterized by agitation and persistency with varied effort; it differs from an appetite in that it continues so long as a certain stimulus, referred to as the disturbing stimulus, is *present*, but ceases, being replaced by a state of relative rest, when that stimulus has ceased to act on the sense organs. An aversion is sometimes accompanied by an innately determined reaction adapted to getting rid of the disturbing stimulus, or by two *alternative* reactions which are tried and interchanged repeatedly until the disturbing stimulus is got rid of. An example of aversion is the so-called jealousy of the male dove, which is manifested especially in the early days of the brood cycle. At this time the male has an aversion to seeing his mate in proximity to any other dove. The sight of another dove near his mate is an 'original annoyer.'[2] If he sees another dove near his mate, he may follow either or both of two courses of action; namely, (*a*) attacking the intruder, with real pugnacity; (*b*) driving his mate, gently, not pugnaciously, away from the intruder. The instinctive aversion impels the dove to truly intelligent efforts to get rid of the disturbing situation.

Instinctive activity runs in *cycles*. The type cycle, as it were a composite photograph representing all such cycles, would show four phases as follows.

Phase I. Absence of a certain stimulus. Physiological state of appetite for that stimulus. Restlessness, varied movements, effort, search. Incipient consummatory action.

Phase II. Reception of the appeted stimulus. Consummatory reaction in response to that stimulus. State of satisfaction. No restlessness nor search.

Phase III. Surfeit of the said stimulus, which has now become a disturbing stimulus. State of aversion. Restlessness, trial, effort, directed toward getting rid of the stimulus.

Phase IV. Freedom from the said stimulus. Physiological state of rest. Inactivity of the tendencies which were active in Phases I, II, III.

Some forms of behavior show all four phases clearly. In other cases one or other of the phases is not clearly present. When the bird shows appetitive behavior but fails to obtain the appeted stimulus, the appetite sometimes disappears, due to fatigue or to drainage of energy into other channels. But some instinctive appetites are so persistent that if they do not attain the normal appeted stimulus they make connection with some abnormal stimulus; to this the consummatory reaction takes place, the tension of the appetite is relieved, its energy discharged, and the organism shows satisfaction. This is 'compensation' in the sense in which that word is used in psychiatry. The cycles and phases of cycles are multiplied and overlapped in very complex ways. Smaller cycles are superposed upon larger ones. The time occupied by each varies greatly, from cycles measured in seconds to those that occupy a year or even longer.

The successive phases are not sharply separated, Thus, from the last phase of one cycle in a series to the first phase of the succeeding cycle, there is often a gradual rise of appetite; active search for satisfaction does not commence until a certain intensity of appetite is attained. This is what is known in pedagogical literature as 'warming up.' This gradual rise of the energy of appetite is followed (Phases II–III, or II–IV) by its sudden or gradual discharge. The rise and discharge are named by Ellis,[3] in the case of the sex instinct, 'tumescence' and 'detumescence.' They are important phases in the psychology of art, in which sphere they are named by Hirn[4] 'enhancement' and 'relief.' The discharge (Phase II) is also exemplified in 'catharsis' in art and in psychiatry.

All human behavior runs in cycles which are of the same fundamental character as the cycles of avian behavior. These appear in consciousness as cycles of attention, of feeling, and of valuation. This description is true not only of our behavior toward objects specifically sought by instinct, such as food, mate, and young, but also of our behavior toward the objects of our highest and most sophisticated impulses, such, for example, as a symphony concert. The entire behavior of the human being is, like that of the bird, a vast system of cycles and epicycles, the longest cycle extending through life, the shortest being measured in seconds, each cycle involving the rise and the termination of an appetite. This view helps us to understand the laws of attention; for example, the law that attention cannot be held continuously upon a faint, simple stimulus. For as soon as such a stimulus is brought to maximum clearness, which constitutes the consummatory situation, the appetite for it is quickly discharged and its cycle comes to an end. This familiar fact

illustrates the general truth that we, like the birds, have but a very limited power of altering the ebb and flow of our behavior cycles. Cyclical recurrence does not prove that human behavior consists of mere chain reflexes, neither does it prove that the instinctive behavior of birds consists of mere chain reflexes.

Doctor Raymond Pearl read a preliminary draft of this paper and suggested important improvements, for which I express my thanks.

The article of which this is an abstract will appear in the *Biological Bulletin.*

[1] Herrick, C. J., *Introduction to Neurology*, 1915, (61).
[2] Thorndike, E. L., *The Original Nature of Man.*
[3] Ellis, H., *Studies in the Psychology of Sex. III. Analysis of the Sexual Impulse.*
[4] Hirn, Y., *The Origins of Art.*

RAPID RESPIRATION AFTER DEATH

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Communicated by G. H. Parker, October 20, 1917

Various observers[1] have reported that respiration may continue after death but apparently the rapidity of post mortem respiration does not in any of these cases exceed the normal rate. It is therefore of interest to find that the marine alga *Laminaria* in the presence of certain reagents may respire more rapidly after death than in its normal state.

In my experiments the rate of respiration was determined by measuring the output of CO_2 (at 16°C.) by means of suitable indicators added to the solution which had been rendered acid by the respiration of the *Laminaria.* The method has been previously described.[2]

As soon as a determination of the respiration had been made, the solution bathing the tissue was renewed and after exposure for the same length of time the amount of respiration was again determined. In this manner the respiration of the material could be followed and it could be seen whether it was approximately constant before the beginning of an experiment. This constancy was obtained in all the experiments here recorded.

In some cases (acetone 17.4% and alcohol 24.2%) the killing agent extracted from the plant a small amount of pigment which interfered with the color of the indicator.[3] But this difficulty disappeared after the first two periods, as was shown by running pure hydrogen through the solution, after which it returned to the color found in normal sea water plus indicator. This method also showed conclusively that the acid excreted by the plant was CO_2 and not an organic acid.

The methods of killing the tissue were various. Sea water containing anesthetics (made up to the conductivity of sea-water by the addition of concentrated sea-water) was employed in many of the experiments. In this case the respiration was determined for several periods of equal length in sea-water (the solution being renewed after each period). The sea-water was then replaced by sea-water containing anesthetic and the respiration determined after successive equal periods until death ensued, and for some time thereafter.

As it was important to know the time of death as accurately as possible, determinations of the electrical conductivity of the tissue were made by the method of Osterhout.[4] If the electrical resistance of the normal tissue be called 100% it is found that on killing the resistance falls to about 10%. When the resistance has fallen to 15% the tissue is for all practical purposes dead, as there is no recovery when it is returned to normal conditions.

It was found that with sea-water approximately saturated with ethyl bromide the rate of respiration was about doubled. After the tissue was dead (as shown by the electrical resistance) it continued to respire for some time at a rate above the normal. Somewhat similar results were obtained when the sea-water contained 17.4% of acetone (by volume) and was made up to the conductivity of sea-water by the addition of concentrated sea-water. In this case the post-mortem rate of respiration is far above the normal and remains so for about 2 hours after the death of the tissue. This is also the case with sea-water containing 24% (by volume) of ethyl alcohol (the solution being made up to the same conductivity as sea-water).

With sea-water which contained 3.2% formaldehyde, (the free acid neutralized with sodium carbonate and the solution made up to the conductivity of sea-water) it is found that at the end of 100 minutes after the electrical resistance shows the tissue to be dead, the rate of respiration has fallen to normal (before this time the rate is much above the normal).

Experiments were made to ascertain whether different methods of killing would give different results. Tissue was exposed to running tap water for 19 hours (which was longer than was required to kill, as was shown by the electrical resistance). After the exposure, the respiration rate was too small to be detected. It is probable that in this case the rise and decline of the rate of respiration was completed before the end of 19 hours.

The respiration of another lot of material was determined before and after killing by means of exposure to a large volume of sea-water at

35°C. for 70 minutes (the time, as found by determining the electrical resistance, which was required to produce death). At the end of the exposure the rate of respiration in sea-water (at 16°C.) had fallen considerably below the normal. This might be expected, as the oxidases are usually injured or destroyed by heat.

Preliminary experiments in which the electrical resistance was determined showed that the tissue was killed by drying for 135 minutes in strong sunlight in a current of dry air. Pieces treated in this manner were washed for 15 minutes in sea water and the rate of respiration was then determined. It was found to be about five times the normal rate. In the course of about two hours it fell to the normal.

As it is well known that wounding,[5] especially if severe, may cause a considerable rise in the respiration, it appeared advisable in this connection to make such experiments with *Laminaria*. The normal respiration in sea-water was first determined. After the tissue had been finely macerated (by means of a broken Pyrex glass-tube) on tested filter paper, it was rinsed from six to ten times with sea-water to free it of CO_2 and of any liberated pigment. The rate of respiration was then determined in sea-water. It was found that the injury cased a doubling of the rate which at the end of an hour was still above the normal. In this case the time of death could not be determined.

It is therefore evident that a considerable variety of killing agents raise the rate of respiration above the normal and that this increased rate may be maintained for some time after death. The fact that the rate eventually falls below the normal may be due to exhaustion of readily oxidizable materials or to destruction of oxidizing enzymes or to other causes.

That previous observers have not found a post mortem rate of respiration which is greater than the normal is probably due to the fact that the time of death was not accurately known and the observations were not commenced until the rate of respiration had sunk below the normal. It seems reasonable to suppose that if the rate of respiration is raised above the normal by an injurious agent it will not suddenly drop below the normal at the moment of death, but will decline gradually, as is the case in these experiments.

Summary.—The respiration of *Laminaria* may be much greater after death than in the normal condition. This is the case when it is killed by alcohol, acetone, ethyl bromide or formaldehyde as well as by wounding, drying and other means.

[1] Cf. Warburg, O., *Ergebn. Physiol.*, **14**, 1914, (313).

[2] Haas, A. R., *Science, New York*, N. S., **44**, 1916, (105).

[3] This did not occur with low concentrations of these substances.

[4] Osterhout, W. J. V., *Science, New York*, N. S., **35**, 1912, (112); *Bot. Gaz., Chicago*, **61**, 1916, (148). The determinations referred to in this paper were made in part by Professor Osterhout and in part by me.

[5] Richards, H. M., *Ann. Bot., Oxford*, **10**, 1896, (551). Czapek, F., *Biochemie der Pflanzen*, **2**, 1905, (400 ff.).

THE MEANS OF LOCOMOTION IN PLANARIANS

By Caroline E. Stringer

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Communicated by G. H. Parker, October 29, 1917

The ordinary locomotion of fresh-water planarians is of two types, gliding and crawling. True swimming movements are also used by the marine form, Bdelloura. When gliding, the planarian slips smoothly and evenly over some supporting surface, with little or no apparent muscular effort. It has been believed generally that this form of locomotion results chiefly, or even entirely from the beating of ventral cilia. Crawling is accomplished through conspicuous muscular contractions.

Contrary to the opinion frequently expressed in papers on the histology of planarians, all species which I have so far examined have been found to be entirely covered externally with cilia. The cilia on the lateral margins of the anterior region beat in response to very weak stimuli. The cilia over most of the dorsal surface, on the lateral margins other than the head region, and on the ventral surface are usually inactive, except when subjected to strong mechanical or chemical stimuli. Long sensory hairs occur not only in the head region, but along the lateral margins and over the dorsal surface, as well. Tests with powdered carmine show that the ventral cilia of a gliding planarian are not beating during normal locomotion. On the other hand, a planarian when gliding on the under side of the surface film of water, so that the light falls upon its foot at an angle of about 45 degrees, and is reflected to the eye shows delicate muscular waves.

To determine whether locomotion could be accomplished either by the beating of cilia, or by muscular activity alone, a series of tests were made with solutions for the purpose of finding one that would inhibit muscular activity and leave the cilia free to beat normally, and another that would check ciliary action without interfering with muscular contractility. In either case, the animal must remain in all other ways as

nearly normal as possible. Various substances were found to check entirely all muscular activity but leave cilia actively beating all over the body. The most satisfactory results were obtained after one and a half to two and a half hours treatment with a solution of magnesium chloride m/7 to m/9. When all trace of muscular contractility was lost, locomotion did not occur, even though the cilia were beating with far greater vigor than under normal conditions. A solution of lithium chloride m/45, applied for eighteen to twenty-two hours, checks entirely the beat of the cilia, and leaves the muscles sufficiently unaffected to permit of locomotion by gliding. In such tests entire freedom from mechanical vibrations is an absolute essential and care must be exercised in handling the treated specimens to avoid a strong mechanical stimulus.

From these observations I conclude that the locomotion of planarians is essentially a muscular act in which the cilia play no necessary part.

[1] Contributions from the Zoölogical Laboratory of the Museum of Comparative Zoölogy at Harvard College. No. 301.

DIURNAL CHANGES IN THE SEA AT TORTUGAS, FLORIDA

By J. F. McClendon

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Communicated by A. G. Mayer, September 21, 1917

The only diurnal change noted in the Gulf Stream was a change in temperature of about 1° and the resulting change in oxygen tension. But in water shallow enough for considerable light to reach the bottom, marked diurnal changes were noted in temperature, hydrogen ion concentration (pH), total CO_2-concentration, CO_2-tension, O_2-concentration and O_2-tension. The temperature, O_2-concentration and O_2-tension were lowest and the CO_2-concentration and CO_2-tension highest about 5 a.m. The temperature, O_2-concentration and O_2-tension were highest and CO_2-concentration and CO_2-tension lowest at about 3 p.m., local apparent time during July. The magnitude and exact time of maxima and minima varied somewhat from day to day and varied a great deal with the location of the station at which the water was studied. The diurnal curves showed secondary notches which were probably due to tidal currents and eddies, since no such notches were present in the diurnal curves of stagnant sea water. The differences between stations were evidently due to previous history of the water carried past the station by currents and to variations in depth and in

fauna and flora at the bottom. The details of this work, including maps, graphs and tables, are to be published by the Carnegie Institution of Washington. The relation of local conditions to the precipitation of $CaCO_3$, thus decreasing the depth of the water, is pointed out.

Studies of the effect of these changes on organisms were made. The limiting factor for plants seems to be fixed nitrogen. Only 0.02 mgm. of fixed nitrogen per liter could be determined and it was not thought practicable to determine local changes with certainty. The limiting factor for animals seems to be food. Oxygen could easily become a limiting factor. One kilogram of fish would use up all of the oxygen in 4300 liters of water of the lowest O_2-concentration found at the surface, in twenty-four hours. It seems improbable that fish alone would suffocate, but swarms of Dinoflagellates might suffocate themselves and other animals present.

NOTE ON INTERFEROMETER METHODS OF MEASURING THE ELASTICS OF SMALL BODIES

By Carl Barus

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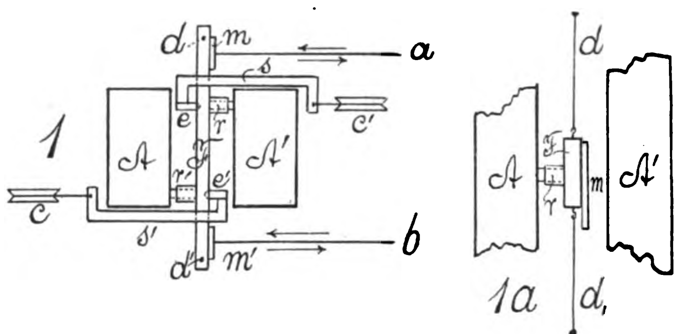
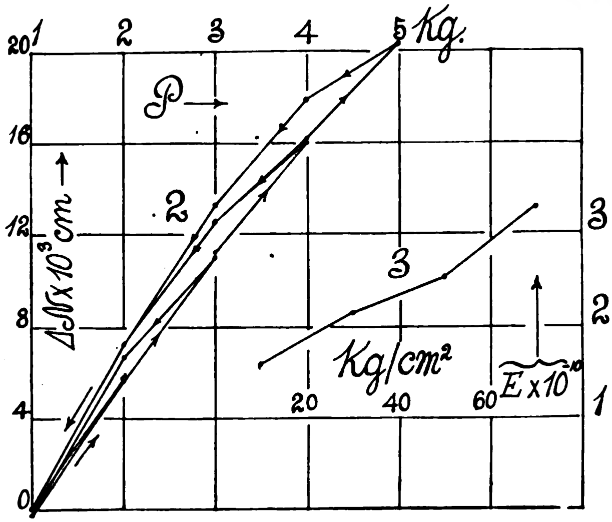
Communicated, October 26, 1917

1. *Method.*—At the request of Prof. W. G. Cady, who was in need of Young's modulus in case of certain crystals used in experiments in which he is interested, I endeavored to adapt for this purpose the interferometer heretofore[1] described for measuring small angles with an auxiliary mirror. The project seems feasible and apparently simple in execution, when the method of end thrust indicated in figure 1, is used. Here F is a rigid metallic bar subjected to a force couple, carrying the coplanar mirrors m, m', and capable of rotating slightly in a horizontal plane. These mirrors receive the corresponding rays, a, b, of the interferometer. The force couple is resisted by the resilience of the rods, r, r', to be tested, as these push respectively against the ends of the bar, F, and against the rigid abutments, A, A', of the apparatus. If the force couple changes the bar, F, rotates correspondingly. The component rays, a, b, then register the amount of rotation in the interferometer.

To apply the force couple, weights suspended from the stationary pulleys, c, c', were utilized. These actuate the rectangular offsets, s, s', which force their conical ends, e, e', into corresponding depressions of the bar, F. When not under stress, F is supported by the double

bifilar suspension,[2] the threads of which are attached at d, d'. The abutments were heavy cast iron bricks, $2 \times 3.5 \times 10$ cubic inches in size, firmly bolted together and standing with screws on a smooth bed plate. Thus the apparatus (not including the interferometer) can be rotated around a vertical axis to enlarge the achromatic fringes and around a horizontal axis parallel to F to rotate them. This is necessary for the adjustment.





Omitting details I may add that all measurements were made in terms of the displacement of the achromatic fringes heretofore described.

If E is the traction modulus, l the elongation of each rod of length L, under the force P, and Δ a differential symbol,

$$E = (\Delta P / A) / (\Delta l / L)$$

If the distance apart of the rays, a, b, is $2R$, and of the forces of the couple is $2R'$,

$$2 R \Delta \alpha = \Delta N \cos i,$$

supposing the bar F to rotate over an angle α, in consequence of the increment ΔP of thrust and ΔN to be the corresponding displacement of the micrometer mirror (rays incident at the angle i), needed to restore the interference fringes to their original position. But,

$$\Delta l = R' \Delta \alpha = R' \Delta N \cos i / 2 R$$

so that after reduction

$$E = \frac{2 L R}{A R' \cos i} \frac{\Delta P}{\Delta N}$$

The method will not of course be very precise, because for rods less than an inch long the quantities involved, particularly ΔN, are so small. Any flexure or slight dislocation of the parts of the apparatus are of relatively great consequence. Moreover there is another serious consideration. In long rods the stresses distribute themselves equally throughout the sectional area; but in short rods this is liable not to be the case. There will be lines of longitudinal stress and part of the area, A, may be relatively unstressed. Hence the value of E will come out too small and the question is rather to what degree such a method can be made trustworthy. If the achromatic fringes are used, the optical method as such presents no difficulties. For reasonably thin rods the observed displacement is adequate. The fringes need not be counted and it is even unnecessary to make the method very sensitive. Fringes of moderate size suffice.

The method of flexure would in some respects seem to be preferable. But it is is theoretically less simple and for short rods difficulties similar to the above would be encountered.

2. *Observations.*—In a large number of measurements made with different bodies, the apparatus finally took the form shown in figure 1, in which the rod r, r', to be tested is held by a rigid metallic tube or sheath, in which it fits loosely. Even this can not be employed quite without misgivings; but these must be passed over here. As an example of the results, I will insert graphically the behavior of hard rubber rods, each of about 2.4 cm. long and 0.35 cm. in diameter, thus having a sectional area of about 0.1 sq. cm. and kept under a minimum load of 1 kgm. These were subjected to cyclically varying stress with the results (contractions positive) given in figure 2. With pressures varying in the sequence (1, 2, 3, 2, 1), (1, 2, 3, 4, 3, 2, 1), etc., kgm., the contractions apparently give evidence of well developed hysteresis loops, upon which is super-imposed the continuous viscous deformation which

results from loading. The thrust modulus, E, computed from triplets of data for definite steps of pressure (1, 2, 1), (2, 3, 2), etc., kg. (i.e., 15, 25, etc., kgm. per square centimeter), are given in figure 3. They increase in marked degree with the load. Turning the rods down to smaller diameters successively and testing them in turn, no essential difference in the results was apparent. With rods of high rigidity like glass, brass, steel, only about one-half of the probable modulus can be reached with rods of the above dimensions. The remainder is lost in the small dislocations within the apparatus. These rods[3] must not be more than 1 or 2 mm. thick and enclosed in corresponding sheaths, to be available in an apparatus-like figure 1. Tentative[4] as the results are, however, they are interesting, inasmuch as the dependence of the elastics of a rod on its molecular instabilities will most probably be clearer in case of bodies of light structure like the organic bodies. The whole phenomenon is very much like the condensation of a vapor, requiring higher pressures to condense the instabilities and lower pressures for their release or evaporation, as it were. Deformation proceeds at a rapidly retarded rate through infinite time.[5]

[1] These PROCEEDINGS, 3, 1917, (412).

[2] Shown in the side elevation, figure 1*a*, with the offsets removed. The fibres d and d_1 are tightly stretched.

[3] Thus in case of steel rods like the above, per kg of load, $\Delta N/\Delta P = 44 \times 10^{-6}$ cm., which is too small for any micrometer.

[4] I have thus far been unable to arrive at a trustworthy distinction, except in magnitude, between the deformations within the apparatus and those of the rods themselves.

[5] From a report to the Carnegie Institution of Washington, D. C.

SUBLACUSTRINE GLACIAL EROSION IN MONTANA

By W. M. Davis

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Communicated November 7, 1917

The mountains of northwestern Montana and northern Idaho are characterized by two classes of deglaciated forms. The forms of one class are the work of relatively small, local glaciers, and are limited to the loftier ranges in which cirques, excavated in the higher slopes, lead down through well-scoured troughs to terminal moraines on the mountain flanks or on the open ground of intermont basins. The forms of the other class are the work of great Canadian glaciers and are limited to the sides and floor of the larger valleys. Two such glaciers crossed the international boundary, as shown in figure 1, truncated the side spurs

of the valleys that they followed, and locally overdeepened the valley floors into lake basins, at the farther end of which large terminal moraines were deposited. One of the Canadian glaciers moved southward along the large longitudinal valley known as the Rocky Mountain trough and, after reënforcement from Glacier National park, spread out in the broad Flathead basin; a brief account of its erosional work, as well as of the

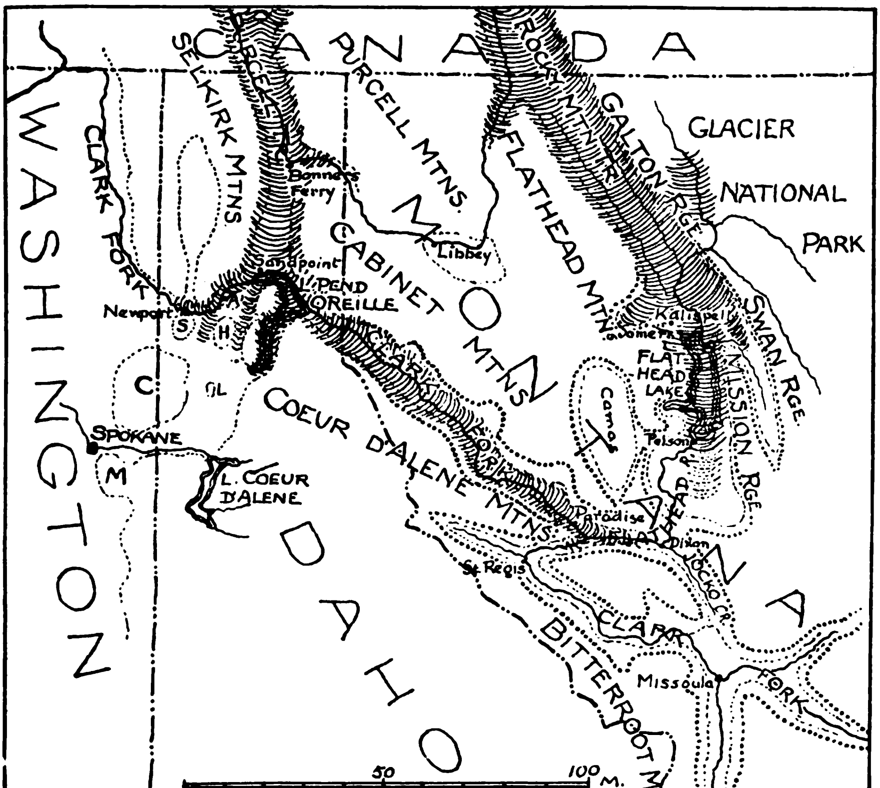

FIG. 1. OUTLINE MAP OF CANADIAN GLACIERS FORMERLY INVADING NORTHWESTERN MONTANA AND NORTHERN IDAHO.

work of local glaciers in the adjoining Mission range, was given in these PROCEEDINGS two years ago,[1] and a somewhat fuller statement has been published in the *Geographical Review*.[2] The other Canadian glacier moved southward along the Purcell trough farther west and invaded the valley of Clark fork of the Columbia river; its work is here summarized. A full report of the Shaler Memorial investigation on which these special studies are based will probably appear in the *Annals of the Association of American Geographers*.

The western glacier may be named after two deep lakes, Kootenay and Pend Oreille, the basins of which it excavated, one to the north, the other to the south of the international boundary. Lake Kootenay is a superb sheet of water of simple outline, ocupying an elongated and greatly overdeepened trough-basin between high mountain slopes characterized by strongly truncated spur-ends and hanging side-valleys; its length is much decreased by the long delta-plain of Kootenay river, which enters the lake from the south after a long detour through Montana from the Rocky mountain trough in which its sources lie; the same river, as the lake outlet, turns westward at mid-length of the lake trough, where a distributary branch of the main glacier scoured out a side trough of catenary cross-profile, with fine hanging side-valleys, but probably 1000 feet less deep than the main trough which holds the lake. Lake Pend Oreille occupies a deep basin of the same kind, that was excavated between two mountain ranges in Montana by the middle one of three terminal branches into which the Kootenay-Pend Oreille glacier was there divided: this lake has been encroached upon by heavy morainic and outwash deposits on the north, which aid in separating it from Lake Kootenay. Huge volumes of gravel were washed southwestward from the terminal moraine at the farther end of Lake Pend Oreille, and now form a terraced intermont plain for 30 or more miles as far as Spokane: several side valleys in the adjoining mountains were barred by the outwashed gravels and now hold lakes, of which the largest is Lake Coeur d'Alene.

Clark fork enters the east side of the broad northern end of Lake Pend Oreille where an arm of the lake would probably have a length of ten or more miles up the river valley but for inwashed gravels; the river flows out from the west side. The two parts of the river, above and below the lake, may be referred to as upper and lower Clark fork. The shortest of the three branches in which the Kootenay-Pend Oreille glacier ended moved westward a score of miles down the valley of lower Clark fork, and supplied the valley beyond its end with a great volume of outwashed gravels, now terraced by the river.

The southeastern terminal branch of the Kootenay-Pend Oreille glacier was remarkable for its long course up the valley of upper Clark fork for 100 miles: its width may have been 10 miles or more near the point of its outbranching, but for much of its length it was less than 5 and sometimes less than 2 miles wide: its greater extension than that of the western branch was probably due to better enclosure between

mountainous highlands and perhaps still more to its being covered for most of its length by the waters of the lake that it ponded, as described below. The work of this long branch glacier in truncating the spurs of the adjoining mountain sides is conspicuous, and gives a peculiarly bold aspect to the valley that it ascended, but with decreasing effect up stream. It was these truncated spurs that caught the attention of the Transcontinental Excursion of the American Geographical Society, when our train ran down the valley from Missoula on the way to Spokane in 1912. We thus passed from the uppermost valley, where the side slopes

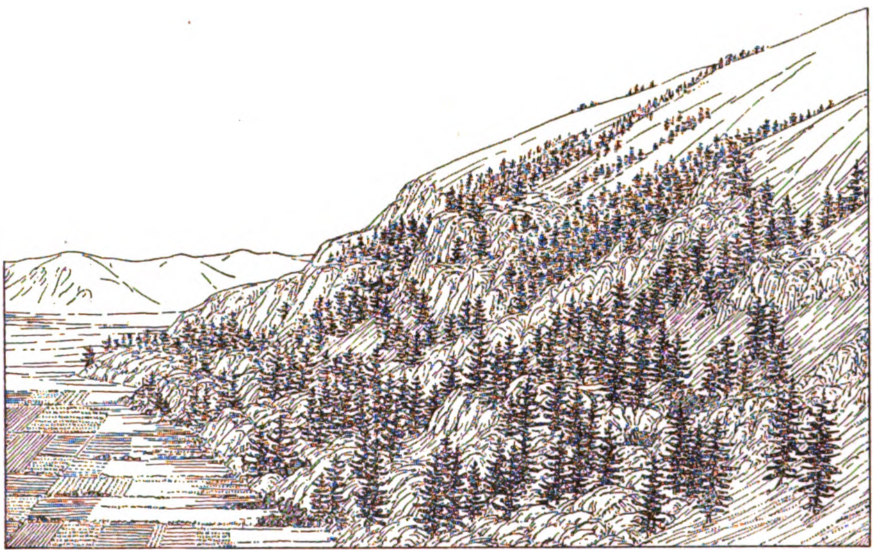

FIG. 2. A TRUNCATED VALLEY-SIDE SPUR, AT PARADISE, MONTANA, LOOKING NORTHWEST.

have normally carved forms, and came unexpectedly on the marks of glacial scouring, faint and low at first, stronger and higher as we proceeded, until the resulting spur-end cliffs gained heights of 500 or 1000 feet, as in figures 2 and 3, and compelled the attention of all observers; I returned there in 1913 for more deliberate study. The contrast between the smooth, maturely rounded forms of normal erosion on the higher, never-glaciated slopes and the ragged, immature cliffs of glacial scouring was as striking as it was persistent. Side valleys are occasionally barred by local moranic embankments, as in fig. 4, and holds swampy hollows behind them: the height of these moraines,

somewhat less than that of the cliff tops, gives the best indication of the local height of the ice margin.

The bare hill sides of the upper valleys of the Clark fork drainage system show many faintly marked shorelines, up to altitudes of 4200 feet; 20 or 24 such lines may be counted, one over the other, in some localities. These have been understood for some years past as recording the occurrence of a temporary lake of fluctuating level, to which the name of Lake Missoula has been given. Pardee pointed out in 1910 that the lake must have resulted from the obstruction of Clark fork by the Canadian glacier at the head of Lake Pend Oreille,[3] where marks of glacial scouring are recognizable in the steepening of the neighboring mountain spur on the south, between Lake Pend Oreille and upper

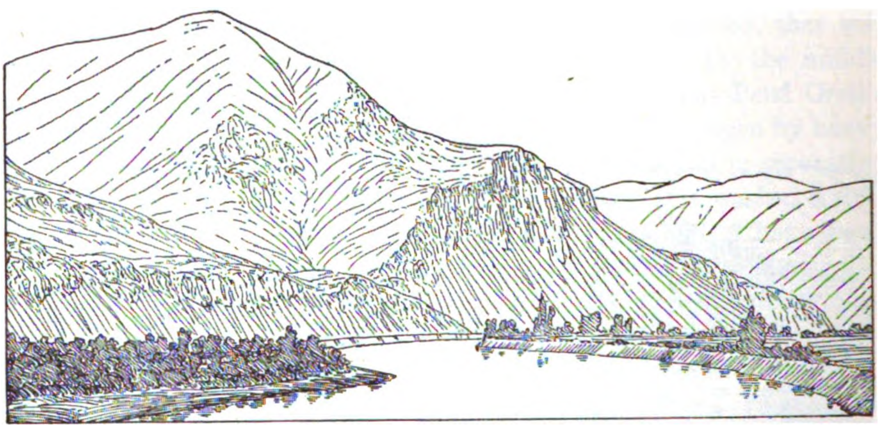

FIG. 3. A STRONGLY TRUNCATED VALLEY-SIDE SPUR, ABOVE PARADISE, MONTANA, LOOKING EAST: CLARK FORK IN FOREGROUND.

Clark-fork valley, up to about the same altitude as that of the highest lake shoreline. The fluctuating level of the lake appears—following the explanation adopted in Sweden for similarly fluctuating glacial lakes—to result from the location of the outlet on the fluctuating surface of the glacier where it impinged on the mountain spur that divided its southern and southeastern branches. It is to be expected that various signs of rushing water should be found on the steepened slope of this spur; unfortunately I had no opportunity of examining that point during my visit of 1913.

Now as the lake shorelines seem to prove that the upper tributary valleys of the Clark fork system were occupied by a lake while the main valley was invaded by the southeastern branch of the Kootenay-Pend

Oreille glacier, and as the lake must have been at its highest level when the branch glacier had its greatest length, it follows that the erosive work of the glacier in truncating the lower ends of the valley-side spurs must have been done under water. The glacier seems to have remained immersed in the lake that it barred because the ice pressed so heavily against the bottom and sides of the valley that no water could enter there to buoy it up. The lake waters at the end of the branch glacier must have been about 1500 feet deep. The same appears to be true of the much broader Canadian glacier in Flathead basin, while it was scouring off the spur-ends on the western slope of the Mission range.[2] The alternative supposition that the glaciers were ordinarily floated up from their valley floors when the lake waters rose, and that they rested on the

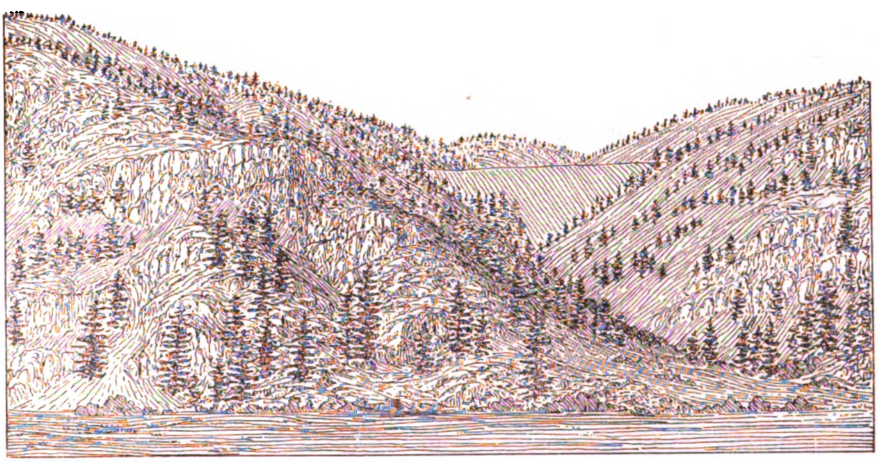

FIG. 4. A SIDE VALLEY BARRED BY A MORAINE, AT PARADISE, MONTANA, LOOKING NORTH

floors and did their erosive work only while the lake was temporarily discharged by leaking through the ice barrier, is, apart from its inherent improbability, not acceptable because the great terminal moraine deposited by the Flathead glacier is of too regular a pattern to have been formed by an agency acting so irregularly.

But the first supposition, on which we are thus thrown back, must also seem inherently improbable; and all the more so when the unfavorable conditions that it imposes on the Clark-fork branch glacier are clearly conceived. This long glacier not only had to creep up a relatively narrow valley that sloped against the direction of glacial advance; it had to creep up the valley against the weight of the lake water in which it was immersed. It is difficult to imagine how the push

from the main glacier could have compelled its narrow branch to advance 100 miles against such discouragements: yet the ice not only did advance 100 miles up Clark-fork valley, but advanced with such insistent pressure that it tore off the resistant rock of the valley-side spurs.

The sublacustrine glacial erosion thus attested takes its place as the last term of a series of unanticipated processes. Seventy years ago, the fiords of Norway, the sea lochs of Scotland, and other similar embayments were interpreted, by those who then accepted Dana's principle of shoreline development, as submerged river valleys. Forty-five years ago, the opinion gained ground that fiord troughs were largely the work of glacial erosion, but the erosion was supposed to have taken place above sea level; the occupation of the troughs by arms of the sea was explained as the result of later submergence. This view was gradually modified by recognizing that a great glacier might erode a trough, if it eroded at all, somewhat below sea level; but the extreme depth of such submarine erosion was placed at about six-sevenths of the thickness of the glacier: at greater depths, the ice would be buoyed up so that it could not erode the trough bottom. Then about twenty years ago Gilbert suggested, as a result of observations that he made in Alaska when a member of the Harriman expedition, that heavy glaciers must press so heavily on their trough beds that water could not enter beneath them; hence such glaciers could erode as well below as above sea level; but it was not supposed that they could be immersed for a score of miles or more. Now the Clark fork branch-glacier seems to have done its visible erosive work on the valley-side spurs—and presumably a considerable amount of invisible work on the valley bottom also—although it must have been wholly immersed in Lake Missoula for two or three score if not for four score miles. It seems impossible for a glacier to perform erosional work under such conditions, yet the erosional work is undeniably visible. Perhaps the conditions of its performance were other than those here indicated, but if so, I have not been able to discover them.

[1] Davis, W. M., these PROCEEDINGS, 1, 1915, (626–628).
[2] Davis, W. M., *Geogr. Rev.*, 2, 1916, (267–288).
[3] Pardee, T. G., *Chicago, J. Geol. Univ. Chic.*, 18, 1910, (376–386).

THE EFFECT OF STRETCHING ON THE RATE OF CONDUCTION IN THE NEURO-MUSCULAR NETWORK IN CASSIOPEA

By J. F. McClendon

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Communicated by A. G. Mayer, September 21, 1917

It was observed by Carlson (*Amer. J. Physiol.*, **27**, 1911, 323) that stretching the nerve of the slug has no effect on the rate of the nerve impulse. This does not support Bethe's hypothesis that the impulse passes over solid neuro-fibrillae which are zig-zagged in the relaxed nerve and straightened out in the stretched nerve. Conditions are not so simple in the nervous network of the sub-umbrella of Cassiopea, but the rate may be more accurately determined. The stretching may increase the original length 84%.

A ring or belt of umbrella tissue was placed in a frame by means of which the circumference could be stretched and at the same time its length measured. The apparatus was immersed in sea-water kept at 30°. A neuro-muscular wave was started in the ring in such a manner that it traveled continuously around the ring and its speed measured by noting the time at which the muscular contraction wave passed a certain point. The time required for the wave to pass 100 times around the ring was recorded with a stop-watch. An example of one of the experiments is as follows:

Length of circumference, mm.	286	306	326	346	366	386	406	426	446	466	486	506	526
Rate (mm. per second)........	376	390	399	410	414	407	403	391	377	368	360	352	342

It may be noted that the rate changed 17% while the length increased 84%, in other words the rate is relatively constant. An uncertainty of rate of 1-5%, due to hysteresis, could not be analyzed with certainty owing to the fact that the course of the neuraxones is zig-zagged and interrupted by synapses, but some speculation on this phenomenon will be published elsewhere. The purpose of this abstract is to point out that the experiments on Cassiopea tend to support Carlson's conclusion that stretching the nerve does not change the rate, and that the conducting substance, itself, can be stretched and relaxed.

A CRITICISM OF THE EVIDENCE FOR THE MUTATION THEORY OF DE VRIES FROM THE BEHAVIOR OF SPECIES OF OENOTHERA IN CROSSES AND IN SELFED LINES

By Bradley Moore Davis

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Communicated by R. Pearl, October 31, 1917. Read before the Academy, November 20, 1917

The mutation theory of Professor De Vries rests so very largely upon deductions from his studies on species of Oenothera that any discussion of it naturally centers upon the interpretation of the behavior of these plants when selfed (in-bred) and in their crosses with one another. Of these species *Oenothera Lamarckiana* stands as the form most thoroughly studied with respect to its habit of throwing off in successive generations variants with numerous distinguishing characters of such a nature that they can with certainty be separated and would rank in systematic botany as clearly defined new species arising suddenly and fully formed from the parent type. Professor De Vries calls these variants 'mutants' and interprets their appearance as the spontaneous origin by mutation of new species from a plant, *Oenothera Lamarckiana*, which he believes to be representative of a pure species. De Vries is not willing to allow that in *O. Lamarckiana* this phenomenon may be the direct result of an impure or hybrid constitution. The behavior of *O. Lamarckiana* and certain other forms in this genus is, therefore, to De Vries direct experimental evidence of the origin of new species through wide and discontinuous variations, the result of spontaneous internal manifestations within the parent plants. De Vries further believes that mutations play a very important part in organic evolution and that they largely supply the material, i.e., the variations, upon which natural selection can operate.

There is no question of the facts as brought out in the extensive and patient work of De Vries; they have been repeatedly confirmed. *Oenothera Lamarckiana* if grown in sufficiently large cultures may be expected to produce in each generation approximately the same set of 'mutants.' The proportions differ but they are apparently fairly stable for each variant; some make up about 1–2 per cent of the cultures, others are much less common. Certain of the 'mutants' breed fairly true when selfed while some are more unstable than the parent *Lamarckiana*. A significant feature of this performance is the clear expression of order and system in the appearance of precisely the same types through successive generations and we have no reason to suppose that *O. Lamarck-*

iana is likely to give up this habit of throwing variants however long it may be cultivated.

Now the regularity with which *Oenothera Lamarckiana* produces its 'mutants' through successive generations indicates conditions within the germ plasm of such a nature that a number of different specific types of sexual cells are produced rather than a single set of gametes uniform in their germinal constitution. There is really not the spontaneity in the production of new forms by *Lamarckiana* which one might expect of a plant in a state of 'mutation' with an organization expressing itself in irregular and unexpected departures from the type through peculiarities of mutating instability in its germinal constitution. Consequently a critic of the evidence for the mutation theory offered by De Vries from the behavior of *Lamarckiana* very naturally is led to question the fitness of this plant as representative of a pure species. The discussion must finally center on the problem of whether or not the germinal constitution of *O. Lamarckiana* is homozygous, i.e., carrying two identical sets of hereditary factors derived from the parents through each sexual union. May not the germinal constitution be heterozygous, or hybrid, the two sets of hereditary factors in some respects differing from one another?

An organism homozygous in germinal constitution can develop only one type of sexual cells, gametes, and these will be identical with those of the parents unless chemical or physical conditions affecting the germ plasm modify directly the germinal constitution carried through the succession of cell divisions that make up a generation, or upset the precision of the reduction divisions previous to the formation of gametes, or acting directly on the gametes themselves change their organization. Variations of the germinal constitution introduced in this manner would constitute mutations and it is an admitted fact that variations which might be interpreted as mutations are very rare in the lines of animals and plants which are believed to be most pure and are consequently most stable in their breeding behavior.

A heterozygous organism must at the time of gametogenesis distribute the hereditary factors unevenly whenever these factors as they come from the two parental lines differ from one another. There are many reasons why hereditary factors are believed to be present in the chromosomes, and the reduction divisions which distribute whole chromosomes into two group clearly furnish a mechanism by which a segregation of factors may take place. The most complete and satisfactory studies on chromosome reduction for both animals and plants have

established the fact that the two sets of chromosomes, derived one from each parent, constitute two series of homologous pairs and that the members of these pairs become closely associated before the reduction divisions and are later separated by this mitosis which may properly be termed a segregation division.

Studies on the reduction divisions of *O. Lamarckiana* and some of its derivatives by Geerts, Gates, Stomps, Lutz and Davis have shown loose associations such that the mechanical conditions favor irregularities of distribution which actually do occur and gametes are known to be sometimes formed with one more or one less chromosome than 7 which is the normal number for the genus. In the two 'mutants' *lata* and *scintillans* there have been observed 15 chromosomes, obviously the result of the union of gametes bearing unlike numbers of chromosomes. Forms with 21 chromosomes are also known which apparently arise from the fertilization of an unreduced egg (14 chromosomes) by a normal sperm nucleus (7 chromosomes). There is also a very rare type, *gigas*, with 28 chromosomes which has been matched in chromosome number by analogous forms discovered by Bartlett from other species of Oenothera. This irregular behavior of the chromosomes in *Lamarckiana* and its 'mutants' gives strong cytological evidence of conditions such as might be expected in heterozygous material where the two sets of chromosomes from parental lines are dissimilar in their genetical constitution and consequently fail to pair closely previous to segregation through the reduction division. One of the oenotheras, a race of *grandiflora*, has been found to present an orderly assembling of chromosomes in pairs at the time of reduction together with an equal distribution of the members of each pair and this history in one of the more stable forms serves to emphasize the striking irregularities of *Lamarckiana*. Therefore the cytological evidence is distinctly favorable to a view that *Oenothera Lamarckiana* contains a chromosomal complex of a mixed or hybrid character rather than two similar sets of chromosomes.

On the genetical side there is more obvious evidence of the heterozygous nature of *Oenothera Lamarckiana*. It is a law of genetics that crosses between organisms which produce uniform gametes must give uniform progenies in the first generation and this constitutes a reliable test of whether or not the parents are monogametic; if the first hybrid generation contains distinct classes then one or the other or both of the parents must have produced more than one kind of fertile gametes. De Vries discovered the striking fact that when *Lamarckiana* and some of its mutants are crossed with certain wild species of Oenothera their

progeny in the first generation fall into two groups sharply separated from one another and these De Vries termed 'twin hybrids.' Since the twin hybrids are produced in crosses of *Lamarckiana* with several species some of which when crossed among themselves give uniform progeny in the first generation the evidence indicates that *Lamarckiana* must supply the two different types of gametes which make possible this splitting in the first generation. De Vries holds that the cause of twin hybrids lies in the state within the gametes of certain factors called pangens whether active, inactive or labile and this appears to be an admission that *Lamarckiana* does not form equivalent gametes.

Long experience of plant and animal breeders has led them to suspect that pronounced sterility in an organism indicates hybrid constitution and critics of the purity of *Oenothera Lamarckiana* have pressed the point that in this plant approximately one-half of the pollen grains and ovules abort and that the proportions of fertile seed are low, being from about 30 to 40 %.

Extensive studies of Geerts followed by observations of other workers have shown these conditions to be generally characteristic of species of Oenothera and allied genera. These facts indicate the necessity of detailed studies on the cytology of gametogenesis, fertilization and embryo formation. Thus if it could be shown that in every group of four pollen grains, tetrad, formed as the result of the reduction mitoses only two grains are perfect the conclusion would be justified that the pollen sterility was the result of this segregation division. Unfortunately the abortion of pollen grains takes place after the members of the tetrad have separated and the relation of sterile pollen grains to one another and to the perfect grains is not evident, but it is a fact that shriveled, sterile pollen is distributed among the perfect grains so evenly as to suggest an origin through the reduction division rather than from some physiological cause such as malnutrition, which under certain conditions is known to produce high degrees of sterility.

The facts of gametic and zygotic or seed sterility have, however, suggested certain working hypotheses that must be considered in present and future research on oenotheras. Thus it is possible to conceive of impure or heterozygous species capable of reproducing their lines (breeding true) and showing little or none of the phenomenon of hybrid splitting provided only such gametes and seeds are fertile as will reproduce the hybrid type. Renner has applied this line of reasoning to *Lamarckiana* by assuming that the small proportion of fertile seeds of this plant are those formed by the union of the two different types of gametes

which produce the twin hybrids and that homozygous combinations of gametes are represented in the sterile seeds. Occasional fertile combinations of gametes varying from the usual type may then be responsible for the so-called mutants which owe their peculiarities to segregation phenomena, to be expected in a hybrid, rather than to spontaneous modifications such as are assumed by the mutation theory.

Finally, in this brief criticism of *Oenothera Lamarckiana* as representative of a pure species and therefore suitable material for conclusions on the importance and character of mutations in organic evolution it should be pointed out that there is no evidence that this plant is a wild species native to the American continent which was the original habitat of the group. On the contrary we have reason to believe that *Lamarckiana* was brought into cultivation from material growing in England where the early introduction of oenotheras established some extensive colonies probably of mixed and hybridized character. It has also been found possible by crossing two carefully selected species of Oenothera (*franciscana* × *biennis*) to synthesize a hybrid scarcely to be distinguished in its systematic characters from *Lamarckiana* and this product which has been named *neo-Lamarckiana* forms twin hybrids when crossed with certain species that give twin hybrids with *Lamarckiana*. *Neo-Lamarckiana* when selfed throws a much larger progeny of variants than does *Lamarckiana* but this fact seems to be correlated with its much higher seed fertility, from 84 to 87 %. These variants have been repeated in their essential characteristics through three generations and the parallel of this behavior to that of *Lamarckiana* is very close although as would be expected, the types of variants are not the same. Thus an undoubted hybrid among the oenotheras has been shown to present breeding habits similar to those of *Lamarckiana*. The behavior of *neo-Lamarckiana* when selfed appears most readily explained as due to the breaking up of a hybrid rather than by principles of mutation since characters more or less similar to those of the parents appear among the derivatives.

In addition to the studies on *Oenothera Lamarckiana* and its 'mutants' there has been a very large amount of work by De Vries, Gates and others involving other species of Oenothera and more recently extensive studies by Shull, Bartlett and Atkinson. These have brought out some very puzzling situations in the behavior of Oenothera species in crosses. In some cases the first generation hybrids show pronounced resemblance to the paternal parent of the cross, in other cases to the maternal parent, and still other combinations give blends with inter-

mediate characters such as frequently appear in the first generation. Second generations in some cases breed fairly true, in others show extensive and characteristic splitting usual to hybrids. Back crosses in some combinations reproduce almost exactly one of the parent types. Shull in a remarkable series of crosses has obtained in the first generation polymorphic progenies of much greater complexity than the twin hybrids of De Vries. Atkinson has described quadruple hybrids in the first generation from crosses between two wild American species. Bartlett has found that selfed lines of certain American wild species may throw 'mutants' in proportions as high as 50, 80 or even 100 per cent of the cultures.

Explanations of these extraordinary types of behavior are not yet clear. Atkinson, insisting on the purity of the species with which he worked, proposes a view that multiple progenies in the first generation are determined by the selection or differentiation of factors in the fertilized egg or zygote, an hypothesis which will require cytological evidence to be convincing. Bartlett holds the view that different classes of gametes are formed, an attitude in accord with much evidence from various studies on animals and plants. This seems to the writer to be an admission of impurity of germinal constitution unless it be shown that gametes may mutate in immense proportions, a view which has no support from genetical and cytological studies in general. None of these investigators seem inclined to admit the possibility or probability that the complexities of Oenothera genetics may be the result of germinal impurity widespread among the species as the result of extensive hybridization.

Yet the high degrees of gametic and zygotic sterility now known for certain forms of Oenothera makes it possible to conceive these species as impure, maintaining themselves because for the most part only those gametic combinations are fertile which will reproduce the heterozygous condition. Variants in selfed lines from such stock would most naturally be interpreted not as mutations but as the result of other gametic combinations which prove to be fertile. Crosses between impure species are, of course, crosses between hybrids and the behavior of their progeny, especially when high degrees of sterility are present, would naturally be expected to prove unusual and irregular. Only recently has the importance of sterility and delayed seed germination received serious consideration in problems of Oenothera genetics and no investigations have as yet been published which give the matters full consideration.

In conclusion it should be noted that although most of the genetical work on oenotheras has not been interpreted by the Mendelian system of notation there is, nevertheless, clear evidence of order in the sharply defined results of both inbreeding and crossing. A few cases are known of simple and clean cut segregation in ratios fairly close to Mendelian expectations, notably in crosses between *Lamarckiana* and *brevistylis* and probably in time more of these will be found. The difficulty has been to discover and to isolate simple material in the confusion of mixed and impure forms present in this group of plants. A great forward step will be taken in Oenothera genetics when types of proven purity have been established, since such forms as standard material in breeding tests may prove to be the keys that will open doors of mystery.

THE SPECTRA OF ISOTOPES AND THE VIBRATION OF ELECTRONS IN THE ATOM

By William D. Harkins and Lester Aronberg

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Communicated by J. Stieglitz, November 12, 1917

According to most of the recent theories of atomic structure and of the origin of light, the emission of light is due to the vibration of the non-nuclear electrons in the atom, but there is a difference of opinion in regard to the process by means of which the radiation takes place. The frequency of the light has usually been assumed to be that of the vibrating electron which emits it, but the theory of Bohr[1] indicates a less simple relationship according to which the frequency of the light varies as the increment of the two-thirds power of the electronic frequency.

The investigation reported in this paper was begun several years ago for the purpose of determining if the electronic periods are wholly dependent upon the *net* positive charge on the nucleus of the atom. In order to get a more definite statement of the problem, it may be assumed that the nucleus of any atom contains a positive electrons and b negative electrons. The net positive charge on the nucleus may be taken as $a - b$ or P. While P is not known exactly it is probably equal to or only slightly greater than the atomic number N. A single element, such as lead, is characterized by a single value of P. Isotopes are different atomic species of the same element, all with the same value of P, but with different values of a and b. Since $a - P = b$, the numerical value b gives not only the number of negative electrons, but also the

number of positive electrons in excess of the nuclear charge P. The fact that isotopes, with a constant value of P, but with a variable value of b, have almost exactly the same spectra, indicates that the non-nuclear electrons vibrate as might as expected if the b positive and b negative electrons were not present in the nucleus, that is as if it consisted of P positive electrons alone. Since, therefore, the vibration of the non-nuclear electrons depends almost entirely upon the net nuclear charge P, it is evident that both the effect (1) of the mass of the nucleus, and (2) of the bound b positive and b negative electrons, must be extremely small. The work here described was undertaken to see if either of these effects is large enough to produce a measureable deviation in the spectra of isotopes. It might be expected, even if there is no effect due to the mass, that if the positive and negative electrons in the nucleus are not coincident, their space arrangement should cause a slight, though possibly unmeasureable effect upon the spectrum.

The isotopes chosen for this work were ordinary lead and lead from radium (Radium G, or Uranio-lead 3). The atomic weight of the former is 207.18, and that of the latter, as determined by Prof. Theodore W. Richards, is 206.34 for this particular specimen used in this investigation. If ordinary lead is a single isotope this specimen of Uranio-lead 3 contains about 25% of ordinary lead as an impurity, since the atomic weight of pure uranio-lead 3 is about 206.05. The value of N for these isotopes is 82.

The spectra of isotopes were first investigated in 1912 by Russell and Rossi,[2] and by Exner and Haschek,[3] who examined the spectra of ionium and thorium (isotopes of atomic number 90) but found no measureable differences. Isotopes of lead have been studied spectroscopically by Soddy and Hyman,[4] by Baxter and by Richards and Lembert,[5] by Rutherford and Andrade,[6] by Honigschmidt and St. Horovitz,[7] by Merton,[8] and by Siegbahn and Stenstrom,[9] all of whom found the spectra of isotopes identical within the limits of accuracy of their work, except that Soddy obtained a slight difference in intensity for the line $\lambda = 4760.1$. While Merton used a higher dispersion than any of the other workers, he does not give the atomic weight of the uranio-lead which he used.

The measurements here presented were made by Dr. Aronberg, and the spectroscopic work was directed by Prof. H. G. Gale. Preliminary determinations with ordinary lead, and with ordinary lead mixed with about 20% of uranio-lead 3, when made on a six inch Rowland concave grating, gave no measureable differences in the spectra or in the

Zeeman effect. A quartz spectrograph with a dispersion of about 25 angstroms per millimeter gave a similar result when used with uranio-lead of atomic weight 206.34, but when the Michelson 10-inch plane grating was used in the sixth order, a difference of about 0.0043 A. U. was found, and as might have been predicted theoretically, the uranio-lead has the longer wave length and therefore the lower frequency. This difference was obtained for the line $\lambda = 4058$, the strongest line in the spectrum of lead. Unfortunately the other lines of lead are too weak to give a photograph with this grating as used in the higher orders, in any reasonable time of exposure.

The structure of the line $\lambda = 4058$ in ordinary lead was studied by Jonicke[10] and by Wali-Mohamed,[11] who used a vacuum lead arc as a source of light. By the use of an echelon they found two satellites at $+0.032$ and -0.041. In the present investigation it was found, however, that both the ordinary and the uranio-lead give only one satellite, at $-$ 0.0480 and -0.0501 for the uranio and ordinary lead respectively, the difference being within the experimental error of the measurement. The difficulty is due to the lack of definition of the satellite.

As a source of light a slightly modified form of the oxycathode arc in vacuo as employed by Wali-Mohamed, was used in the form shown in figure 1. This gives a bright source and at the same time very sharp and narrow lines, as is essential for work of such delicacy. Such an arc had the advantage too, that the extremely rare uranio-lead 3 was not wasted. It was first used with a Hilger quartz spectrograph, in which case the interesting observation was made that the lines λ = 2823 and 2833 had different intensities in the spectrum of the uranio-lead from what they had in the ordinary lead. This was similar to the result obtained by Soddy, but it was found to be due to the fact that after the arcs started the pressure in the lamp containing the uranio-lead rose from 0.05 mm. to 0.25 mm., and it was the small difference in pressure which caused the change in intensity. For this reason, when the grating was used, the two lamps were connected in parallel to the vacuum pump in order to keep the pressure in both the same, about 0.04 mm. In order to avoid mechanical shifts both exposures were made at the same time. The light from one lamp went through a right angle prism attached to the slit, thus forming a spectrum, the middle of which belongs to one kind of lead while the lines both above and below belong to the other. The voltage, 40 volts, was kept the same in both within one volt, and the amperage, 1.1 amperes, was kept within 0.05 ampere. In this way seven exposures were made in one position,

then the two lamps were interchanged and six more exposures were made, when the positions were again reversed and three more measurements were made. On the average the wave length of the uranio-lead was 0.0043 A. U. longer than that of the ordinary lead. The seventeen measurements were as follows, where all should have the positive sign: 0.0046, 0.0046, 0.0068, 0.0036, 0.0057, 0.0043, 0.0036, (shift of lamps)

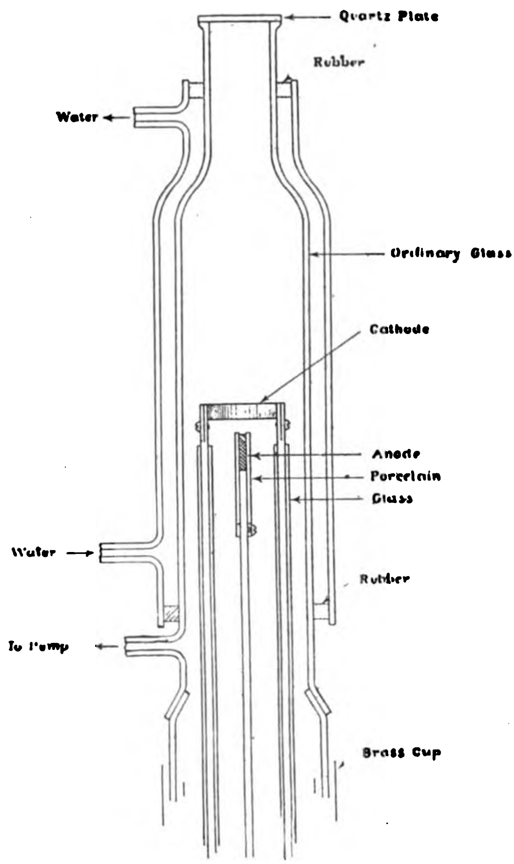

FIG. 1

0.0036, 0.0036, 0.0039, 0.0046, 0.0057, 0.0036, (shift) 0.0046, 0.0036, 0.0046 A. U. The average difference was 0.012 mm. on the plates, and the dispersion was 0.359 A. U. per millimeter. In experiments 6 and 7 the voltage of the ordinary lead was lowered, and in 12 and 13 that of the uranio-lead was lowered, in each case by 3 volts. In experiment 17 the amperage of the uranio-lead was lowered by 0.2 amperes. It

may be seen that these changes of voltage and amperage produced on noticeable changes in the shift of the lines. Finally, to establish the change in a new manner, the uranio-lead was removed from its lamp, and ordinary lead put in its place, and when this was done the shift was found to disappear.

When Merton did his work on the isotopes of lead, J. W. Nicholson calculated for him the shift in a lead line at $\lambda = 4000$, which would be caused by an atomic weight difference of 0.5 unit according to Professor Hicks' theory that the atomic weight term enters exactly into the separation of doublets and triplets in series spectra, and assuming that at this wave length lead has a doublet series spectrum with a separation of 50 A. U. The calculation showed that the shift should be 0.15 angstroms which is very much larger than the shift actually found. On the other hand the theoretical formula developed by Bohr

$$\nu = \frac{2\pi^2 e^2 E^2 m}{h^3\left(1 + \frac{m}{M}\right)}\left(\frac{1}{n_1^2} - \frac{1}{n_2^2}\right)$$

gives a much smaller value (0.00005 angstroms) but of the same sign as that found experimentally. However, Bohr's calculation was made for an extremely simple atomic system, while the lead atom contains a large number of non-nuclear electrons (probably about 82).

Since this is the first investigation reported in which a shift has been found in the spectra of isotopes it would seem advisable to check the experimental work by an altogether new investigation, especially since the shift as determined is extremely small. However, it should be stated that the difference in the position of the lines is easily observable under the measuring microscope. One remarkable feature of the photographs is that the lines seem to be shifted, and not to be broadened.

We wish to thank Prof. H. G. Gale for directing the spectroscopic work and Prof. Theodore W. Richards for the extremely rare Radium G which he placed at our disposal for this work.

[1] Bohr, *Phil. Mag., London*, **26**, 1913, (1, 476, 857); **29**, 1915, (332).
[2] Russell and Rossi, *London, Proc. R. Soc.* (A), **87**, 1912, (478).
[3] *Berlin, SitzBer. Ak. Wiss.* **121**, IIa, 1912, (175).
[4] *Trans. Chem. Soc.*, **105**, 1914, (140).
[5] *J. Amer. Chem. Soc., Easton, Pa.*, **38**, 1914, (1329).
[6] *Phil. Mag., London*, **27**, 1914, (854–68).
[7] *Berlin, SitzBer. Ak. Wiss.* **123**, IIa, 1914.
[8] *London, Proc. R. Soc.* (A), **91**, 1914, (198).

[9] *Paris, C. R., Acad. Sci.*, **163**, 1914, (428).
[10] *Ann. Physik., Leipzig*, **29**, 1909, (833).
[11] *Astroph. J., Chicago, Ill.*, **39**, 1914, (189).

THE EFFECT OF OXYGEN TENSION ON THE METABOLISM OF CASSIOPEA

By J. F. McClendon

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Communicated by A. G. Mayer, September 21, 1917

It was shown by Verzár (*J. Physiol.*, **45**, 1912, 39) that decreased oxygen tension in the blood capillaries decreased the metabolism of muscle but not of salivary glands. The animal died before the oxygen tension in the salivary glands was reduced sufficiently to cause a noticeable fall in metabolism. In order to avoid complications in circulation, as in Verzár's experiments, I used the umbrella of Cassiopea in such a manner as to maintain a thin layer of cells of uniform activity constantly bathed with sea water at 30°. It had been determined in preliminary experiments that a rise of 10° in temperature doubled the metabolism, but that the hydrogen ion concentration could be changed within certain limits without changing the metabolism to a degree that could be measured with certainty. The average of a large number of determinations placed the respiratory quotient at 0.95, but whether it was constant could not be determined. From the heat produced and nitrogen lost it was concluded that proteins with a small admixture of carbohydrates and fats were burned.

Since the temperature was constant, the oxygen-tension was proportional to the O_2-concentration, as determined by the Winkler method, the mean of the values at the beginning and end of the experiment being used. The metabolism was measured by the oxygen used, as that was determined more accurately than the heat and CO_2 eliminated. *The metabolism varied with oxygen concentration.* This may be true of the cells of all animals. It seems possible that Verzár did not succeed in markedly changing the O_2-tension in the salivary gland, owing to the great store of oxygen in the hemoglobin. There is, however, a distinction between the metabolism of vertebrate muscle cells and Cassiopea. If vertebrates are asphyxiated, the muscles give out lactic acid. A Cassiopea may remain without oxygen for seven hours without giving out CO_2 or any other acid causing a noticeable change in hydrogen ion concentration, although in the presence of O_2 such a change appears in

a few minutes due to elimination of CO_2. After seven hours without O_2, nerve conduction and noticeable contractility of muscle returns in thirty seconds after suspending the Cassiopea in air.

Since O_2-tension may affect metabolism, it seems probable that changes in the threshold of stimulation of the respiratory and vasomotor centers may affect metabolism in man and mammals. The details of the experiments will be published elsewhere.

NATIONAL RESEARCH COUNCIL

SCIENTIFIC PUBLICATIONS FROM GERMANY

By Paul Brockett

At the request of the Chairman of the National Research Council, Dr. George E. Hale, I have prepared the following statement regarding the scientific publications obtained abroad.

Since 1914 the shipments received in this country from Germany have been few and far between, as the number of publications seized by the British Government increased up to the time of the entrance of the United States into the war. This situation has been relieved somewhat by the release of German publications, through the efforts of the Librarian of Congress on behalf of the libraries and educational institutions of the country.

The Librarian of Congress, Dr. Herbert Putnam, has taken a keen interest in the situation and is ready to render aid wherever he can. The first of this year he sent Mr. Theodore W. Koch, Chief of the Order Division of the Library of Congress, to London in the interest of the Library of Congress and the libraries of the United States. Mr. Koch listed the packages held in London, giving the address to which they were consigned, and sent this to the Librarian of Congress, who in turn notified the librarians concerned. Some of the books intended for scientific use were addressed to members of the staffs of the institutions to which they belonged. In such cases no notification was sent by the Library of Congress.

The following extract from the report of Mr. Koch to the Librarian of Congress will give a better understanding of the object of the censorship:

> Two important memoranda were issued in May, 1915 as Parliamentary Papers—one on the Censorship, the other on the Press Bureau. Together they provide the official justification of the Censorship as it affects both the individual and the Press.
>
> This new branch of the government—the Censorship—is described in the memorandum as one of several institutions designed with a threefold object: To prevent information of military value from reaching the enemy, to acquire similar information for the British government, and to check the dissemination of information useful to the enemy or prejudicial to the Allies. When the transmission of correspondence and the publication of news are consistent with the attainment of these objects there is little or no interference. Every endeavor is made to safeguard the legitimate interests, private and commercial of British subjects and neutrals.
>
> In the course of the present war it has become apparent that in the Censorship there lies ready to hand a weapon, the full value of which was perhaps not anticipated prior to the war. It can be used to restrict commercial and financial transactions intended for the benefit of enemy governments or persons residing in enemy countries.
>
> The Memorandum discusses the Censorship as it affects (1) private and commercial communications; and (2) the Press. It states that the censorship of private and commercial communications is under the direction of a general officer who is responsible to the Army Council. The Censorship is organized in two sections: (1) the Cable Censorship under the

control of the Chief Cable Censor, who is a senior officer of the General Staff at the War Office, and (2) the Postal Censorship, controlled by the Chief Postal Censor. In addition to some 120 cables and wireless stations in various parts of the Empire the Chief Cable Censor controls in the United Kingdom messages sent over the cables of the private cable companies. Every twenty-four hours from 30,000 to 50,000 telegrams pass thorough the hands of the censors of the United Kingdom. Exclusive of those in the official Press Bureau, about 180 censors are employed in the United Kingdom in the censorship of cables; elsewhere in the Empire about 400. In the United Kingdom with few exceptions they are retired naval and military officers.

The Memorandum further states that the objects of the Postal Censorship are similar to those of the Cable Censorship. All mails that have to be censored are necessarily subject to some delay, but harmless letters, whether private or commercial, are not detained, even when coming from an enemy country or addressed to an enemy person. No letter, however, addressed to an enemy country can be transmitted unless its envelope is left open and is enclosed in a cover addressed to a neutral country. Letters in which any kind of code or secret writing is used are liable to be detained even if the message appears to be harmless and totally unconnected with the war. In the private branch more than a ton of mail matter is censored every week, exclusive of parcels. Commercial correspondence with certain foreign countries is dealt with in the trade branch and amounts to nearly four tons every week. . . . .

There are still a number of packages of publications held for individuals and institutions in the United States which could be had if proper application were made. Forms for this purpose are given below. Form A should be used where the contents are known. In the event the contents are not known but it is supposed that there are packages held in London, Form B should be made out in a general way and forwarded to the Librarian of Congress for his endorsement. After he has certified that it is in good order, it will be returned to the individual making the application, by whom it should at once be forwarded to the Procurator General in London through the American Consul General at the same place, Mr. Robert P. Skinner.

Form A. *To be used where the contents of the consignment are known.*

APPLICATION FOR RELEASE OF SHIPMENT DETAINED BY THE BRITISH AUTHORITIES

comprising certain books of a philosophical, scientific, technical, or educational character, specifically destined for universities, colleges or public bodies in the United States.

....................191 .

To the Honorable
The Procurator General
London, England.

Respectfully represents the undersigned:

1. That he is....................of the...
....

2. That such institution is a {University / College / Public Body}

3. That it requires for its use from a country now hostile to Great Britain the books whose titles are shown on the attached list marked A, comprising in all...... titles, and the number of copies stated respectively.

4. That he believes there is no one of the said books which may not truly be described as "philosophical, scientific, technical, or educational" in character.

5. That the packet (packets) containing the said books, consigned by the firm of....... at.................... {was / were} shipped by the S. S. ....,— by by {letter mail / parcels post;} but that on or about the......day of............191.. {was / were} seized by the British authorities, and are now detained. {It / They} {is / are} listed as no. (nos.) ....of such seizures.

6. Wherefore he prays that {it / they} may be released and delivered to the London agent of said institution, Messrs. ....or to the American Consul at London, to be forwarded to said institution.

Signed......................,

[*Endorsement.*]

The Library of Congress,
Washington, D. C.,....................191 .

I am satisfied that the within application is genuine and that the volumes covered by it are in fact destined for the use of the {University / College / Public Body.} named.

....
Librarian of Congress.

Form B. *To be used where exact contents of consignment cannot be given.*

APPLICATION FOR RELEASE OF SHIPMENT DETAINED BY THE BRITISH AUTHORITIES

supposed to comprise certain books of a philosophical, scientific, technical, or educational character, specifically destined for universities, colleges or public bodies in the United States.

....................191

To the Honorable
The Procurator General
London, England.

Respectfully represents the undersigned:

1. That he is....................of the..
....

2. That such institution is a {University / College / Public Body}

3. That he is informed that............packet......of books addressed to the said institution by the firm of.................... {was / were} on or about the.....day of...... 191 , seized by the British authorities in the {letter mail / parcels post} on the S. S.................... and {is / are} now detained by them. The packet......bear(s) the number......in the list of seizures.

4. That he cannot state with certainty the contents of the said packet..; but that he believes that {it / they} include(s) no books not within the above categories.

5. Wherefore he prays that {it / they,} or such portion as shall be found to be within the said categories, shall be released, and delivered to the London agent of the said institution, Messrs. ....or to the American Consul at London, to be forwarded to said institution.

Signed.........................

[*Endorsement.*]

The Library of Congress,
Washington, D. C.,....................191 .

I am satisfied that the within application is genuine and that the volumes covered by it are in fact destined for the use of the {University / College / Public Body} named.

....
Librarian of Congress.

The forms given are for the securing of mail matter held in London and do not apply to publications ordered through agents which have been held in Rotterdam. Part of this lot has already been shipped to this country, and for the remaining 102 cases shipping permits have been granted, and these will be forwarded as soon as ships can bring them. It will, therefore, not be long before all this material will be in New York, where it will be passed upon by a representative of the Department of Justice and a member of the staff of the Library of Congress. After that the publications will be delivered to those for whom they were intended.

Regarding the securing of more recent German publications, I quote the following from a letter of Dr. Herbert Putnam, Librarian of Congress:

> . . . . But since the entrance of the United States, there has been no attempt to continue this procedure, the only effort made being to secure the release of material already seized by the British authorities or detained at Rotterdam. Among our libraries, generally, it is assumed, I think, that if not a trading with the enemy act, other considerations would necessarily suspend the purchase of books and periodicals from the enemy countries. I suppose, however, that there are German and Austrian publications the actual need of which by our scientific investigators, even for the work which they are doing in the general defense, should be held to outweigh the objections to trading with the enemy. While we were still neutral, the British Government offered to secure such publications through its own (H.M.) Stationery Office. I suppose that that offer would still hold good, and that an American

university, college, or public body, requiring a German publication of a philosophical, scientific, technical or educational character, which it could conscientiously import in spite of the trading with the enemy act, might place an order for it with its London agent, who, under the above arrangement, would submit it to H. M. Stationery Office, the latter securing and delivering it to that agent.

The procedure would involve delay and some additional expense, the Stationery Office charging a penny in a shilling for its labor in the matter. . . . .

The Department of State has recently instructed the Ambassador at London to report on the procedure which is followed by the British Government in securing needed publications of German origin with a view to making an effort to have a similar procedure put into effect here.

NATIONAL RESEARCH COUNCIL

REPORT OF THE GEOLOGY AND PALEONTOLOGY COMMITTEE

At the organization of this Committee in January, 1917, its Chairman had been authorized by the Geological Society of America to appoint a Committee which should be charged with the effort to fortify the instruction in geology given in civil engineering training in the technical schools of the country. With the approval of the Executive Committee of this Council, this function was taken over as proper to the present Committee, and in this capacity it has served as such Committee of the Geological Society of America. Upon this phase of its activity and on favorable results secured by assurances from various engineering schools of high standing, a report has already been made to the geologists of the country. Changed conditions since the entry of the country into the war may delay, for a while, the perfection of the program urged by the Committee.

As a preliminary procedure, a census was made of all the geologists of the United States, with special reference to their training, their special investigations in progress, special lines of research planned, effective lines of possible public service and the regions of the United States with whose topography and geology they had an intimate acquaintance. This has served as the basis of correspondence, reference and inquiry in the course of the organization.

To make clear to the military officers the nature of the service which geologists could render, a brochure entitled *What a Geologist Can Do in War* was printed in a large edition and freely distributed among the officers. This was a very brief statement in entirely untechnical language, put together in convenient form for the pocket, and since the original circulation of it, the demand for copies from various sources has been large.

1. *Materials and Facilities for Rapid Road and Fortification Construction.*—The purpose of this undertaking has been to bring together and organize for the use of the Engineers Corps, outstanding data bearing on the natural available supply of material for quick road and fortification construction along the Atlantic seaboard; to indicate upon maps the locations of immediately avail-

able supplies of every sort, and to tabulate for prompt reference the capacity of all commercial workings.

The area covered includes all of the states from Maine to Texas, with an intensive detailed study of certain states covering a belt from 10 to 20 miles back from the coast line. The work has been performed gratuitously by geologists familiar with the local conditions in each of the states in association with highway engineers representing the American Association of State Highway Officials, the full reports by states covering (*a*) the topography, climate, and geological conditions; (*b*) a digest of the active and inactive quarries, quarry sites, sand and gravel pits, rubble piles, and stone walls where materials may be obtained quickly; (*c*) a summary of road machinery available at given points, and a list of engineers with their qualifications for undertaking road and bridge construction; (*d*) and a series of maps representing this information cartographically. Two sets of the detailed reports for file in the general and departmental archives have been prepared in bound volumes and atlas cases for ready reference. The reports have been abstracted and conditions summarized by Military Departments and State Divisions.

Eventually, and because of the value of the compiled data, the entire work will, it is hoped, be put in permanent form.

2. *Measurement of Earth Vibrations as a Means of Locating Heavy Batteries*.—It was early suggested that a perfected seismographic apparatus might be devised which would be competent to detect and measure, by earth vibrations, the distance of heavy artillery discharges. The matter was studied carefully by the most competent seismologists, and Dr. Reid, of the Committee, on his trip to the war front, was specially commissioned to look into any actual or possible applications of this service. The service which it was thought ought to be rendered by seismic ranging, is now being given by sound ranging and airplane observation. Dr. Reid, however, a member of this Committee, has given successful attention to the means of correcting the troublesome compass variation in rapidly-moving airplanes, in which work he has coöperated with the Committee on Physics of the Research Council.

3. *Water Supply for Camps*.—This subcommittee was organized under the title of *Camp Sites and Water Supply*, in the expectation that the knowledge the members were competent to supply would be of service in the selection of camp sites, with reference to quality of soil, topography and underdrainage, ground water and deep water distribution. In the location of the present camps this knowledge was only incidentally used, it being assumed that other agencies were adequate.

As the military camps are now located, the subcommittee retains its organization to meet any demands for information regarding water supply or other questions which may develop with the growth and further movements of the Army.

4. *War Minerals*.—The Subcommittee on Imported Minerals, instituted by this Committee, has merged into an independent Committee on War

Minerals, constituted to represent the American Institute of Mining Engineers, United States Geological Survey, United States Bureau of Mines and the National Research Council.

As its operations combine the joint activities of several organizations, it is transmitting its reports through the organizations in its representation. The Committee has been effective for the past four months, in which time it has taken a census of the minerals required in war preparations of all kinds, particularly with reference to amounts imported, stock on hand, production, active and suspended, possible enlargement of production; and it has inaugurated surveys in several states, partly under Government and partly under state auspices, for the purpose of perfecting data of present and increased production, to determine new ore bodies and invite prompt exploitation thereof. The work is important; it has already instigated wide-spread activity throughout the country in the search for mineral supplies; it has ascertained with approximate accuracy the most imperious exigencies of these war industries; it has led to direct recommendations to the Secretary of War; and it is effectively aiding the labors of the War Industries Board.

5. *Pacific Coast Sub-committee on Geology.*—This has recently been organized for active service. Its personnel is made up of men representing the states of California, Nevada, Oregon and Washington, and in its Chairman and several of his associates has the benefit of experience and achievement in connection with the scientific work of the State Defense Council of California.

6. *Geology of Cantonments and Geological Instruction in Training Camps.*—This sub-committee has recently been organized and the work it has laid out consists in:

(*a*) The preparation of topographic and geological maps of the cantonments and camps and their environs, for a radius of about 20 miles.

(*b*) Preparation of descriptive matter explanatory of such maps, to be printed, both with them and in separate pamphlet form.

(*c*) Provision for instruction in geology and physiography, to be undertaken in connection with the mobilization and training camps.

Each phase of this work has been initiated. The preparation of the maps and their description are being carried out by the Government and the State Geological Surveys. The courses of instruction, if approved by the War Department, will be given in most cases by geographers and geologists who are conveniently situated with reference to the geographical location of the camps.

7. Among the earliest suggestions of service before this Committee was the possibility of constructing a superior armor for the National Army, on the basis of the construction in the armored Devonian fishes.

In conjunction with the Sub-committee on Protective Body Armor of the Engineering Committee of the Council and under the direction of the Ordnance Bureau, Dr. Dean, curator of the Department of Arms and Armor in

the Metropolitan Museum and of Fossil Fishes in the American Museum, a member of this Committee, has designed effective models for new armor which are now being manufactured for trial in actual field operations.

8. The hope of the Committee has been to attach a geologist in an advisory capacity, to each military unit of large size. This end has not yet been attained; some geologists, former members of Officers Training Camps, hold commissions in France with the Commanding General; a number are now in active service here, and under instructions from the Secretary of War, a list has been compiled of all geologists willing to accept commissions in any department of the Army. This list is a large one and the intimations of the Secretary of War are that, as the military activities develop, additional calls will be made for active field service by the geologists.

9. The 'Plan of Service' submitted by the Committee has now been approved, in general, by the Engineer Officers and the Secretary of War. This plan of service laid special emphasis on the importance of our ability to interpret soil and rock conditions in relation to trenchability, tunneling, and the location and control of ground water.

J. M. Clarke, *Chairman*

W. W. Atwood	J. C. Merriam
C. P. Berkey	R. A. F. Penrose, Jr.
A. L. Day	H. F. Reid
Bashford Dean	C. R. Van Hise
F. W. DeWolf	C. D. Walcott
W. O. Hotchkiss	J. B. Woodworth
E. B. Mathews	

NATIONAL RESEARCH COUNCIL

FIRST REPORT OF COMMITTEE ON ZOOLOGY

1. *Organization of Committee.*—In organizing the Committee on Zoology the attempt has been made to select its members so as to represent various branches of Zoological research as well as different sections of the country and at the same time not to have the Committee so large or so widely scattered as to be unworkable. The Zoological Committee of the Committee of One Hundred on Scientific Research of the American Association for the Advancement of Science was incorporated in the present Committee on Zoology and a few other persons were added. As the work develops it is expected that other members will be added to the Committee and that Sub-committees will be formed in special branches of the science. At the present time the membership of the Committee on Zoology consists of the following persons, whose addresses and principal scientific interests are given after their names:

D. G. CONKLIN, *Chairman*, Princeton University, Embryology, Cytology.

S. A. FORBES, University of Illinois, Entomology, Ichthyology, Ornithology.

C. A. KOFOID, University of California, Protozoology, Parasitology.

F. R. LILLIE, University of Chicago, Embryology, Marine Biology.

T. H. MORGAN, Columbia University, Experimental Zoology, Genetics.

G. H. PARKER, Harvard University, General Zoology, General Physiology.

J. REIGHARD, University of Michigan, Ecology, Ichthyology.

H. M. SMITH, Commissioner of Fisheries, Washington.

As now constituted, the Committee represents predominantly pure zoology as contrasted with applied and universities rather than technical institutions. This is due in part to the fact that there has hitherto been a greater lack of cooperation and organization in pure zoology than in applied and in part to the facts that applied science must be developed on a foundation of pure science and that universities must supply most of the investigators for all lines of scientific research. Many zoological subjects of economic importance, not now represented directly on this Committee, are organized under separate bureaus, divisions or surveys of the general Government or of the several States—such as the United States Bureaus of Animal Industry, Biological Survey, Entomology, Fisheries, and the Agricultural and Biological Stations and the Fish and Game Commissions of the various States. Furthermore, the Committees of the Research Council on Agriculture, Medicine, and Physiology deal with many problems which are essentially zoological. Many zoological investigators are at work in fields occupied by these different organizations and to a large extent the work and the workers which are left to the Committee on Zoology represent the residue of zoological science after these more practical subjects have been taken away.

Our Committee has attempted to establish cooperative relations with many of these organizations and to avoid conflicts with all of them. Wherever work is already being done by any existing organization it is the desire of the Committee on Zoology to assist in that work in any way possible and not to duplicate it. The members of the Committee are in contact with or have knowledge of the majority of the zoological investigators of this country and in many cases it would be possible for the Committee to recommend to these National or State organizations investigators or students for particular lines of work or to find laboratory facilities for the investigation of special problems.

2. *Suggestions for National Service.*—During the war the immediate needs of the nation take precedence over all other aims and the practical and economic applications of research must to a large extent supersede the extension of knowledge for its own sake. Many zoologists have turned from their customary duties to some form of national service; several are doing important work with the Food Administration, others have taken up research work in some of the governmental bureaus or State Commissions, still others have

offered their services to the nation through the Committee on Zoology. In response to many inquiries from zoologists as to how they might be of service in this national emergency the Committee on Zoology published in *Science*, volume 45, number 1173, "Some Suggestions for National Service on the Part of Zoologists and Zoological Laboratories," from which the following is extracted with certain modifications and additions:

The greatest national service which the biological sciences can render in war as well as in peace is in conserving human life, in protecting and improving useful animals and plants, and in controlling or destroying injurious ones; when it is remembered that practically everything which we eat or wear comes from animals or plants it will be realized that this last is no slight service.

Many of the practical and economic branches of biology have long been well organized for public service and this applies particularly to medicine, sanitation and agriculture; in each and all of these branches the trained zoologists may render valuable aid. Probably no other non-medical men are better prepared by training and no other institutions better fitted by equipment to assist in medical and sanitary work than are zoologists and zoological laboratories, and in the matter of the propagation and improvement of useful animals and the destruction of useless or injurious ones zoologists should be especially at home. In many instances zoologists who have hitherto confined their attention to theoretical and general problems would need to turn their attention to new lines of work, but it can not be doubted that experience in solving general and theoretical problems would be of great value in dealing with special and practical ones.

3. *Sanitary Work*.—(*a*) Much sanitary work is primarily zoological as, for instance, the study of the life histories of parasitic protozoa, tapeworms, flukes, roundworms, insects, mites, etc., together with methods of their control or eradication.

(*b*) The elimination or control of animal pests, which are often carriers of disease-germs, such as flies, mosquitoes, bugs, lice, rats, etc.

(*c*) Assistance in medical diagnosis, as in the microscopical or chemical examination of blood, urine, feces, sputum, etc.

(*d*) Microscopical or chemical examination of water and soil of camp-sites, drainage-areas of cities, etc.

(*e*) The zoological aspects of the collection and disposal of garbage and sewage.

(*f*) "The effects of sewage contamination upon the system of life and hence upon the economic productivity of specific waters, especially of important rivers, and a program of management, calculated to gain and retain whatever economic advantages, and to avoid whatever disadvantages, the contamination of streams by sewage may involve or entail." (Letter from S. A. Forbes.)

In view of the importance of zoological science in dealing with old and new

problems which will arise in connection with sanitation, it would be very desirable to have at least one trained zoologist connected with the medical staff of each mobilization camp.

4. *Agricultural Work.*—(*a*) Application of principles of heredity to the improvement of breeds of domestic animals.

(*b*) Study of physiology of reproduction with a view to increasing productivity in animal breeding; for example, improved methods of incubation, brooding and rearing of fowls; better methods of increasing egg laying, especially during winter months; the serious problems of abortion in cattle, small litters and destruction of young among swine, etc.

(*c*) Determination of standards of feeding and care of domestic animals for best general or specific results and for greater economy.

(*d*) Cooperation with the agricultural agencies of the states and nation in the elimination of animals which prey upon or are parasitic upon domestic animals; of animal pests destructive to crops, fruits, forests,—to stored vegetables, grains and other food supplies,—to clothing, woodwork and other manufactured products. The annual losses due to such animal pests are probably not less than the enormous taxes levied for the prosecution of the war; here is a perennial tax which would relieve us of all national taxation if it could be saved and applied to that purpose; here are problems which call for research work of the highest order and for an army of investigators.

(*e*) The protection of insectivorous birds and the propagation of enemies of injurious insects. It is estimated that in Kansas alone the annual preventable damage to food crops amounts to thirty million dollars. The largest natural elements in the prevention of this loss are ground-nesting birds. Migratory birds should be protected by the passage of the "Migratory Bird Treaty Act."

In this connection the Chief of the Biological Survey has written:

"The National Stock Growers Associations estimate that the annual losses in live stock from predatory animals in the Western United States amount to from $12,000,000 to $15,000,000. The directors of extension work in the Western States estimate that the losses in cereal and other crops through depredation of noxious rodents in the four states of Montana, North Dakota, Kansas and California, amount to between $50,000,000 and $60,000,000. I have determined that, at a conservative estimate, the damage done by house rats each year amounts to $200,000,000. This involves the continuous labor of more than 200,000 men. This is entirely apart from the very serious losses and deaths through the disease carrying proclivities of these animals. The work of conserving and increasing our migratory birds through the Migratory Bird Treaty with Great Britain is another important activity of this Bureau. The Canadian Government is now proceeding to secure enactments necessary to make the Migratory Bird Treaty effective on the recommendation from Government officials that to do so will assist in protecting the crops through the better protection of insectivorous birds."

5. *Better Utilization of Wild Animals.*—(*a*) Preservation, propagation and domestication of useful wild animals. The game resources of the country can be fostered and enormously increased with intelligent effort in the right direction, thus adding not only to the pleasure and health of a large number of people, but distinctly increasing the national wealth by enlarging the food supply. The fur trade could once more be made a great national industry. Some useful animals, now wild, could be brought into captivity or domesticated and by selective breeding could be greatly improved. Probably certain useful birds and mammals of other countries might be imported under proper precautions.

(*b*) Exploitation and propagation of useful marine and fresh-water animals in cooperation with the Bureau of Fisheries. There is an unlimited supply of food in the oceans of the world and we have scarcely begun to reap the "harvest of the seas." Countless forms of fishes, crustacea, mollusks and other types which are not now generally used as food are both wholesome and delicious when properly prepared. The Commissioner of Fisheries says:

"Zoologists may perform a service by bringing to the attention of people, in the course of their conversations, lectures, etc., the reasons for looking to the fisheries for increased food supply, the wholesome character of the meat, the economy with which fish are produced without dependence upon agriculture for their food. Many new fishes are being introduced into the market,—sharks, howfin, burbot, sable-fish and others,—and it is certain that zoologists can do a good service in helping to overcome popular inertia that will be encountered. The Bureau will gladly send circulars announcing new fishes to any who apply.

"The biological problems of fish ponds are numerous. Recently a college zoologist in association with the Bureau began giving special attention to the relation of dragon flies and damsel flies to fish culture in ponds. Already he has gained results that were unexpected, but that are highly significant. The larvae of dragon flies were known to prey upon fish fry, but this investigator finds that they also prey in greater measure upon other insects that are more effective enemies of fish fry; various other interesting interrelations are discovered. This is only an illustration of what may be done with various groups of aquatic and semi-aquatic animals and plants. Results of value may, in some cases, be obtainable in a brief space of time.

"We know very little about the parasites of fishes, their relative abundance under different conditions of environment, their life-histories and alternate hosts. Means of control can not be devised without more complete knowledge regarding particular species.

"We should be glad to advise either directly or through you with any zoologist who is considering a particular problem related to fisheries."

(*c*) Prof. S. A. Forbes suggests the following very important investigation: "The effect on the productivity of streams and their dependent waters traceable to operations for the agricultural reclamation of bottomlands, to-

gether with a plan for the management of our more valuable streams such as to compensate so far as practicable for the losses due to such reclamation. We have a problem of this kind on the Illinois River, the normal productivity of which has fallen off to about a third its former rate in consequence of diking and drainage operations on its bottomlands; and I have been unable to find any agency, either state or national, in position to handle effectively the practical problem of the managment of the stream and the development of the resources remaining after this inevitable reclamation process has gone to its limit. It is primarily a fisheries problem and all aware of the facts admit that it should be worked out, but neither State nor United States fisheries commissions are able to undertake it. The consequence is a considerable and increasing loss of food to the country at large, since the product of this stream is disposed of chiefly in the principal cities of the eastern United States."

6. *Educational and Social Work.*—(*a*) Thorough studies of human heredity as a necessary preliminary to any attempt to permanently improve our human stock and increase our national efficiency.

(*b*) The teaching of zoology may be made an especially important means of promoting national intelligence, cooperation and welfare, for in many respects zoology comes nearer to man and his problems than does any other physical or natural science; it is indeed the foundation or background of human studies, since man also is an animal. There are many important lessons for mankind in the heredity, development, evolution and adaptations of animals as well as in the organization of animal states.

(*c*) The laying of broad zoological foundations for medical education and research must always be an important part of the work of the zoologist. While the present demands on medical men continue zoologists would be well qualified to assist in the more fundamental subjects of medical education, especially in anatomy, histology, embryology and neurology.

(*d*) The prosecution of research work, whether in pure or applied science, is a national duty of the first magnitude; the continuance of research work in zoology, and especially of work already begun which cannot be interrupted without serious loss to science, is a real national service.

7. *Miscellaneous Problems.*—In addition to these general lines of work the following special problems have been suggested:

(*a*) The microscopical inspection of food, clothing and supplies.

(*b*) Studies of the coat coverings of animals with a view to the utilization of nature's principles in making the clothing of soldiers light, warm, well-ventilated, impervious to water and protectively colored.

(*c*) Studies of the mechanism of aquatic and aërial locomotion in animals with reference to its application to submarines and aëroplanes.

(*d*) Utilization of gulls and other aquatic birds in locating submarines.

(*e*) Studies of the mechanisms of limbs and joints with a view to offering suggestions in the construction of artificial limbs.

(*f*) Investigation in tissue cultures, grafting and regeneration, with a view to their surgical applications.

(*g*) The protection of submerged timbers from depredations of ship worms and the prevention of the fouling of ship bottoms by attachment of marine organisms.

8. *Activities of the Committee on Zoology.*—In carrying out some of these suggestions members of the Committee have recommended to the Government investigators trained in the study of certain important human parasites which are not widely known; one member of the Committee has offered his services for the study of intestinal flagellates which cause trench diarrhea; zoologists trained in microscopical technique and parasitology have been sent into medical and hospital service. The suggestion that at least one zoologist especially familiar with animal parasites should be assigned to the Medical Staff of each mobilization camp has not been acted upon so far, but it seems to the Committee that this should be done as soon as possible.

Some zoologists and zoological students have been cooperating in the campaigns against animal pests which have been organized by the Bureau of Entomology, the Biological Survey and by Commissions of certain States. Others have been employed by the Bureau of Fisheries in exploiting new sources of marine food and in devising new methods of curing and preserving food fishes.

One member of the Committee has been making experiments in consultation with naval officers, on new methods of protecting ship bottoms from fouling due to the growth of marine organisms. Several zoologists have been interested in the problem of utilizing gulls in locating submarines. It seems probable that by feeding gulls from a submerged submarine, practically all of these birds along our coast could be trained within a brief time to hover over submarines and thus reveal their location. There is some reason to believe also that individual gulls could be trained to work from ships at sea to which they would return. In this connection the Commissioner of Fisheries writes: "The great success of the Japanese in training wild cormorants for the service of man should be borne in mind."

Finally, the members of the Committee in common with many other zoologists, have been actively engaged in research and in the promotion of research chiefly in non-applied lines. Probably the output of zoological research in this country was never greater and the facilities for carrying on such work were never better than in the year preceding our entrance into the war; nevertheless conditions in these respects can be greatly improved and the Committee on Zoology of the National Research Council considers that one of its main functions is to prepare for research work in all branches of zoology in the period after the war. To this end one of the first duties of the Committee will be to make a census of workers, laboratories, publications, fellowships, assistantships, etc., together with a list of the most general needs of

research work in zoology. The Committee will undertake this work as soon as possible and in the meantime it invites from every zoological investigator in the country a statement of the things most urgently needed for the promotion of his own research work.

For the Committee, EDWIN G. CONKLIN, *Chairman.*

NATIONAL RESEARCH COUNCIL

THE SCOPE AND WORK OF THE BOTANICAL RAW PRODUCTS COMMITTEE

At the twenty-fourth meeting of the Executive Committee of the Council held on July 12, 1917, the appointment of a Botanical Raw Products Committee was approved. This committee has since been organized with Edward M. East, Chairman, and Oakes Ames, L. H. Dewey, H. M. Hall, Henry Kraemer, A. D. Little, George T. Moore, W. W. Stockberger and W. P. Wilson, members. It is designed to serve as a clearing house where manufacturers needing raw products of a botanical nature may obtain information regarding them. The scope of its work may be outlined somewhat as follows:

1. The collection of agricultural, botanical and commercial data on all species and varieties of plants having an economic value (exclusive of food staples).
2. Dissemination of such information among importers and manufacturers.
3. Investigation of requirements of the trade for known raw materials.
4. The discovery of new geographic sources of plants necessary to the trade.
5. The development of plans for meeting the needs of industry by the cultivation of economic plants in the United States.
6. The initiation of investigations calculated to discover the value of conventional equivalents and substitutes for raw products of a botanical nature.
7. The discovery and investigation of the value of new equivalents and substitutes.
8. The investigation of the requirements of the trade for new raw materials.
9. The suggestion of new species as possibly meeting trade requirements; and the initiation of the proper investigations as to whether or not they meet these requirements.
10. The suggestion of new uses for botanical raw products.

Owing to the magnitude of the work proposed, there being over 25,000 species and varieties of plants having an economic value (exclusive of agricultural and horticultural novelties), it would be some time before active work

as a clearing house for manufacturers could begin were it not that at the commencement of its labors there was already available to the committee a large amount of data on special subjects gathered and catalogued at various research institutions. It is therefore already on a working basis, both in its advisory and its research capacity.

That there is great need for a work such as this hardly requires demonstration. Exclusive of foods, numerous botanical raw products are very important to our industries. There are gums and resins, rubbers, vegetable fats and oils, vegetable dyes and tannins, fibres, cellulose, drugs and herbs, essential oils and perfumes, and possibly most important of all, forest products. A great number of facts have been discovered about many of these products, but in too many cases even the name of the species from which the raw material comes is uncertain, obscure or unknown. Very often a great industry buys its raw material from a broker or an importing house without knowledge of either the geographic or the specific source. When this source is cut off, as has frequently been the case during the past three years, and as possibly will be more frequent during the next few years, the manufacturer has been placed in an uncomfortable position. Curiously enough, such a predicament is many times brought about by the curtailment of a products used in such relatively small quantities that the fact that it is essential to the finished article is overlooked or forgotten during times of plenty.

The Botanical Raw Products Committee, if it gives the service expected of it, must answer questions concerning all such materials. To do this, data are required along five different lines: botanical, agricultural, industrial, commercial and bibliographical.

One should know the correct scientific name together with the scientific synonomy, the published descriptions, original sources and plates. He should have at hand the native names, for, though often confusing, they are frequently the sole clues to the identity of commercial specimens. No less important is the history, the morphology, the physiology and the geographical distribution both of the plants themselves and of their near relatives.

Pertinent agricultural facts are those regarding varieties, their types and origin; cultural requirements, including data on soils and fertilizers, climate, temperature, moisture, planting and cultivation; harvesting and storing; diseases and their treatment; and pests and their control.

Industrial data are still more complicated. One must know all of the economic uses of a plant, and oft-times these are varied as well as numerous. The products go under many aliases, both trade names and native names. These must all be listed. Various methods of preparation must be entered. Data on yields, grades and values, must be assembled. The raw products themselves must be identifiable, and methods of detection frequently must be worked out. And even here the work does not end. Equivalents, substitutes and adulterants must be described and tabled, and the uses for which

they are fitted and for which they are unsatisfactory investigated. Many of these facts are obtainable only if commercial firms offer their hearty cooperation and support. They are not trade secrets but pertain to specialized industries and are not usually available in published form.

These agricultural, botanical and industrial data, if brought together in a systematic manner, will have a lasting value and will serve as a basis for placing economic botany on the high plane of usefulness that industrial chemistry has held for many years. But they will not meet the practical requirements of economic life unless considered from the commercial viewpoint. They must be supplemented by all available statistics concerning the importation and exportation of each product, and the prices current through a term of years.

Finally, there must be a reference library department. Information is of little use unless it is systematized in such a manner that it is readily available. Adequate cross reference catalogues containing citations of the best literature must therefore be kept up to date.

This, in a general way, is the work which the Botanical Raw Products Committee must do to be in a position to act as an industrial service bureau. This is the work which, though it must be continuous, though it never reaches completion, is really preliminary to the main activities listed above. True service must come from actual contact with the technical problems of manufacturers and importers.

For the Committee, EDWARD M. EAST, *Chairman.*

NATIONAL RESEARCH COUNCIL

MEETINGS OF THE EXECUTIVE COMMITTEE

The thirtieth meeting of the Executive Committee convened in the offices of the Council in the Munsey Building, Washington, D. C., on September 13, 1917, and was called to order at 3.00 p.m. by the Vice-Chairman, Mr. Millikan.

Messrs. Bogert, Dunn, Millikan, Noyes and Vaughan were present, and; by invitation, Messers. Durand, G. N. Lewis, Manning, and Mendenhall.

Discussion took place with regard to the advisability of extending the scope of the Research Council, so to have it serve, if practicable, as a means to bring about the coördination and centralization of research activities carried on in connection with the war in different bureaus of the Departments of the Government. Mr. Millikan outlined the development of the work of the Council in its capacity as an advisory agent for the scientific services of the Signal Corps and presented the following statement which has been prepared with the approval of the Chief Signal Officer as expressive of existing relations:

The Science and Research Division of the Signal Corps, established on July 16 under the direction of the National Research Council, of which Major Millikan is the Executive Officer,

is expected to keep in close touch with all of the divisions of the Signal Corps in which research is carried on or is needed. Its status and connections are shown in the accompanying diagram. Its functions are twofold.

1. To furnish personnel of the research sort to the other divisions when the stiuation warrants attaching men of this type to these divisions.

2. To have a personnel of its own which keeps in intimate contact with all research development work in other divisions, such as new methods, new devices, new instruments and distributes research problems to the research laboratories of the country, university, industrial or governmental, with which it is associated.

Officers in charge of various divisions and services of the Signal Corps in discharging responsibility for the effectiveness of their respective services are expected to make use of the Science and Research Division to the fullest possible extent.

By order of the Chief Signal Officer.

Upon motion this statement was approved by the Executive Committee.

Mr. Manning suggested that it might be desirable for the Council to address a communication to the chiefs of technical bureaus of the Government, offering assistance of the Council in securing adequate representation and coöperation in research. He thought it would be desirable for a scientific representative to be appointed on the general staff of the Army and to have subordinate representatives to aid in the correlation of the research work of the bureaus of the War and Navy Departments.

Mr. Millikan reported that the Engineering Foundation had appointed a special committee consisting of Messrs. Pupin, Goss and Rand to confer with the National Research Council with regard to relations which may appropriately exist between these organizations in the future. Upon motion a special committee or three members of the Research Council was correspondingly authorized to confer with the special committee of the Engineering Foundation on this subject. Mr. Millikan was appointed chairman of this committee and Mr. Carty as a member, these two gentlemen being given the power to name the third member of the committee.

The meeting adjourned at 4.20 p.m.

The thirty-first meeting of the Executive Committee convened in the offices of the Council, Munsey Building, Washington, D. C., September 19, 1917, and was called to order at 9.05 a.m. by the Chairman of the Committee, Mr. Carty.

Messrs. Carty, Dunn, Millikan, Noyes, Pearl, Stratton, Vaughan, and Welch were present, and by invitation, Messrs. Durand, Jewett, Manning, Mendenhall, Paton and Watson.

The minutes of the regular monthly meeting of the Committee of August 22 and of special meetings of the Committee of September 5 and September 13 were read and approved.

The Executive Officer reported:

1. That a lease for the year ending August 31, 1918, was signed by the

Executive Officer of the Council on September 10 for rooms 323 to 335, both inclusive, in the Munsey Building, Washington, D. C.

2. That Messrs. Dunn and Noyes have been added as members of the special committee of the Council, appointed at its meeting of September 13, to confer with a committee of the Engineering Foundation with regard to future relations between these organizations.

3. That Dr. L. R. Williams and Dr. R. P. Strong, members of the Foreign Service Committee of the Council, have submitted extensive reports with reference to their observations abroad on sanitary and medical questions. Copies of Dr. Williams' report have been distributed to officials of the Government, of the American National Red Cross, of the Rockefeller Institute for Medical Research and of the General Medical Board of the Council of National Defense, and numerous papers and documents which accompanied the report have been deposited with the last named organization. Steps are being taken to make a similar distribution of Dr. Strong's report and a similar disposition of his material.

4. That an additional amount of $5,000 has been deposited by the Carnegie Institution of Washington in the Riggs National Bank to the credit of the National Research Council from the appropriation of $50,000 made for this purpose by the Carnegie Corporation of New York.

5. That the Chairman of the Committee on Gases used in Warfare has reported that the Surgeon General of the Navy has designated P. A. Surgeon Joseph R. Phelps, U. S. N., on duty in the Bureau of Medicine and Surgery, as a member of this Committee to take the place of Medical Director James D. Gatewood, U. S. N., detached, and that the Chief of Engineers, U. S. A., has designated Captain Earl J. Atkinson, Corps of Engineers, to represent the office of the Chief of Engineers at meetings of the Committee on Gases used in Warfare.

A financial statement was presented, showing expenditures and balances available in the three bank accounts of the Council.

After explanation by Mr. Welch, it was voted that, acknowledging receipt of reports submitted by members of the Foreign Service Committee, the Executive Committee extends its appreciation of the admirable work accomplished by the separate members of this Committee.

Mr. Paton, upon invitation, submitted a report with regard to the proposed coöperative work of the Sub-committee on Psychiatry on the subject of shell shock and the desirability of the maintenance of intensive work of this character abroad.

Mr. Millikan presented an outline of the present activities of the Physics and Submarine Committees of the Council and explained the features of the work of the Council in coöperation with the U. S. Signal Corps, particularly as evidence of desirable relations which may similarly be established with other bureaus of the War and Navy Departments.

He reported that the following recommendations have been submitted to the Chief Signal Officer:

1. That First Lieutenant John Q. Stewart, A. S. Signal O. R. C. be placed on active duty and detailed to Princeton for work in the sound-ranging service.

2. That Mr. Bertram A. Sherry be commissioned as First Lieutenant in the Aviation Section, to assist in the development of the Aerological Service.

3. That in connection with requests received from France for the detail of twenty-five scientists to work in French laboratories,

(*a*) Major R. W. Wood proceed to France to work in coöperation with Langevin or else under the direction of the Interministerial Commission.

(*b*) Messrs. Roy W. Chestnut and Samuel Sewall now in Paris be commissioned as Lieutenants and assigned to the laboratory of Professor Perrin, Director of Inventions.

(*c*) Dr. Leonard Loeb be commissioned as a lieutenant and sent to France for assignment to service by the Interministerial Commission.

He also reported that further recommendations with regard to the detail of men in physics and radio-telegraphy for services in France would be deferred until a report may be received from Maj. Wood with regard to the urgent need for such men and that requests for men in bacteriology, chemistry and hygiene have been provided for through the office of the Surgeon General.

A communication from E. M. East, Chairman of the Botanical Raw Products Committee, was read, urging the allotment of funds by the Council to assist the work of this Committee and the need of securing closer relations with governmental activities. Upon motion of Mr. Noyes, an allotment of $100.00 was made for use by the Botanical Raw Products Committee.

Mr. Pearl submitted a communication from Irving W. Bailey, Chairman of the Sub-committee on Forestry of the Botany Committee urging reconsideration of the action of the Executive Committee in appointing this Subcommittee and recommending a proposed enlargement in the scope of the work in forestry under the auspices of the Research Council by the creation of a special Forestry Committee with appropriate sub-committees, so as to encourage desirable coöperation in research with existing agencies. The relation of this proposed Forestry Committee with the Botanical Raw Products Committee and with work in botany and agriculture was also discussed, and, upon motion of Mr. Vaughan, it was finally decided to refer these questions with power to a special committee consisting of Messrs. Pearl, Chairman, Bailey, Coulter, East and Millikan, the decisions of this Committee to be reported at the next regular meeting of the Executive Committee.

Discussion took place concerning coöperative relations which may appropriately exist between the Council and the National Food Administration, in order that members of the Council who are actively interested in food research problems may be prepared to urge such coöperation whenever an appropriate time arrives.

Messrs. Millikan and Stratton submitted brief reports relative to recent developments in procuring an adequate supply of optical glass for purposes of the Government.

Upon nomination of Mr. Durand, Chairman of the Engineering Committee, the following Sub-committee on Protective Body Armor was appointed:

Bashford Dean, Chairman, Col. E. B. Babbitt, G. O. Brewster, G. K. Burgess, W. F. Durand, Henry M. Howe, Edward H. Litchfield, Clarence Mackay, Ambrose Monell, Thomas Robins, David B. Rushmore, Capt. A. T. Simonds, Alexander McM. Welsh and W. H. Wilmer.

He also rendered a report with regard to the prospective activities of this Sub-committee in undertaking coöperative work with the Government concerning advisable types of individual armor.

Mr. Watson presented a statement concerning the activities of the Psychology Committee, which led to a discussion of methods employed in the selection of men for aviators. Upon his recommendation, with the endorsement of the Executive Officer of the Council, a second allotment of $100.00 was voted for use by the Psychology Committee for maintenance of its office in the Munsey Building.

Mr. Pearl submitted a report with regard to the work of the Agriculture Committee and stated that the Council of National Defense had made an appropriation for the necessary expenses of this work. He also stated that, with the approval of the Secretary of Agriculture, this Committee is now recognized as the official coördinating body for agricultural research in the United States.

Mr. Durand reported that the special committee on the Patent Office situation had conferred with the new Commissioner of Patents. He submitted the following recommendations:

That the National Research Council undertake to organize a committee for the purpost of making a preliminary survey of the problems presented by the work of the U. S. Paten Office. The purpose of this committee would be the following:

1. To make a preliminary study of the character and scope of the investigations needed for an adequate study of the problems presented by the work of the Patent Office.

2. To recommend means adequate for carrying forward such a study.

3. To consider and recommend whether such investigations should be undertaken at the present time and under war conditions, or whether they should be deferred to a later time and under peace conditions.

After discussion it was decided to take a broad view of the functions which such a committee might be expected to assume, and upon motion the present committee appointed to deal with this subject, consisting of Messrs. Durand, Millikan and Stratton, was given power to enlarge its membership to deal with the problems presented in accordance with the above recommendations.

Mr. Dunn submitted a report concerning the relations of the Engineering Committee of the Council to other agencies engaged in engineering work and in the performance of functions which may be similar in character to those undertaken by this Committee, and Mr. Durand submitted a brief outline

of the activities of the Engineering Committee as explained to members of this Committee at its recent meeting in New York.

Discussion took place concerning the preparation and publication of literature regarding the work of the Council, and upon motion, it was decided to issue two pamphlets in such form that they may be suitably bound or fastened together; one of these pamphlets to contain a complete list of the officers and members of the Council and of the members of committees and subcommittees; the other pamphlet to contain a description of the organization and scope of the Council with explanatory statements relative to the purposes and activities of its committees.

Upon recommendation of Dr. Hale, Chairman of the Council, conveyed by telegraph, an appropriation of $1000.00 was authorized for the submarine work being carried on at the San Pedro Station of the Navy Department under the direction of Dr. Hale.

The meeting adjourned at 12.45.

The thirty-second meeting convened in the offices of the Council in the Munsey Building, Washington, D. C., on Friday, October 19, 1917, and was called to order at 9.05 a.m. by the Chairman of the Committee, Mr. Carty.

Messrs. Bogert, Carty, Chittenden, Conklin, Dunn, Manning, Millikan, Noyes, Stratton and Walcott, and, by invitation, C. A. Adams, Durand, Kellerman, Mendenhall and Yerkes were present.

The minutes of the regular monthly meeting of the Committee of September 19 were approved as prepared and distributed to the members of the Committee.

The Executive Officer reported:

1. That the Chairman of the Council has appointed Mr. Manning a member of the Executive Committee and has asked Mr. Welch to serve as an ex-officio member. Upon instruction, the name of Dr. Gatewood, former director of the Army Medical School, has been removed from the list of members of the Council and of its Military Committee, on account of assignment for duty away from Washington.

2. That the U. S. Navy Department has requested the National Research Council, the Naval Consulting Board and the American Institute of Electrical Engineers to submit nominations from which one hundred electrical engineers may be selected and commissioned as lieutenants, junior grade, in the U. S. Naval Reserve Force; and that Mr. Pupin and Mr. Hutchinson have been appointed by the Executive Officer to represent the Council on a joint committee of these three organizations for the purpose of performing the functions required by the Navy Department.

A financial statement was presented showing expenditures and balances available in the bank accounts of the Council.

Botanical Raw Products Committee.—A letter from the Chairman of this Committee was read and, upon motion, he was authorized to invite a member

of the firm of Arthur D. Little, Incorporated, of Boston to become a member of the Botanical Raw Products Committee, and to extend a similar invitation to Dr. W. W. Stockberger, in charge of the division of Drug Plant Investigations of the U. S. Department of Agriculture.

Chemistry Committee.—Upon recommendation of the Chairman of this Committee, the name of the Sub-committee on Dyestuffs was changed to read Sub-committee on Dyestuffs and Textiles, and the name of the Sub-committee on Nitrogen Fixation was changed to read Sub-committee on Nitrates and Ammonia. Furthermore the following were added to the membership of the Chemistry Committee:

George A. Burrell, Assistant to the Director, U. S. Bureau of Mines.
William F. Hillebrand, Chief Chemist, U. S. Bureau of Standards.
Charles L. Parsons, Chief Chemist, U. S. Bureau of Mines.
James F. Norris, Professor of General Chemistry, Massachusetts Institute of Technology.
Joel H. Hildebrand, Associate Professor of Chemistry, University of California.
John Johnston, Research Department, American Zinc, Lead and Smelting Co.

and membership in the Sub-committee on Nitrates and Ammonia was appointed as follows:

Arthur A. Noyes, *Chairman.*
Frederick G. Keyes, Associate Professor of Physico-Chemical Research, Massachusetts Institute of Technology.
Alfred H. White, Professor of Chemical Engineering, University of Michigan.

The resignation of Mr. W. V. Vawter as a member of the Sub-committee on Chemistry of Fuels was accepted.

Geology and Paleontology Committee.—Upon request of the Chairman, the following were added as members of this Committee:

Edward B. Mathews, State Geologist of Maryland.
Frank W. DeWolf, State Geologist of Illinois.

and a Sub-committee on the Geology of Cantonments and Topographical Instruction in Training Camps was authorized with the following membership:

Frank W. DeWolf, *Chairman*, President, Association of American State Geologists.
Wallace W. Atwood, Professor of Physiography, Harvard University.
Isaiah Bowman, Secretary, American Geographical Society.
Marius R. Campbell, Geologist, U. S. Geological Survey.
William O. Hotchkiss, State Geologist of Wisconsin.
Douglas W. Johnson, Professor of Geography, Columbia University.
Lawrence Martin, Professor of Geography and Physiography, University of Wisconsin.
George Otis Smith, Director, U. S. Geological Survey.

An allotment ot $100 was also authorized for miscellaneous expenses incident to the work of the Geology and Paleontology Committee and its Sub-

committees, payments to be made in accordance with instructions of the Chairman of the said Committee.

Physiology Committee.—Upon request of the vice-chairman of this Committee, Sub-committees were authorized and members thereof were appointed as follows:

Sub-Committee for Investigations upon the Physiology of Shock

W. B. Cannon, *Chairman*, Professor of Physiology, Harvard University.

Joseph Erlanger, Professor of Physiology, Washington University.

C. C. Guthrie, Professor of Physiology and Pharmacology, University of Pittsburgh.

Yandell Henderson, Professor of Physiology, Yale University.

D. R. Hooker, Associate Professor of Physiology, Johns Hopkins University.

H. C. Jackson, Professor of Physiology, New York University and Bellevue Hospital Medical School.

D. R. Joseph, Professor of Physiology, St. Louis University Medical School.

F. H. Pike, Associate Professor of Physiology, Columbia University.

T. Sollmann, Professor of Pharmacology, Western Reserve University.

C. J. Wiggers, Assistant Professor of Physiology, Cornell University Medical School.

Sub-Committee on Control of Hemorrhage

W. H. Howell, *Chairman*, Professor of Physiology, Johns Hopkins University.

Jay McLean, Student, Johns Hopkins Medical School.

Sub-Committee on Solution Adapted for Transfusion After Hemorrhage

L. J. Henderson, *Chairman*, Assistant Professor of Biological Chemistry, Harvard University.

Cecil H. Drinker, Instructor in Physiology, Harvard University Medical School.

Sub-Commitee on Fatigue in Industrial Pursuits

Frederick S. Lee, *Chairman*, Professor of Physiology, Columbia University.

Psychology Committee.—Upon recommendation of the Chairman, the following members were added to the Psychology Committee:

James R. Angell, Professor of Psychology, University of Chicago.

Walter D. Scott, Director of Bureau of Salesmanship Research, Carnegie Institute of Technology.

Upon motion, the Executive Committee voted to accept the resignation tendered by J. McKeen Cattell of Columbia University as a member of the Psychology Committee.

Forestry Committee.—The special committee appointed at the meeting of the Executive Committee of September 19, 1917, to submit recommendations relative to an adequate organization to represent the interests of forestry and its relation to work in botany and agriculture submitted a report, after the discussion of which it was voted, upon motion, to approve the organization of a special Forestry Committee of the Council with the understanding that recommendations concerning membership in such a committee would be submitted at a subsequent meeting.

Physics Committee.—Mr. Millikan reported with reference to development of work in meteorology and sound-ranging and mentioned the highly organized work of the Council relative to submarines; and Mr. Mendenhall submitted a report of progress concerning work in aeronautic instruments, with particular reference to the subject of photographic lenses.

A communication from Dr. Hale was read relative to the question of international scientific relations after the war. Extended discussion of this subject followed.

Mr. Millikan referred to a communication from the Chairman of the Council respecting the question of financial assistance from the Government, in view of the co-operative investigations which have been undertaken by the Council in the interests of the Government. Discussion which followed led to the consideration of the question of possible development of the Council which may become necessary as a result of the extension of these activities; and in this connection Mr. Bogert submitted an oral report relative to the work of the special committee which had been appointed by the Executive Committee to consider questions of organization. This special committee was thereupon discharged.

Upon motion, a Sub-committee to consist of five members, of which the Chairman of the Council shall be Chairman and the Chairman of the Executive Committee a member, was authorized to consider and report at a future meeting of the Executive Committee, with reference to the advisability of securing further financial assistance from the Government in the work of the Council, with regard to recommendations concerning advisable changes in the organization of the Council with a view to increasing its usefulness for future work, and with reference to the relations which members of the Council and of its committees may appropriately maintain with the U. S. Government should such individuals be directly associated with industrial and commercial interests from which the Government is receiving assistance.

A letter from Cary T. Hutchinson, submitting his resignation as Secretary of the Council, was read. He stated, however, that he expected to continue as Secretary of the Engineering Foundation for the present and would be glad at the same time to serve the Council, if of convenience.

After discussion and upon motion, the offer of Mr. Hutchinson to continue his work as Secretary of the Council was accepted with thanks.

The following resolution passed by the Engineering Foundation at its meeting on September 20, 1917, was submitted, in reply to the offer of the National Research Council, tendered by resolution at a meeting of its Executive Committee on August 22, 1917.

Resolved, That the Engineering Foundation receives with pleasure the resolution of the National Research Council expressing appreciation of the financial and personal assistance rendered to the organization and work of the Council during the past year. Reciprocating the Council's desire for the maintenance of close relations between workers in science and in

engineering, the Foundation hereby declares that it will be its policy to continue the co-operation between the two bodies in all practicable ways that may be now, or may become, mutually available, such ways consisting of the interchange of helpful suggestions, advice, information, office representation and similar facilities, and in addition a recognition of community of purpose that shall promote in the field of engineering research increasingly intimate relations between engineering and science.

The reciprocal designation of the secretary of each of the bodies as an assistant secretary of the other for the purpose of enabling both bodies to have offices in both New York and Washington is favorably regarded, and the Foundation welcomes and gladly accepts the offered assistance of the Research Council in the national co-relations of the Engineering Research work of the Foundation to which the Foundation's resources will be devoted.

Extended discussion took place with regard to advisable means for bringing about a more co-ordinate effort among engineering interests with reference to the Government. Upon invitation, Mr. C. A. Adams, Chairman of the General Engineering Committee of the Advisory Commission of the Council of National Defense, outlined plans already made for a general conference on this subject to be held in Washington on October 26, to which all of the national societies and special organizations interested in engineering work have been invited to send representatives. He stated that the National Research Council, its Engineering Committee and its Aeronautics Committee have each been invited to appoint two representatives to attend this conference. There followed further discussion in which Mr. Adams participated, concerning the engineering work of the Research Council, after which the Chairman of the Executive Committee was requested upon motion to appoint the six representatives of the Council to attend the conference on October 26, with the understanding that these conferees will meet in advance to discuss and determine upon appropriate recommendations which may advisedly be made by them at the conference in question. The following members of the Council were appointed as conferees; Messrs. Carty, Dunn, Durand, Noyes, Millikan and Pupin.

The meeting adjourned at 1.10 p.m.

The thirty-third meeting of the Executive Committee convened in the offices of the Council in the Munsey Building, Washington, D. C., on Saturday, November 10, 1917, and was called to order at 10.10 a.m. by the Chairman of the Committee, Mr. Carty.

Present: Messrs. Bogert, Carty, Chittenden, Conklin, Dunn, Hale, Manning, Millikan, Noyes, Pearl and Pupin. Mr. Mendenhall was also present upon invitation.

The minutes of the regular monthly meeting of the Committee of October 10 were approved as prepared and distributed to members of the Committee.

Record is made of the following actions which have taken place since the last meeting of the Committee:

1. That upon suggestion of the Chairman of the Council, Major General William M Black has been appointed a member of the Council by the President of the National Academy of Sciences and has been designated by the President of the United States for such service. General Black has accepted such appointment and has furthermore been added to the membership of the Military Committee.

2. That leases of Rooms 304, 312, 313 and 314 in the Munsey Building have been taken for a period of ten months, beginning November 1, 1917, at the monthly rental of ninety-five dollars.

3. The following have been appointed and have accepted appointment as members of the Botanical Raw Products Committee; Prof. H. M. Hall, Professor of Botany, University of California; Mr. Arthur D. Little, President of Arthur D. Little, Incorporated, Boston; and Dr. W. W. Stockberger, physiologist in charge of drug plant and poisonous plant investigations, U. S. Department of Agriculture.

4. Upon request of the Chairman of the Chemistry Committee, record is made of the following additions to membership in the Sub-committee on Biochemistry:

William J. Gies, Professor of Biochemistry, Columbia University; Lawrence J. Henderson, Assistant Professor of Biochemistry, Harvard University.

Mr. Hale, upon invitation of the Chairman, stated that the primary purpose in calling the present meeting was to afford opportunity for necessary discussion relative to the development and organization of the Council.

Upon request, Mr. Hale outlined proposed plans with regard to the development of the Council, and spoke at length concerning the possibility of obtaining financial assistance in its work. Extended discussion took place, emphazising the important possibilities of the Council's various fields of work, as indicated in a written statement presented by Mr. Hale. Upon motion of Mr. Noyes, it was voted that these preliminary plans, as submitted and outlined by the Chairman of the Council, be substantially approved.

Upon motion it was also voted that the duties of the Sub-committee of the Executive Committee, authorized at the last meeting of the Committee to consider and report with reference to questions of organization and Governmental relations, be increased by requesting this Sub-committee likewise to undertake careful consideration of plans which have been approved for the future development of the Council. This Sub-committee was thereupon appointed to consist of Mr. Hale, Chairman, and Messrs. Carty, Dunn, Millikan, Noyes, Pearl, Vaughan and Walcott.

The meeting adjourned at 12.45 p.m.

CARY T. HUTCHINSON, *Secretary.*

LIST OF PUBLICATIONS OF THE NATIONAL ACADEMY OF SCIENCES

From the year 1864 to date the National Academy of Sciences has published at various times Scientific Memoirs, Biographical Memoirs, Annual Reports, Proceedings, and Reports of Committees. The detailed list of titles, with the number of pages and the year of publication, is given below.

MEMOIRS OF THE NATIONAL ACADEMY OF SCIENCES

VOLUME I. 1866

1. Reduction of the observations of fixed stars made by Joseph LePaute D'Agelet, at Paris, in 1783–1785, with a catalogue of the corresponding mean places, referred to the Equinox of 1800.0. BENJAMIN APTHORP GOULD. Read January 8, 1864. Pp. 1–261.
2. The Saturnian system. BENJAMIN PEIRCE. Read January 8, 1864. Pp. 263–286.
3. On the distribution of certain diseases in reference to hygienic choice of location for the cure of invalid soldiers. AUGUSTUS A. GOULD. Read August 5, 1864. Pp. 287–290.
4. On shooting stars. H. A. NEWTON. Read August 6, 1864. Pp. 291–312.
5. Rifled guns. W. H. C. BARTLETT. Read August 25, 1865. Pp. 313–343.

VOLUME II. 1884

1. Report of the eclipse expedition to Caroline Island, May, 1883. Pp. 5–146.
2. Experimental determination of wave-lengths in the invisible prismatic spectrum. S. P. LANGLEY. April, 1883. 4 plates. Pp. 147–162.
3. On the subsidence of particles in liquids. WILLIAM H. BREWER. Read November 15, 1883. Pp. 163–175.
4. Upon the formation of a deaf variety of the human race. ALEXANDER GRAHAM BELL Read November 13, 1883. Pp. 177–262.

VOLUME III. (PART 1.) 1885

1. The sufficiency of terrestrial rotation for the deflection of streams. G. K. GILBERT. Read April 15, 1884. Pp. 5–10.
2. On the temperature of the surface of the Moon. From researches made at the Allegheny Observatory by S. P. LANGLEY, assisted by F. W. VERY and J. E. KEELER. Read October 17, 1884. Plates 1–6. Pp. 11–42.
3. On a method of precisely measuring the vibratory periods of tuning-forks, and the determination of the laws of the vibrations of forks; with special reference to these facts and laws to the action of a simple chronoscope. ALFRED M. MAYER. Plates 1–4. Pp. 45–59.
4. The Baumé hydrometers. C. F. CHANDLER. Read at Philadelphia meeting, 1881. Pp. 61–71.
5. On small differences of sensation. C. S. PEIRCE and J. JASTROW. Read October 17, 1884. Pp. 73–83.
6. Description of an articulate of doubtful relationship from the Tertiary beds of Florissant, Colorado. SAMUEL H. SCUDDER. Read April 20, 1882. Pp. 85–90.
7. The structure of the columella auris in the Pelycosauria. E. D. COPE. Read October 16, 1884. Pp. 93–95.
8. On the structure of the brain of the sessile-eyed Crustacea. I. The Brain of Asellus and the eyeless form Cecidotæa. A. S. PACKARD. Plates 1–5. Pp. 97–110.

VOLUME III. (PART 2.) 1886

9. Contributions to meteorology. ELIAS LOOMIS. Plates 1–16. Pp. 5–66.
10. On Flamsteed's stars "observed, but not existing." C. H. F. PETERS. Read November 11, 1885. Pp. 67–83.

VOLUME IV. (PART 1.) 1888

VOLUME IV. (PART 2.) 1889

VOLUME V. 1891

4. The embryology and metamorphosis of the Macroura. W. K. BROOKS and F. H. HERRICK. Plates 1–57. Pp. 319–576.
5. On the application of interference methods to astronomical measurements. A. A. MICHELSON. 7 Plates. Pp. 577–590.

VOLUME VI. 1893

1. On the capture of comets by planets, especially their capture by Jupiter. H. A. NEWTON. Pp. 5–23.
2. A study on the relation of atmospheric electricity, magnetic storms, and weather elements to a case of traumatic neuralgia. ROBERT CATLIN. 7 Plates. Pp. 25–33.
3. On certain new methods and results in optics. CHARLES S. HASTINGS. Pp. 35–47.
4. The proteids or albuminoids of the oat kernel. THOMAS B. OSBORNE. Presented November 13, 1890, and November 10, 1891. Pp. 49–87.
5. A comparison of antipodal faunas. THEODORE GILL. Read November, 1887. Pp. 89–124.
6. Families and sub-families of fishes. THEODORE GILL. Pp. 125–138.
7. Human bones of the Hemenway Collection in the United States Army Medical Museum DR. WASHINGTON MATTHEWS. With observations on the Hyoid bones of this collection. Dr. J. L. WORTMAN. Reports presented to the National Academy of Sciences, with approval of the Surgeon-General of the United States Army. DR. J. S. BILLINGS. Plates 1–59. Pp. 139–286.
8. Further studies on the brain of Limulus Polyphemus, with notes on its embryology. ALPHEUS S. PACKARD. Plates 1–36. Pp. 287–331.

VOLUME VII. 1895

1. Monograph of the Bombycine moths of America, north of Mexico, including their transformations and origin of the larval markings and armature. Part I. Family I. Notodontiadæ. ALPHEUS S. PACKARD. Plates 1–49, Maps 1–10. 1895. Pp. 3–390.
2. On reaction-times and the velocity of the nervous impulse. J. MCKEEN CATTELL and CHARLES S. DOLLEY. Presented at Albany meeting, 1893. Pp. 391–415.
3. The bacteria of river waters. JOHN S. BILLINGS. Including a paper on the bacteria of the Schuykill River, by J. H. WRIGHT, and Appendix by Dr. OLMSTEAD. Plates 1–5, Diagrams 1–5. Pp. 417–484.

VOLUME VIII. 1902

1. Notes on the bacteriological examination of the soil of Philadelphia. MAZŸCK P. RAVENEL. 4 Plates. 1896. Pp. 1–41.
2. A contribution to the study of the effect of the venom of Crotalus Adamanteus upon the blood of man and animals. S. WEIR MITCHELL and ALONZO H. STEWART. Plates 1–6. 1898. Pp. 43–56.
3. General perturbations of Minerva (93), by Jupiter, including terms only of the first order with respect to the mass, together with a correction of elements. W. S. EICHELBERGER. 1899. Pp. 57–77.
4. Ophiura Brevispina. W. K. BROOKS and CASWELL GRAVE. Plates 1–3. 1899. Pp. 79–100.
5. Anatomy of Nautilus Pompilius. LAWRENCE EDMONDS GRIFFIN. Plates 1–17. 1900. Pp. 101–230.
6. An experimental inquiry regarding the nutritive value of alcohol. W. O. ATWATER and F. G. BENEDICT. 1902. Pp. 231–397.
7. West Indian Madreporarian Polyps. J. E. DUERDEN. Plates 1–25. 1902. Pp. 399–648.

VOLUME IX. 1905

Monograph of the Bombycine moths of North America, including their transformations and origin of the larval markings and armature. Part II. Family Ceratocampidæ, subfamily Ceratocampinæ. ALPHEUS SPRING PACKARD. Plates 1-61. 1905. Pp. 1-272.

VOLUME X. 1911

1. The absolute value of the acceleration of gravity determined by the ring-pendulum method. CHARLES E. MENDENHALL. Plates 1-3. 1905. Pp. 1-23.
2. Claytonia gronov. A morphological and anatomical study. THEODORE HOLM. Plates 1-2. 1905. Pp. 25-37.
3. A research upon the action of alcohol upon the circulation. HORATIO C. WOOD and DANIEL M. HOYT. Plates 1-3. 1905. Pp. 39-70.
4. Phoronis architecta: Its life history, anatomy, and breeding habits. WILLIAM KEITH BROOKS and REINART PARKER COWELS. Plates 1-17. 1905. Pp. 71-148.
5. The affinties of the pelagic tunicates. No. 1. On a new Pyrosoma (Dipleurosoma elliptica). WILLIAM KEITH BROOKS. Plates 1-2. 1906. Pp. 149-156.
6. Commelinaceœ. Morphological and anatomical studies of the vegetative organs of some North and Central American species. THEODORE HOLM. Plates 1-8. 1906. Pp. 157-192.
7. Tables of minor planets discovered by James C. Watson. Part I. Tables of (93) Minerva, (101) Helena, (103) Hera, (105) Artemis, (115) Thyra, (119) Althaea, (128) Nemesis, (133) Cyrene, (139) Juewa, (161) Athor, (174) Phaedra, (179) Klytaemnestra. With the assistance of R. T. CRAWFORD, FRANK ROSS, BURT L. NEWKIRK, ADELAIDE M. HOBE, ESTELLE GLANCY, and others. Being in part a continuation of previous investigations by E. BECKER, W. S. EICHELBERGER, WILLIAM MCKNIGHT RITTER, and G. K. LAWTON. ARMIN O. LEUSCHNER. 1910. Pp. 193-374

VOLUME XI. 1913

Agave in the West Indies. WILLIAM TRELEASE. Plates A-E, 1-116. 1913. Pp. 3-298

VOLUME XII. (PART 1.) 1914

1. Monograph of the Bombycine moths of North America, including their transformations and origin of the larval markings and armature. Part III. Families Ceratocampidae (exclu ive of Ceratocampinae), Saturniidae, Hemileucidae, and Brahmaeidae. ALPHEUS SPRING PACKARD, edited by THEODORE D. A. COCKERELL, being a continuation of previous investigations published as Volume VII (First Memoir) and Volume IX (Second Memoir) of the Memoirs of the National Academy of Sciences. Plates 1-113. 1914. Pp. 1-516.

VOLUME XII. (PART 2.) 1915

2. The variations and ecological distribution of the snails of the genus Io. CHARLES C. ADAMS. Plates 1-61. Pp. 1-184.
3. The turquoise. A study of its history, mine alogy, geology, ethnology, archaeology, mythology, folklore, and technology. JOSEPH E. POGUE, Ph.D., Plates 1-22. 1915. Pp. 1-206.

VOLUME XIII. 1915

1. Catalogue of the meteorites of North America, to January 1, 1909. OLIVER CUMMINGS FARRINGTON. Plates 1-36. 1915. Pp. 1-513.

VOLUME XIV

1. Report on researches on the chemical and mineralogical composition of meteorites, with especial reference to their minor constituents. GEORGE PERKINS MERRILL, 1916. Pp. 1–29.

BIOGRAPHICAL MEMOIRS

VOLUME I. 1877

Joseph Stillman Hubbard, 1823–1863. B. A. GOULD. Read August 5, 1864. Pp. 1–34.
Joseph Gilbert Totten, 1788–1864. J. G. BARNARD. Read January 6, 1866. Pp. 35–97.
Benjamin Silliman, Sr., 1779–1864. ALEXIS CASWELL. Read January 25, 1866. Pp. 99–112.
Edward Hitchcock, 1793–1864. J. P. LESLEY. Read August 9, 1866. Pp. 113–134.
James Melville Gillis, 1811–1865. BENJAMIN APTHORP GOULD. Read January 26. 1866. Pp. 135–179.
Alexander Dallas Bache, 1806–1867. JOSEPH HENRY. Read April 16, 1869. Pp. 181–212d.
John H. Alexander, 1812–1867. J. E. HILGARD. Read April 18, 1872. Pp. 213–226.
William Chauvenet, 1820–1870. J. H. C. COFFIN. Read April 16, 1873. Pp. 227–244.
John Fries Frazer, 1812–1872. JOHN L. LECONTE. Read October 22, 1873. Pp. 245–256.
James Henry Coffin, 1806–1873. A. GUYOT. Read April 24, 1874. Pp. 257–264.
John Torrey, 1796–1873. ASA GRAY. Read April 15, 1873. Pp. 265–276.
William Starling Sullivant, 1803–1873. ASA GRAY. Read April 22, 1875. Pp. 277–285.
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REPORTS OF COMMITTEES

(Report on the question of the value of the water-proofing process employed in the manufacture of the fractional currency.) In House Misc. Doc. no. 163, part 2, 44th Congress, 1st Session, pp. 22–28. April 3, 1876.

Forty-fifth Congress, 3rd Session, House of Representatives Misc. Doc. no. 5. Surveys of the Territories. Letter from the Acting President of the National Academy of Sciences, transmitting a report on the surveys of the Territories. Ordered printed, December 3, 1878. 8°. Pp. 1–27.

Forty-seventh Congress, 2d Session. Senate Misc. Doc. no. 51. National Academy of Sciences. Investigation of the scientific and economic relations of the sorghum sugar industry, being a report made in response to a request from the Hon. George B. Loring, U. S. Commissioner of Agriculture, by a committee of the National Academy of Sciences. November, 1882. Washington: Government Printing Office. 1883. 8°. Pp. 1–152.

United States Internal Revenue. Report on glucose, prepared by the National Academy of Sciences, in response to a request made by the Commissioner of Internal Revenue. Washington: Government Printing Office. 1884. 8°. Pp. 1–108.

Report of committee of National Academy of Sciences concerning classification of Donskoi wool, January 30, 1886. 1886. Treasury Department Doc. no. 805.

Forty-ninth Congress, 1st Session. Senate, Ex. Doc. no. 67. Letter from the Secretary of the Navy, transmitting, in compliance with the Senate resolution, February 2, 1886, report of the National Academy of Sciences upon the proposed new Naval Observatory. Ordered printed, February 10, 1886.

(Report on the organization of the National Surveys and the Signal Service.) In Senate Misc. Doc. no. 82, 49th Congress, 1st Session. Pp. 1–37. Ordered printed, March 16, 1886. 1886.

National Academy of Sciences. Standards for Electrical Measure, February 20, 1895. Printed for the Academy. Washington: Judd & Detweiler, Printers. 1895. 8°. Pp. 1–9.

Fifty-third Congress, 3rd Session. Senate, Misc. Doc. no. 115. Report of the National Academy of Sciences, made in compliance with a requirement of the law (H. R. 6500) entitled "an act to define and establish the units of electrical measure," approved July 12, 1894. Ordered printed, February 19, 1895.

Report of the committee appointed by the National Academy of Sciences upon the inauguration of a forest policy for the forested lands of the United States to the Secretary of the Interior, May 1, 1897. Washington: Government Printing Office. 1897. Pp. 1–47.

Fifty-eighth Congress, 3d Session. Senate Doc. no. 145. Report by committee appointed by Academy to consider desirability of instituting scientific explorations of Philippine Islands. Pp. 1–22. 8°. Ordered printed, February 7, 1905.

Sixtieth Congress, 2d Session. House of Representatives, Doc. no. 1337. Conduct of scientific work under United States Government. Message from the President of the United States, transmitting report of the National Academy of Sciedces relating to the conduct of the scientific work under the United States Government. Pp. 1–5. 8°. Ordered printed, January 18, 1909.

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REPORT OF THE AUTUMN MEETING

Prepared by the Home Secretary

The Autumn Meeting of the Academy was held in the Engineering Building of the University of Pennsylvania at Philadelphia, on November 20 and 21, 1917.

Fifty-five members were present as follows: Messrs. C. G. Abbott, Bailey, Bogert, Carty, F. W. Clarke, J. M. Clarke, Conklin, Coulter, Cross, Davenport, Day, Donaldson, Farlow, Flexner, Hale, E. H. Hall, Halsted Harper, Harrison, Holmes, Howard, Howe, Howell, Iddings, Jennings, Kasner, Leuschner, Levene, Loeb, Lusk, Mayer, Meltzer, Mendel, Millikan, Morgan, E. S. Morse, E. L. Nichols, A. A. Noyes, H. F. Osborn, Pearl, Pupin, Reid, Schlesinger, Scott, Alexander Smith, Edgar F. Smith, Erwin F. Smith, Stratton, Thomson, Thorndike, Walcott, Welch, Wheeler, David White, H. S. White, and Wilson.

BUSINESS SESSIONS

The President announced the following deaths since the last Annual Meeting of the Academy: Arnold Hague, elected 1885, died May 15, 1917; J. M. Crafts, elected 1872, died June 21, 1917; William B. Clark, elected 1908, died July 27 ,1917, and Franklin P. Mall, elected 1907, died November 17, 1917; also Adolf von Baeyer, Foreign Associate, elected 1898, died August, 1917.

The President also announced the assignment of the following biographical memoirs: Arnold Hague to J. P. Iddings; William Bullock Clark to John M. Clarke; and Franklin P. Mall to R. G. Harrison.

Under the rules of the Academy the following members of the Editorial Board of the PROCEEDINGS retire December 1, 1917; E. G. Conklin, C. B. Davenport, E. B. Frost, W. H. Holmes, and E. H. Moore. The Home Secretary announced that the following members had been appointed by the Council to serve in their places until December 1, 1920: Jacques Loeb, W. M. Wheeler, E. B. Frost, E. L. Thorndike, and E. H. Moore. E. B. Wilson was reappointed Managing Editor for one year.

The President appointed an Auditing Committee consisting of C. G. Abbot, chairman, W. F. Durand, and A. L. Day.

Considering the request of Mr. Julius Stieglitz, member for the United States of the International Commission on Annual Tables of Constants and Numerical Data, asking on behalf of the Commission for a continuation of the support of the Academy in the publication of the tables under the patronage of the International Association of Academies, the following grant was approved:

That a grant of $200, or such portion of it as may be approved by the President and Foreign Secretary, be made from the general funds of the Academy as a subvention in support of the annual tables of constants published under the patronage of the International Association of Academies.

The following minute from the Council relating to the development of a Section of Engineering was approved:

It was the sense of the Council that the Home Secretary be requested to obtain suggestions from members of the Academy of names of engineers to be considered by the Council for nomination at the next annual meeting.

Considering a communication from the American Association of University Professors, requesting coöperation in the classification of scientific men for war service, the following recommendations were adopted by the Academy and forwarded to the Secretary of War:

The National Academy of Sciences, being convinced that such action is absolutely necessary for the successful prosecution of the war, urges that the privilege of enlistment granted to the medical profession, including students and internes, under orders of the War Department, Office of the Surgeon-General, dated September 4, 1917, if not already provided for by the intended interpretation of the new classification of drafted men, be expanded to embrace men in the following scientific professions, including junior, senior and graduate students in educational or research institutions, so that all such men may be privileged to enroll in the

appropriate Reserve Corps or in such branch of the service as in your judgment will enable them to make their special knowledge and training of greatest use to their country:

Agriculturists	Civil Engineers	Pathologists
Anatomists	Electrical Engineers	Physicists
Astronomers	Geologists	Physiologists
Bacteriologists	Mechanical Engineers	Psychologists
Biologists	Metallurgical Engineers	Zoologists
Botanists	Meteorologists	Experts in Public Health, Hygiene and Sanitation.
Chemists	Mining Engineers	
Chemical Engineers		

The Academy recommends that, pending the working out of the details requisite for the establishment of these privileges, the professional men affected by the same be placed in Class III of the new classification of drafted men.

The purpose of the establishment of the Academy by special act of Congress, as stated in its charter, was to create an organization whose duty it should be to advise the Government on scientific matters. It would be recreant to this duty, therefore, if it failed to point out the urgent need of the action recommended above, and to express its firm conviction that to win this war our scientifically trained men must be used to do the work which they alone can do. The inclusion of such men in Class I of the new draft classification would result in a wholly disproportionate loss of national efficiency in comparison with the size of the army so created. Of the many grievous losses sustained by our Allies, the one felt most keenly, according to their own oft-repeated statements, is that of the scientific men who went to the front at the first call, and laid down their lives there.

These recommendations are prompted by the following further considerations:

1. The failure of many of the District Boards created by the selective draft legislation, to recognize the necessity of retaining these scientific men for the kind of work for which they have been especially trained.

2. The retention of many scientific men as privates in the training camps of the national army, who it is believed could render much more valuable service to their country if employed in the lines of their special profession.

3. The eager patriotism of our university men has led large numbers to enlist in the rank and file of our Army and Navy, and has correspondingly thinned the ranks of students, teachers and investigators. There is immediate danger that without specific provision to the contrary, the Universities will lack the teachers and students necessary to insure a steady flow of new effectives to the industrial and military fronts.

4. Many industrial establishments now of the utmost importance for the security and defense of our country, are seriously impeded in their work by the fact that numbers of their highly trained scientists already have either been drafted, or have volunteered for service and sought commissions for fear that they would be drafted and assigned to military duties of such a character as would not enable them to render to their country the greatest service of which they were capable. The result of this has been to reduce the working force of scientists in such establishments to the point where, unless further withdrawals are made with the greatest care, many of these plants will have to close their doors and go out of business.

It is respectfully urged that professional students be instructed to enlist in the appropriate Reserve Corps of the service, with the privilege of furlough for the completion of their training, similar to that adopted to secure the most efficient service of the medical men, and that this opportunity be granted only to men of proven ability. Students, teachers, and research men should be given the opportunity of enlistment with the privilege of furlough or discharge subject to the recommendations of the presidents of the institutions concerned. For others, the decision should rest with the authorities designated by the Secretary of War.

In case the Academy can be of assistance in working out any of the details involved in the foregoing recommendations, it will esteem it both an honor and a patriotic duty to cooperate with whomsoever you may designate.

The following amendment to the constitution reported from the Council and returned to the Academy from the Committee of the Whole with favorable recommendation was adopted:

That the Academy shall hold one stated meeting, called the Annual Meeting, in April of each year in the city of Washington, and another stated meeting, called the Autumn Meeting, at a place to be determined by the Council. The Council shall also have power to fix the date of each meeting.

The following rule recommended by the Council from the Annual Meeting (April, 1917) was adopted:

The annual meeting of the Academy shall begin on the fourth Monday of April.

The Committee of the Whole, upon recommendation of the Council, considered the following amendment to the constitution and reported it for favorable action by the Academy at the annual meeting in 1918:

That the constitution be amended by substituting in Article II, Section 1, line 3 (Report, 1916) the word *four* for the word *six* so that it will read . . . for a term of four years . . . to take effect on the expiration of the term of office of the present incumbents or in case of a vacancy.

Article II, Section 1, when amended to read: Section 1. The officers of the Academy shall be a president, a vice president, a foreign secretary, a home secretary, and a treasurer, all of whom shall be elected for a term of *four* years (line 3 in Report, 1916) by a majority of votes present, at the first stated meeting after the expiration of the current terms, provided that existing officers retain their places until their successors are elected. In case of a vacancy, the election for *four* years shall be held in the same manner at the meeting when such vacancy occurs, or at the next stated meeting thereafter, as the Academy may direct. A vacancy in the office of treasurer or home secretary may, however, be filled by appointment of the president of the Academy until the next stated meeting of the Academy.

The following motion relating to the administration of the Watson Fund adopted at the Business Session of April 16, 1917, which was found to conflict with a clause in the will of James Craig Watson, was rescinded:

That the trustees of the Watson Fund be authorized to act for the Academy in the approval of grants and to report such action to the Academy at the next stated meeting.

Under business from the Council a statement of the financial condition of the PROCEEDINGS was laid before the Academy in considerable detail. To meet the anticipated deficit of $5000 in the conduct of the PROCEEDINGS to July 1, 1918, Mr. Pupin announced that three members of the Academy had agreed to be responsible for one half of this sum. The Chairman of the Finance Committee, Mr. Davenport, reported that his commitee had raised $1100 for the same purpose. After discussion by the President, the Chairman of the Editorial Board, and Messrs. Hale, Meltzer, John M. Clarke and the Home Secretary, it appeared to be the sense of the meeting that further financial arrangements be left in the hands of the Finance Committee.

The Academy further considered securing additional subscriptions for the PROCEEDINGS and upon the recommendation of the Council adopted the following resolution:

That the Editorial Board recommend to the Academy that each member of the Academy be responsible for one subscription to the PROCEEDINGS, to be paid for by the member or by a subscriber.

The following resolution was unanimously adopted:

That the Home Secretary be requested to transmit the thanks of the Academy to the Provost of the University of Pennsylvania, the President of the American Philosophical Society, the President of the Academy of Natural Sciences of Philadelphia and the members of the local committee for the courtesies extended to the members of the National Academy of Sciences at the Autumn Meeting, 1917.

SCIENTIFIC SESSIONS

Two public scientific sessions were held on November 20 and 21 at which the following papers were presented (an asterisk denotes presentation only by title):

ERWIN F. SMITH, Bureau of Plant Industry, U. S. Department of Agriculture: The wheat problem of the United States.

LIBERTY H. BAILEY, Cornell University: The modern systematist.

BRADLEY M. DAVIS, University of Pennsylvania: A criticism of the evidence for the mutation theory of De Vries from the behavior of œnothera in crosses and in selfed lines. (By invitation.)

JACQUES LOEB, Rockefeller Institute: The chemical mechanism of regeneration.

HENRY H. DONALDSON, The Wistar Institute: A comparison of growth changes in the nervous system of the rat with the corresponding changes in man.

CHARLES B. DAVENPORT, Station for Experimental Evolution, Carnegie Institution: Hereditary tendency to form nerve tumors.

LAFAYETTE B. MENDEL and THOMAS B. OSBORNE, Yale University: Food hormones or vitamines in some animal tissues.

EDGAR F. SMITH and WALTER K. VANHAAGEN, University of Pennsylvania: The atomic weight of boron.

SAMUEL J. MELTZER and JOHN AUER, Rockefeller Institute: The effect of intravenous injection of magnesium sulphate upon tetanus (with a lantern slide demonstration by J. Auer).

SIMON FLEXNER, Rockefeller Institute: Chemotherapy of spirochetal infections. (For Doctors Jacobs and Brown.)

CLARENCE E. MCCLUNG, University of Pennsylvania: Possible action of the sex-determining mechanism. (By invitation.)

THOMAS H. MORGAN, Columbia University: The cause of Mosaics and Gynandromorphs in Drosophila.

HERBERT E. IVES, Physical Laboratory, The United Gas Improvement Company: Spectrum analysis by differential persistence of vision. (By invitation.)

CHARLES G. ABBOT, Smithsonian Astrophysical Observatory: The atmosphere and terrestrial radiation.

EDWARD KASNER, Columbia University: Geometric aspects of the theory of heat.

OLIVER E. GLENN, University of Pennsylvania: Invariants which are functions of parameters of the transformation. (By invitation.)

EDWIN H. HALL, Harvard University: The validity of the thermoelectric equation $P = T\,dv/dT$.

EDWIN H. HALL, Harvard University: A thermoelectric diagram on the P-V-plane.

WILLIAM B. SCOTT, Princeton University: The Astrapotheria of the Patagonian Miocene.

HENRY F. OSBORN, American Museum of Natural History: Evolution of the Titanotheres; final conclusions.

ERIC DOOLITTLE, University of Pennsylvania: Study of the motions of forty-eight double-stars. (By invitation.)

GUSTAV STRÖMBERG, Mt. Wilson Solar Observatory, Carnegie Institution: A determination of the solar motion and of stream motion based on absolute magnitudes. (Read by Professor Hale.)

ARMIN O. LEUSCHNER, University of California: On finite velocity of gravitation as a possible factor in stellar evolution.

ALFRED G. MAYER, Marine Laboratory, Carnegie Institution: The coral reefs of Tutuila, Samoa.

WILLIAM M. DAVIS, Harvard University: The subsidence of volcanic islands.

WILLIAM M. DAVIS, Harvard University: A duty of The International Association of Academies.

WILLIAM H. HOLMES, U. S. National Museum: The work of the Anthropology Committee of the National Research Council.

EDWARD L. THORNDIKE, Columbia University: The work of the Psychological Committee of the National Research Council.

GEORGE E. HALE, Mt. Wilson Solar Observatory, Carnegie Institution: The work of the National Research Council.

LOUIS V. PIRSSON, Yale University.* Biographical memoir of James D. Dana.

WILLIAM J. HUMPHREYS, U. S. Weather Bureau:* (Introduced by A. L. Day.) Biographical memoir of Cleveland Abbe.

E. H. HALL, Harvard University:* Biographical Memoir of Benjamin Osgood Peirce.

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